Answer to: Editor decision: Publish subject to technical corrections (12 Sep 2025)

16. Oct 2025

Dear Caroline.

Thank you so much again for carefully checking the manuscript. We addressed all points. Additionally, we found a technical error in Table B4 which is also corrected now. All links to Zenodo Repositories are final now.

Wish you a great day!

Andreas

12 Sep 2025
Editor decision: Publish subject to technical corrections
by Caroline Clason
Public justification (visible to the public if the article is accepted and published):
Dear Andreas and co-authors,

Thank you for your responses to feedback and revision to your manuscript. I am happy to accept your paper for publication after the following technical corrections have been made (page numbers refer to the document with tracked changes):

- Lines 59-60: "...climate change, and also as a baseline for..."

- Line 209: "...Alps-wide glacier volume of 283..."

- Lines 267-269: The meaning of these two sentences could be clearer. By ice mass (line 268) do you mean thickness, following on from the preceeding sentences?

Line 277: "...modelling, as we have already seen..."

Line 351: "Here we chose a rate factor for ice that is five times more rigid (A x 0.2) and likely represents..."

Line 464: "...for the pre-industrial LIA." perhaps clearer?

Line 469: "... as an attribute..."

Line 525: "...Angles of incidence are defined..."

- Update the text in red on page 24 and ensure text is black. Also ensure the final Zenodo link is provided on page 31.

- The caption for Table B4 is incomplete ("The regression for median sun incidence for the Alps-wide case is done ……………." - text needs updated).

Best wishes and thank you for your contribution to The Cryosphere,

Caroline

Final Author comments

Answer to:

Anonymous Referee #1 (4 Jul 2025)

General:

We are very grateful for these constructive comments. We agree to make the following improvements as suggested.

Comments and intended improvements from the minor comments

1. *“Section 3.1 could more clearly present the comparison between the modelled glacier geometries and the empirical outlines from Reinthaler & Paul (2025) … “*

As suggested, we will summarise and expand upon the findings on regional volume and overall thickness in Section 3.1. Section 3.1 should also demonstrate the model's success in reproducing the (physics-based) surfaces more clearly. However, the level of agreement in area should not be analysed. As we are using the outlines as targets and constraints, this would introduce circularity. Figure 6 requires further explanation and leads to an extended discussion. Also on the limitations of the modelling (in the subsequent section 4.2). Therefore, it is planned to leave it in the discussion section.

2. *“Some key findings in the Results section could be more clearly emphasised.”*

Key findings will be better presented in topic sentences and conclusion sentences of paragraphs.

3. *“The broader significance of the ELA results should be more clearly articulated.”*

We will add a small paragraph on broader significance, that is an excellent remark.

The other comments (per line) will also be edited. Except for the comments at L 88, which will be adjusted according to referee review #2.

Anonymous Referee #1

nominated 05 Jun 2025, accepted 16 Jun 2025, report 14 Jul 2025[**Report #1**]

Summary: This study presents a novel, high-resolution, physics-based reconstruction of Little Ice Age glacier geometry across the European Alps using the Instructed Glacier Model (IGM). The manuscript makes an important contribution to cryospheric science by combining empirical glacier outlines with ice-dynamical modelling to generate detailed glacier geometries and equilibrium line altitudes (ELAs) for over 4000 glaciers. The results are carefully analysed, including a thorough sensitivity study and evaluation of climatic and topographic controls on ELAs. The manuscript is well structured and clearly written, and the authors are commended for making their code and data publicly available. However, while the technical execution is strong, the manuscript would benefit from some minor refinements to the presentation of results, clarification of model-data comparison, and minor editorial corrections. Please see my comments below.

Minor Comments

1. Section 3.1 could more clearly present the comparison between the modelled glacier geometries and the empirical outlines from Reinthaler & Paul (2025), especially since those outlines serve as the modelling target. Currently, this comparison is deferred to the Discussion (Section 4.1), but it would strengthen the Results if quantitative agreement in area, volume, or regional thickness (as later shown in Figure 6) were briefly summarised here. Doing so would help validate the model's success in reproducing known LIA extents and clarify that the model does more than just "fill in" outlines—it provides a physics-based reconstruction of the ice surface and flow geometry.

2. Some key findings in the Results section could be more clearly emphasised.

The manuscript presents a large and detailed dataset, but in places the main findings are difficult to extract due to the density of technical information. I recommend that the authors more clearly highlight the major conclusions of each results subsection—particularly regarding the Alps-wide glacier volume estimate (283 ± 42 km^3), the robustness of ELAs to model sensitivity, and the spatial ELA patterns—in order to help readers better understand the scientific significance of the outputs. This could be done through clearer topic sentences or brief summary statements at the end of key paragraphs.

3. The broader significance of the ELA results should be more clearly articulated.

The paper does a good job comparing modelled ELAs to standard methods (AAR, AABR, THAR), but it stops short of fully discussing why the spatial patterns of ELAs matter. I recommend briefly elaborating on how these results could inform reconstructions of past climate gradients, lapse rates, or mountain-scale variability in LIA conditions. This doesn't need to be lengthy—just a paragraph that connects the modelling output to larger questions in palaeoclimatology would help underscore the value of the ELA dataset.

- **Line 45:** "enables to model" → "enables modelling"
- **Line 88:** "a Arrhenius factor" → "an Arrhenius factor"
- **Line 223:** "asses" → "assess"
- **Line 356:** Expand discussion on the implications of the steady-state assumption, particularly in light of asynchronous glacier responses across the LIA.
- **Line 298–310:** The solar incidence angle correction is innovative. A brief comment on how this compares in performance to full shortwave radiation models (e.g., RMSE, spatial correlation) would be valuable.
- **Line 311:** The text in line 311 is not formatted correctly (this might just be in the pdf version).
- **Line 364–368:** The handling of debris-covered glaciers is acknowledged. Consider adding brief mention of how future versions of the model could incorporate debris or avalanche input schemes.
- **Section 4.5:** The term "outliers" is appropriate statistically, but given the physical processes discussed (e.g., debris cover, avalanche input), consider reframing some as "physically atypical" rather than model anomalies.
- **Appendix C:** The solar incidence angle correction is innovative—consider briefly stating in the main text how it compares (quantitatively or qualitatively) with full shortwave radiation modelling.
- **Figure 2:** The grey text should be made more readable.

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Answer to:

Julien Seguinot, Referee #2 (13 Aug 2025)

General:

Dear Julien, we greatly thank you for your detailed review. We see all comments here will improve our script and are happy that you took the time to read and comment so carefully.

Response to general comments

"Specifically, I would recommend to publish the gridded ice topography (or thickness) maps alongside "glacier-wise values" (l. 468) mentioned in the data availability statement."

As planned and recommended here, we will publish the gridded ice topography (geotiff) and thickness (geotiff) as well as glacier-wise values (shapefile with attributes).

"Please consider this an optional improvement, but I wonder: is there a glacier size (or elevation range) where parametric paleo-ELA approximations break? It is a common assumption in such reconstructions that they work better on small glaciers, which were arguably closer to equilibrium, and less prone to complex ice-flow feedback processes or surges. The discussion touches upon this topic (e.g. l. 395, Fig. 10) but I wonder if a threshold can be derived from your data."

As discussed in 4.6, especially glaciers with high relief, tend to not fit well with the paleo-ELA approximations. We did not see a certain size or elevation threshold, rather than it depends on local topography. Also small glaciers with high relief and possible high influence from steep rock faces are affected by diverging ELA values when comparing the different methods. We agree to expand this section slightly to better explain the differences. However, we do not find certain glacier sizes or elevation ranges to be critical (eventually stated in the revised version).

Response to specific comments

"l. 89 (and others) "a Arrhenius factor": this term is misleading as it implies an Arrhenius equation, i.e. an exponential dependency (of ice softness in this case) on temperature,..."

We agree with the confusing "Arrhenius" term and will check for another terminology like "rate factor", and say what it means (ice softness, creep parameter) as suggested. The reviewer is right, that in a single simulation we did not include a dependency on temperature (isothermal). With changing the rate factor (ice softness resp. creep parameter) we change the flow properties of the ice in the sensitivity simulations, which could also be caused by the temperature.

"Fig. 7: Could it be worth adding regression lines? Parametric paleo-ELA studies could perhaps use them to estimate possible temperature and precipitation changes from ELA changes."

Even though the trends are significant most data is quite scattered. However, we can add a supplementary table with the all the regression details, R^2, p-values for each region and for the whole Alps for the use for other (paleo-)ELA studies.

“Fig. 8: I really liked the concept of midday-midsummer sun incidence, but had to draw a little sketch, and remember that incidence is measured relative to the normal of the mountain slope and not the slope itself, in order to understand the figures. I think an explanatory cartoon would fit well on Fig. 8, depicting a mountain with south and north-facing slopes, incidence rays and slope-normal vectors, or something similar.

Fig. 8 caption "by definition have an angle of 30°": Does this imply that the latitude of the Alps is assumed exactly 30° above the tropic? It seems like a valid approximation, but would need to be spelled out somewhere.”

Good point, we will find a place in Fig. 8 and include a small sketch.

The 30° incident angle comes from the fact the summer midday sun stands about 60° above the horizon at a latitude of the Alps (around 40-45° North). Incident angles are defined to the surface normal, which gives 90°-60°=30°. We will add this information.

“l. 303 "the median sun incidence angle": is it truly the median (or the midday-median)? Could you please explain the term? Or stick to midday-midsummer / misummer noon incidence angle.”

Median is related to “area-averaged” – like the median the glacier surface. We will make this clearer. Thanks for pointing out!

“Fig. 9 caption "lower but insignificant ELAs": should this be "lower ELAs but insignificantly so" or maybe "insignificant ELA lowering"? "Insignificant ELAs" would imply ELAs close to sea-level.”

We check and make this clear. We meant insignificant ELA change, so lower ELAs but insignificantly so.

Response to technical comments

All technical comments will be addressed.

Dear authors, dear editor,

Summary

The Little Ice Age period in the Alps has long captured scientific interest, and it is well documented that its glaciers were significantly more extensive than in recent decades, albeit confined to the valley heads. This has posed a challenge to both ice-sheet (planar flow) models, more suitable to study continuous ice expanses, and glacier (flowline) models, that rely on present-day glacier inventories for future projections.

The manuscript by Henz et al. on "Alps-wide high-resolution 3D modelling reconstruction of glacier geometry and climatic conditions for the Little Ice Age" use a recent inventory of LIA glacier extents in the Alps and the Instructed Glacier Model to fill this gap. The main results are a detailed reconstruction of glacier geometries for that period, and a dataset of properties for 4094 individual glaciers. This is supplemented by a detailed analysis of equilibrium line altitude (ELA) patterns and comparison to independent atmospheric modelling output.

Significantly, the authors choose to parameterize mass balance as a piecewise-linear function of elevation rather than using a positive degree-day or an energy-balance model. This approximation is well justified as it sets the paper independent from paleoclimate uncertainties and opens the door to comparison with parametric approaches to paleo-ELA reconstruction based on reduced ice-flow physics, of which the introduction provide a much-needed review. I found the manuscript to be excellently written and illustrated and virtually publication-ready.

General comments

- It is not yet clear which data the authors plan to make available at publication. Specifically, I would recommend to publish the gridded ice topography (or thickness) maps alongside "glacier-wise values" (l. 468) mentioned in the data availability statement. I thing that both products have tremendous value for derivative studies and outreach, but the latter is a much reduced set of the study's output.
- Please consider this an optional improvement, but I wonder: is there a glacier size (or elevation range) where parametric paleo-ELA approximations break? It is a common assumption in such reconstructions that they work better on small glaciers, which were arguably closer to equilibrium, and less prone to complex ice-flow feedback processes or surges. The discussion touches upon this topic (e.g. l. 395, Fig. 10) but I wonder if a threshold can be derived from your data.

Specific comments

- l. 89 (and others) "a Arrhenius factor": this term is misleading as it implies an Arrhenius equation, i.e. an exponential dependency (of ice softness in this case) on temperature, but the same sentence states the model is isothermal meaning no Arrhenius equation is needed. Later in the sensitivity analysis the term "Arrhenius factor" is used to refer to ice softness changes independently of temperature. I would suggest to call this term "ice softness factor" or "creep parameter" (two terms used in Cuffey and Paterson's Physics of Glaciers). The creep parameter may be modelled with an Arrhenius equation (Physics of Glaciers chapter 3.4.5) but in this case it seems to be constant.
- Fig. 7: Could it be worth adding regression lines? Parametric paleo-ELA studies could perhaps use them to estimate possible temperature and precipitation changes from ELA changes.
- Fig. 8: I really liked the concept of midday-midsummer sun incidence, but had to draw a little sketch, and remember that incidence is measured relative to the normal of the mountain slope and not the slope itself, in order to understand the figures. I think an explanatory cartoon would fit well on Fig. 8, depicting a mountain with south and north-facing slopes, incidence rays and slope-normal vectors, or something similar.
- Fig. 8 caption "by definition have an angle of 30°": Does this imply that the latitude of the Alps is assumed exactly 30° above the tropic? It seems like a valid approximation, but would need to be spelled out somewhere.
- l. 303 "the median sun incidence angle": is it truly the median (or the midday-median)? Could you please explain the term? Or stick to midday-midsummer / misummer noon incidence angle.
- Fig. 9 caption "lower but insignificant ELAs": should this be "lower ELAs but insignificantly so" or maybe "insignificant ELA lowering"? "Insignificant ELAs" would imply ELAs close to sea-level.

Technical comments

- l. 47 "high-order ice flow equations": I would recommend the term "higher-order", as in higher than the zero order of the shallow ice approximation.
- l. 50 "addtion": addition
- l. 86 "shallow-ice approximation (Imhof et al., 2019)": This study uses a hybrid SIA-SSA approach, and thus already incorporates some higher-order terms (the longitudinal gradients) via the shallow-ice approximation, albeit in a heuristic way.
- l. 129 "UTM zone 32N": maybe spell out UTM as other abbreviations.

- l. 319 "mountain massive": mountain massif
- l. 442 "based on the sun ray incidence angle": I find it worth explaining again "at midsummer and midday" here.

Apologies for the late review and good luck with the final stages of publication.