# Comment on egusphere-2025-3870 (referee #2)

*We would like to thank reviewer 2 kindly for taking the time to read our manuscript and for the constructive feedback, which will help us to improve the study. In the following, we will address all comments point by point and suggest respective corrections. Please find our answers in blue italic font.*

**General Assessment**

Overall, the manuscript is well-written and the sections are clearly defined. The topic is relevant and timely, given the increasing computational capacity that now allows high-resolution simulations of mountain glaciers. The approach is interesting, and the study addresses an important question: how spatial resolution affects the simulation of ice thickness, flow, and thermal regimes in mountainous regions.

However, while the manuscript is organized into clear sections, some aspects of the results and discussion are difficult to follow, partly due to reliance on references to supplementary material. Certain patterns and the interpretation of figures could be presented more cohesively in the main text.

Besides, there are aspects that require clarification or more explicit justification. While the authors explore a wide range of resolutions (50 m to 2 km), the study relies on a single model (the Instructed Glacier Model, IGM). It is not clear whether generalizations about the impact of resolution on mountain glaciers are fully warranted based on this single framework. Explicit discussion of model limitations and how the resolution interacts with the underlying Blatter-Pattyn approximation, compared to other approaches (full Stokes or others), would strengthen the study.

*We thank the reviewer for the general assessment. We fully agree that the manuscript should be clearer, shorter and not all references to the supplementary material are needed. Additionally, we believe that slightly more background information on IGM, its functionality and limitations will help to put our results into perspective.*

*We would like to clarify that the IGM framework builds on the Blatter-Pattyn model which cannot be replaced by other ice physics approximations or a full Stokes implementation. Due to substantial higher costs of higher-order models, it is computationally too expensive to run our experiments with a numerical, standard and CPU-based Blatter-Pattyn or Full Stokes model (e.g., Elmer Ice). Therefore, it would be impossible to compare our experiments to corresponding simulations using a full Stokes model. To justify using IGM that depends on the Blatter-Pattyn model rather than a full Stokes model, we refer to Pattyn et al. (2008) who showed that the Blatter-Pattyn model generally reproduces ice velocities that agree well with full Stokes models.*

*We believe that a comparison between the Blatter-Patty model and other approaches goes beyond the scope of the study, which is focused on the effect of the spatial resolution rather than the simplification of Blatter-Pattyn vs. Stokes. Instead, we believe that a more extensive comparison to other studies on resolution effects in Greenland and Antarctica (Williams et al., 2025; Rückamp et al., 2022; Aschwanden et al., 2016; Cuzzone et al., 2019) will help to generalize our findings (see other comments below and the review by referee 1 for further details).*

*For a detailed description of IGM and comparisons to an analytical solver, we would like to refer to Jouvet and Cordonnier (2023) and point out that the retraining frequency is the most critical parameter that ensures that the ice flow does not deviate too much from the Blatter-Pattyn model. In our experiments, the emulator is retrained every 0.02-0.28 (model) years, i.e., every 1-2 weeks. Moreover, our setup is based on Leger et al. (2025) who compared an IGM simulation of the European Alps at 2 km resolution to the respective simulation using the Parallel Ice Sheet Model (PISM; Winkelmann et al., 2011) published by Jouvet et al. (2023) and found minor deviations. We believe that adding this information will help to address inaccuracies of IGM.*

**Introduction**

- The introduction's opening paragraph reads as a series of descriptive statements rather than a connected narrative. Consider revising for smoother flow.

*We agree, the importance of ice models to model present-day climate change as well as paleo-reconstructions will be presented with a smoother flow in that paragraph.*

- The knowledge gap and justification for the study could be clarified more explicitly. For example, are there other studies on the effect of the spatial resolution using similar or different approaches or applied to a different setting?

*Thank you for this raising this important point. The impact of resolution has been studied for the Greenland and Antarctic ice-sheets (Williams et al., 2025; Rückamp et al., 2020; Cuzzone et al., 2019; Aschwanden et al., 2016). The analysis by Williams et al. (2025) is based on the Wavelet-based Adaptive-grid Vertically-integrated Ice-sheet model (WAVI), therefore using ice velocity not in three dimensions (like IGM). Results from Rückamp et al,( 2020) are based on ISMIP6 (Ice-Sheet Model Intercomparison Project) experiments, i.e., using models of various complexities. Cuzzone et al. (2019) uses the Ice Sheet System Model (ISSM) at different resolutions based on the Blatter Pattyn model. Aschwanden et al. (2016) compares simulations of Greenland outlet glaciers using the Parallel Ice Sheet model (PISM) that combines the Shallow Ice Approximation (SIA) and the Shallow Shelf Approximation (SSA) at different resolutions. We believe that despite different approaches and although ice dynamics are different for ice-sheets than for mountain glaciers and ice fields, these studies are useful to put our analysis into context. To our knowledge, there exists no systematic resolution analysis for ice fields and mountain glaciers, which emphasizes the importance of our study.*

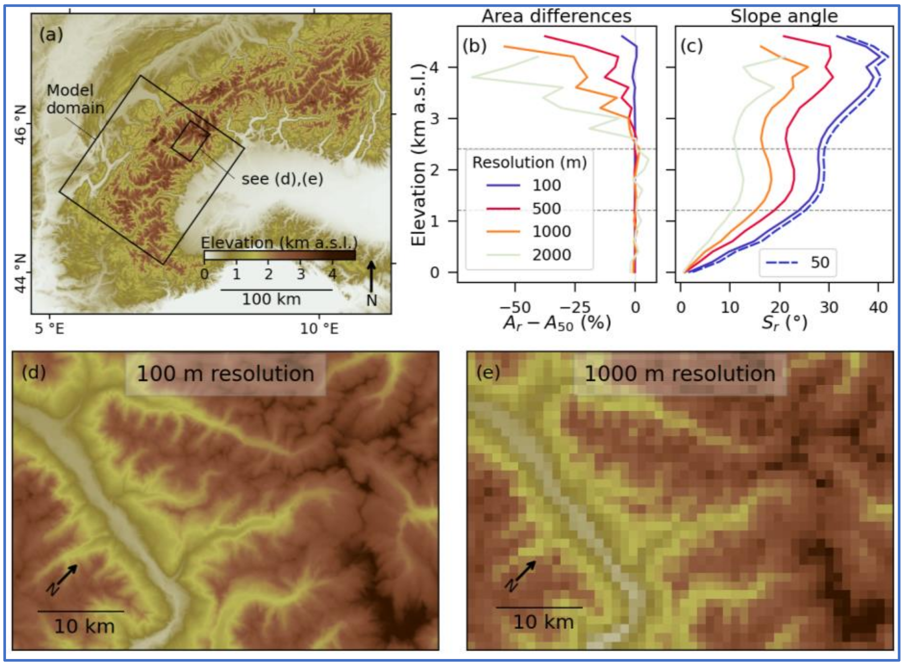
**Methodology**

- The choice of model parameters requires more justification. For example:
  - The retraining frequency of the neural network is mentioned, but no explanation about the selection of the values is provided. Explaining why this frequency was chosen would help readers understand its impact.

*Thank you for pointing out the unclarity regarding IGM model choices. All our parameter choices are based on Leger et al. (2025), except for the higher retraining frequency for the finest-resolution simulations. With IGM's is physics-informed strategy, a higher retraining frequency permits to maintain fidelity level of the Blatter-Pattyn model. The retraining frequencies of 2 and 7 used in our experiments refer to retraining every 0.02-0.28 years (given the typical adaptive time step of 0.01-0.04 years). This means that the emulator is retrained based on the Blatter-Pattyn model multiple times a year, reducing deviations. For justifications of the retraining frequencies we refer to Jouvet and Cordonnier (2023) and Leger et al. (2025). We will add this information and references on the retraining parameters to the revised manuscript. Additionally, we will make sure that it is clear which model parameters are based on Leger et al. (2025) and which are defined by us.*

  - Elevation bands for low, mid, and high altitudes are defined as 0–1200 m, 1200–2400 m, and >2400 m. A brief justification or reference would improve clarity.

*Thank you for raising this point. We would like to clarify that the elevation bands are used to roughly distinguish between low, mid, and high altitudes across the Alp's hypsometry and elevation range. We use these elevation bands to distinguish between topographic features that are typical for these altitudes (e.g., deeply incised valleys at low altitudes). We propose to add the elevation ranges to panels b and c of Fig. 1 to visualize that the elevation bands distinguish between gentle-sloping regions at 0-1200 m a.s.l., areas with intermediate slopes at 1200-2400 m a.s.l., and steep regions above 2400 m a.s.l., e.g.,:*




- While the study explores a wide range of resolutions, all resampling is performed using cubic interpolation. Would the authors expect differences by using other techniques (bilinear, kriging…)? Could this lead to some misleading results?

*We will add a justification for the cubic resampling method in the revised manuscript, e.g.: "For all DEM resampling of input fields, we use cubic convolution due to its high accuracy in resolving topographic features such as slope angles compared with other analytical methods like bilinear or kriging resampling (Mihn et al., 2024)." Consequently, we expect slightly higher differences between fine and coarse resolution DEMs using other resampling methods, which might lead to even stronger resolution effects. However, we consider differences due to resampling methods to be minor compared to the overall resolution effects analyzed.*

*Please note that we used cubic resampling for the preparation of the input DEMs, while for post-processing comparisons of our model outputs at different resolutions, we instead used nearest neighbour resampling to preserve the pixel structure at coarse resolutions (lines 171-172).*

**Results**

- Some aspects of the results are difficult to interpret solely from the figures, particularly given the reliance on supplementary material. Consider highlighting the key patterns in the main text to improve readability.

*We fully agree that the readability of the results section should be improved. To that end (and in lines with comments from reviewer 1), we propose to reduce the number of figures (removing Figs. 6 and 9) and generally shorten the paragraphs to highlight main aspects. We believe that the key patterns are visible in the figures already provided in the main text and references to the supplementary material will be reduced substantially.*

- While the manuscript identifies three resolution “modes”, the evidence is not entirely clear except for Figure 8 and not for all the outputs and elevation bands.

*Thank you for the comment. In line with the comments from reviewer 1, we agree to remove the term “modes” and focus on the comparison to the 50 m reference run. We would like to emphasize resolution effects at fine and coarse resolutions (e.g., generally thinner and faster-flowing ice at fine resolutions), instead of mainly focusing on strong changes at intermediate resolutions.*

*However, we argue that there is more evidence for the three resolution “modes” than Figure 8, e.g., the maximum ice volume can be divided into rather consistently low ice volume at fine resolutions and consistently high ice volume at coarse resolutions, with high increase in ice volume at intermediate resolutions (Fig. 3). Moreover, the ice thickness differences are small at fine resolutions and thickness differences strongly increase with resolution at intermediate resolutions (Fig. 5).*

**Discussion**

- The discussion is generally relevant but could be more explicitly connected to practical implications. For example, explaining how these findings could guide modelers in selecting appropriate spatial resolutions would increase the impact (other than just going for finer resolution as a general rule).

*Thank you for this comment. As mentioned above, we think it best to shift the focus from the “critical resolution mode” to a comparison to the 50 m reference run, which shows that model results at 300 m and finer are generally comparable to the reference. Therefore, we can conclude that a resolution of 300 m is sufficiently fine to accurately simulate glacier dynamics in our exemplary ice field in the Western Alps. Removing the confusing “resolution modes” and instead identifying a “sufficiently fine resolution” will already be more useful from a practical perspective. Our paper might therefore provide a valuable reference for determining a resolution for a mountain region similarly steep to the Western Alps.*

*Moreover, the experiments using a smoothed topography show similar resolution-effects that depend on altitude and are non-linear. From those experiments, we can conclude that a resolution of 400-500 m is sufficiently fine to accurately model the icefield in smoother terrain than the Western Alps. A discussion of different resolutions for ice-sheets (Greenland, Antarctica) in context of our experiments for the Western Alps will provide further guidance for modellers.*

*Because we find that input and output variables vary differently to resolution, it is very difficult to give specific recommendations for spatial resolution based on any DEM. However, we believe that a recommendation for a coarse limit of resolution can be based on an inherent wavelength of the landscape. For example, if the spatial resolution is coarser than the characteristic scale of the valleys, it is definitely too coarse. In our study, we do not consider this case because the* *coarsest resolution of 2 km is fine enough to capture the main valley system. However, at even coarser resolution of, e.g., 5 or 10 km, more topographic characteristics would be lost, leading to even stronger resolution effects.*

- I feel that maybe more detailed exploration of this 400-800 "critical" mode should be addressed before to clearly talk about strong sensibility.

*We agree and as pointed out above, we will remove the "resolution modes". Strong changes at intermediate resolutions will be described more explicitly instead, which will add clarity to the results and discussion. Since we would like to focus more on differences compared to the 50 m reference run, we believe it to be more valuable to contrast ice field characteristics at fine (generally thinner glaciers and faster ice flow) and coarse resolutions – instead of focusing on intermediate resolutions.*

*We would like to add that the strong sensitivity at intermediate resolutions is visible in Figs. 8, 5, and 3, where generally small changes in resolution lead to quite high differences in output variables. To give an example for the higher sensitivity, the maximum ice volume from the 400 m run increases by over 25 % when using a 500 m resolution. For comparison, the difference of maximum ice volume is less than 1 % between the 50 and 100 m runs and ~1.5 % between the 1800 and 2000 m runs, which indicates that the intermediate resolutions are more sensitive to small changes in spatial resolutions.*

**Conclusions**

- I like the bullet structure, but I think that some points could be rearranged for more clarity.

*Thank you for this comment. We agree that the conclusion points could be clearer. By integrating previous comments and removing the phrase "resolution modes" from the manuscript, we expect to enhance clarity. Additionally, the points on mid and low altitudes can be combined and focused on resolution effects in the valleys (which is also true for the corresponding discussion paragraphs in section 4.1).*

*We admit that it might be confusing to start the conclusions with point (i) by contrasting ice field characteristics at fine and coarse resolutions, followed by a more detailed description of resolution effects at different bedrock altitudes later in points (iii)-(v). We decided on this structure because we ordered the points based on importance. However, we are happy to rearrange the points by, e.g., putting points (i) and (iii)-(v) closer together.*

**Figures and Supplementary Material**

- While the figures themselves are informative, the main text often relies on references to supplementary material to interpret the results. This can make the narrative difficult to follow. Consider ensuring that the key patterns and findings are clearly described within the main text, using the supplementary figures as support rather than as primary evidence.

*Thank you for the suggestion. The references to supplementary figures are mainly additional information. We are confident that we are able to present our results in a more concise way in the revised manuscript without excessive reference to supplementary figures for details that we won't show in the main text.*

**Minor Comments**

Here I will try to skip the comments raised from the other reviewer so we don't need to repeat the same things.

**L108:** Please, justify this choice of (0.01-0.04).

*The time step in IGM is adaptive and not static, i.e., it is not a parameter chosen by the user but instead calculated in IGM based on the CFL condition that ensures numerical stability for the ice thickness evolution. We propose to remove the values for a typical time step and retraining time step frequencies from the manuscript and instead give the retraining frequencies in years: For adjustments to new ice field states attained through run time, we retrained the physics-informed neural network multiple times within a single model year (every ~0.18 years). The on-the-run retraining in the highest-resolution simulations at 50 and 100 m was even performed more often (every ~0.05 years)."*

**L178:** Please, justify the selection of the elevation bands.

*As mentioned above, we agree to add an explanation for the choice of elevation bands, e.g.: "We used elevation bands of 0–1200, 1200–2400, and >2400 m a.s.l. to distinguish between low, mid, and high altitudes across the Alp's elevation range (Fig. 1a) that feature deeply incised, gently-sloping valleys at low altitudes and steep mountain summits at high altitudes." We propose to add the elevation ranges to panels b and c of Fig. 1 to visualize that the elevation bands distinguish between gentle-sloping regions at 0-1200 m a.s.l., areas with intermediate slopes at 1200-2400 m a.s.l., and steep regions above 2400 m a.s.l. (see updated Fig. 1 above).*

**L184:** effects -> affects?

*Yes, we will adopt this in the revised manuscript.*

**L209:** This 10 times more is not clear to me, at least from the cited figures.

*Thank you for highlighting the need for clarification. Due to the y-axis, it is not really visible in the figure that the 2000 m simulation has 10 times more ice volume than the 50 m run at 1000 years of model time. However, it is clear in the figure that at 1000 years, the coarse resolutions have (relatively speaking) much more ice volume than the fine resolutions. In numbers, the 50 m* *run has ~12 $km^2$ ice volume at 1000 years, while the 2 km run has ~160 $km^2$ ice volume at this time step. These numbers will be included in the text.*

**L254:** It is not clear to me looking at Fig. S12.

*Thank you for pointing this out. We apologize for the typo and refer to Fig. S3 that shows the spatial distribution of ice thickness at full glaciation.*

**L426-7:** I really don't see those greatest relative changes at high altitudes. Probably you have the numbers, but looking at Fig. 8f such a thing is not clear to me.

*Thank you for the comment. As mentioned above, the analysis of the Arrhenius factor does not add many insights, and we would like to remove this from our analysis to focus more on key variables (thickness, velocity, temperature). We would like to add that the mean Arrhenius factor at high altitudes is ~66 M $Pa^{-3}$ $yr^{-1}$ for the 50 m run and ~112 $Pa^{-3}$ $yr^{-1}$ for the 2 km run, leading to a difference of almost 60 % relative to the 50 m run.*

**L429-31:** Again, this is not obvious to me just looking at Fig. 8.

*Similar to the previous comment, we propose to remove the Arrhenius factor parts from the manuscript.*

**L474-8:** Nothing to object here, the opposite, really clear statement and good point resulting from your work.

*Thank you very much.*

**L500:** Here you suggest to go to finer than km- and ideally 500-m scale- but your defined critical mode ranges from 400 to 800m. I found this a little bit contradictory.

*Thank you for spotting this inconsistency, this should be changed to "400-m-scale".*

**Overall Recommendation**

Even if this is a relevant and well-executed study that addresses an important question in glacier modelling, especially in regional or global studies, I consider the manuscript as major revisions after considering some points:

1. Clarifying model limitations and generalization potential.
2. Justifying some methodological choices, such as elevation bands, retraining frequency, and resampling method.
3. Streamlining results and discussion to highlight the most important findings and improve readability. Exploring a bit more this critical mode sensibility.
4. Enhancing figure clarity and reducing overreliance on supplementary material.

I believe that addressing these points will help the resulting paper become a solid and valuable contribution to the field.

*Thanks for the valuable suggestions, which we hope we have addressed in the above mentioned planned modifications.*

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