

## Author's response to RC1

*Text in black: Reviewer's comments*

*Text in blue: Author's response*

### Summary

This paper details a set of related modelling developments that allow CISM to be applied to regional studies of mountain glaciers, rather than its native ice-sheet domains. The authors develop a protocol to allow the glaciers to be initialised in a manner similar to how an ice sheet would be, and also solve the computational- time problem by having ice-free blocks of the model domain ignored during simulations (these blocks update dynamically if glaciers advance/retreat). CISM is then applied to the European Alps using the full suite of GlacierMIP3 experiments to demonstrate the effectiveness of this new approach, with results showing that it performs in line with expectations based on previous studies.

I think this paper is well-written and put-together. I would particularly like to congratulate the authors on their concluding sections on model limitations and future work, which addressed several queries I'd had at the back of my mind while reading the rest of the manuscript, and allowed me to write a much shorter review! The technical advance presented in the paper is novel, particularly the ability to ignore ice-free blocks of the model domain, and the possibility it raises of having a unified ice-sheet-glacier representation in the same model is, to my mind, the major advantage it presents (whilst this is technically possible in other models, it is not often done, certainly not in a sustainable way). It is also clear that the model functions well.

My comments below are largely fairly minor and most should be able to be addressed with a line or two of extra explanation. There is some amount of restructuring, though, that I think would improve the paper, but, overall, I think this fits within the broader category of minor revisions.

We thank the reviewer for their very constructive and encouraging feedback. We have incorporated their suggestions and provided detailed responses below.

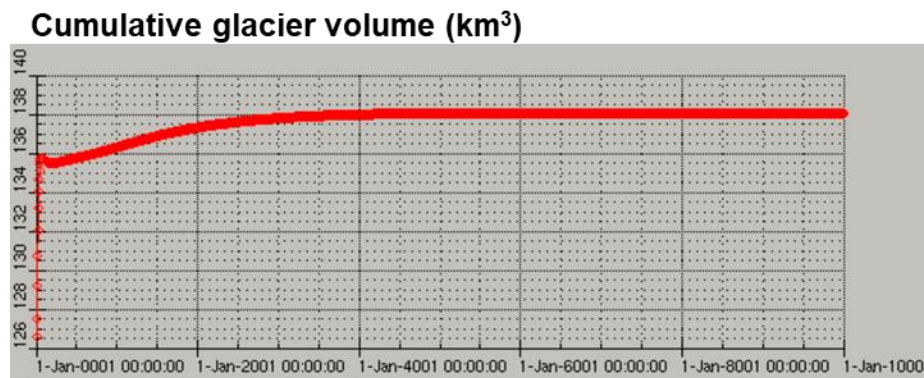
Page and line numbers refer to those in the clean version of the submitted manuscript.

### Major Comments

- Spin-up: This is not a problem *per se*, but I think the 10,000-year spin-up for the glaciers seems a little excessive. Clearly, too much spin-up is far better than too little, but the paper does not justify why such a long period is necessary for such small ice bodies. Especially given the authors note in Section 4.6 that the glaciers are close to equilibration with the warmer 2010 temperature conditions by 2184 (so, 174 years). Given the authors are not starting from ice-free topography or similar, but with an initial glacier that is not all that far from the stable 1984 state they are aiming for, I can't believe that it takes 10,000 years to sort itself out. I suspect the authors could drop an order of magnitude and have 800 years of inversion for C\_p followed by 200 years of holding it constant without the results being materially different, and even this might be at the upper end of what I'd think is reasonable in

this setting. The main weakness of the method is that it does need a substantial amount of HPC resource to run and there seems to be a very low-hanging fruit here that could greatly improve the situation. Could the authors at least provide some commentary in the paper on why this length of spin-up is necessary or, if it isn't, note that this is the case (I'm not expecting the authors to check whether a shorter spin-up does affect the results, given the computational cost, but I would like some explanation of the reasoning here)?

During the forward runs, the glaciers adjust to the warmer climates relatively quickly (~200 years). During the model initialization, however, it takes longer for the simulated ice geometry and internal state to reach quasi-equilibrium with the climate forcing. Below, we show the total glacier volume for the Alps during the original 10,000-year spin-up (x-axis). After a large response in the first few centuries, the volume slowly equilibrates over a few thousand years.



We recognize, however, that changes are very small after the first 4,000 years, and we do not want to give readers the impression that a full 10,000-year spin-up is necessary.

For the revised submission, we ran a new spin-up of 5,000 years (4,000 years with inversion and 1,000 years without inversion). We stopped the inversion once the rate of change of total ice volume fell below 0.1 km<sup>3</sup> per 1,000 years.

We describe the modified spin-up and equilibrium criterion in Section 4.1 (*Spin-up and historical runs*) in the revised manuscript.

- Structure: The paper is generally well-structured, but I found the presentation of the method in a theoretical manner in Section 3, followed by the concrete application details in Section 4 a bit confusing. I read Section 3 expecting to find a lot of details that come up later and then had to keep cross-referencing the theory and the application, which was a bit annoying (for instance, I read Section 3.3 and was very concerned that the authors hadn't said anything about the datasets they were using, and then had to wait till Section 4.2 to find out what had actually happened, then check back to 3.3 to remind myself what the actual method was. This seems as if it could be simpler). I might recommend that the authors consider merging the two sections and restructuring such that each bit of theory is followed by how it was implemented in this study, just to make it easier to follow what's going on

Thank you for the feedback! We have revised the structure of the manuscript to merge the

two sections. The revised version combines methodological theory and application within the same sections to improve clarity and flow.

The structure for the new Sections 3 - 5 is as follows:

### 3. CISM development for glacier modeling and application to the European Alps

- 3.1 Forcing and initialization data
- 3.2 Model domain and resolution
- 3.3 Glacier identification and tracking
- 3.4 Glacier surface mass balance scheme

### 4. Model initialization and calibration

- 4.1 Spin-up and historical runs
- 4.2 Thickness inversion
- 4.3 Mass balance calibration
- 4.4 Surface velocity

### 5. GlacierMIP3 simulations

- 5.1 Committed ice loss
- 5.2 Equilibration runs

### Minor Comments

- p.4, 1.91: Could the authors expand a little here and say in which glaciated regions DIVA does not tend to match Blatter-Pattyn? It will make it easier for readers to understand where this model formulation is likely to be more/less reliable.

DIVA differs from Blatter-Pattyn (BP) for flow with large vertical shear over a bed with significant topographic variations on short spatial scales, as discussed by Goldberg (2011). A classic example is Test A in the higher-order benchmark experiments introduced by Pattyn et al. (2008). In our experiments, the flow of many glaciers is dominated by sliding. For a few large glaciers, e.g., the Aletsch in the Bernese Alps, there is significant vertical shear, but the bed topography is fairly smooth, so the differences between BP and DIVA are relatively small.

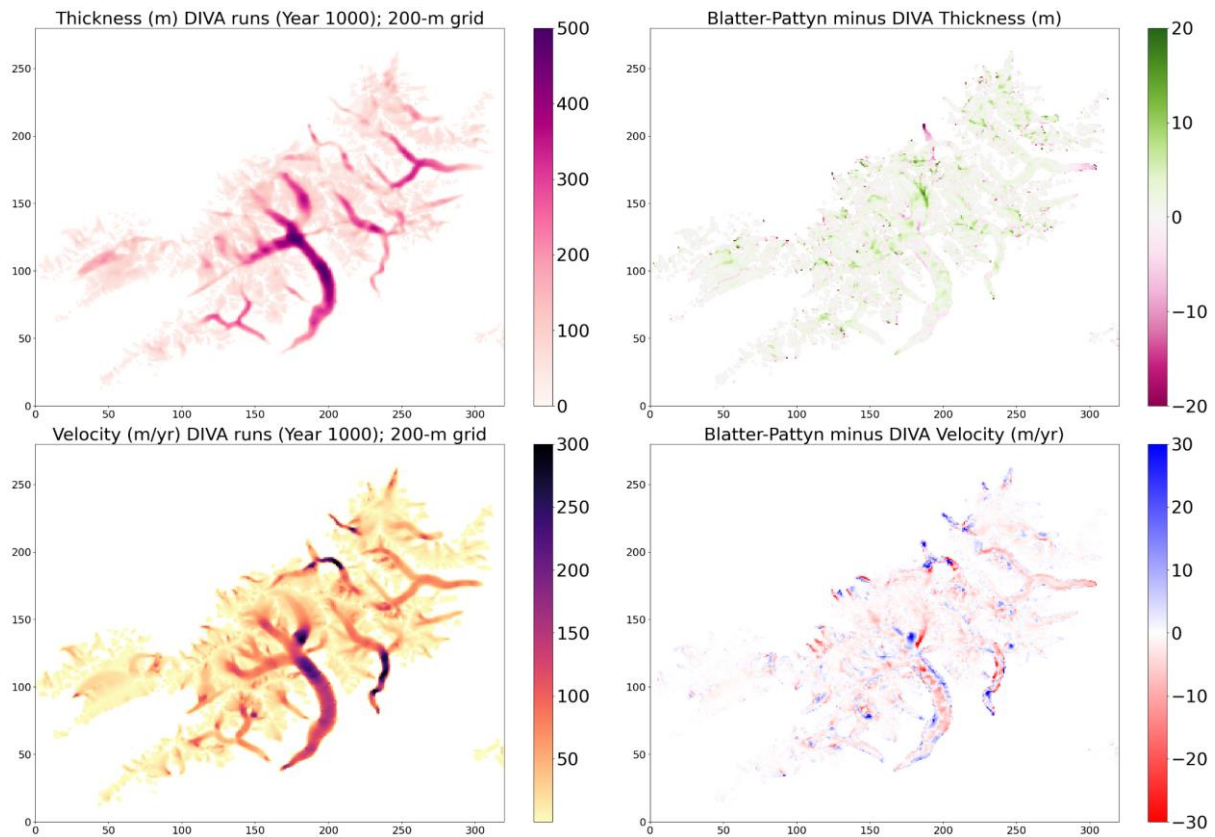
We clarified these points with new text in the revised Section 2.1:

*“The DIVA solver is computationally much faster than BP, while computing velocities similar to BP in most glaciated regions. An exception would be flow with large vertical shear over a bed with rough topography, as discussed by Goldberg (2011). In the runs below, most glaciers have relatively smooth beds and/or sliding-dominated flow with small vertical shear, for which the two solvers give similar results. DIVA also scales well to the high resolutions needed to model mountain glaciers (Robinson et al., 2022). We therefore used DIVA for the simulations in this study.”*

To illustrate this, we ran a 1,000-year BP simulation on the 200-m grid and compared it with the same DIVA set-up. Since BP is an order of magnitude more expensive than DIVA, we did not carry out a full spin-up with BP.

The figure below shows ice thickness and velocity differences for DIVA versus BP in the

Bernese Alps. The plot shows small differences compared to the mean values, especially in the thickness field.



The comparison of the two schemes for the Alps glaciers is not included in the revision as we deemed it outside the paper's scope.

- p.11, l.286: I think these spin-up times could have a 0 knocked off them and the results would not change very much at all. Out of interest, why did the authors choose such long simulations?

For the revised submission, we reduced the spin-up time to 5,000 years. It is true that the results would be fairly similar had we stopped at year 1000, but we took a conservative approach and continued the spin-up until the rate of change of total volume fell below 0.1 km<sup>3</sup> per 1000 years.

- p.14, l. 309-315: I agree that it's good to compare to the Farinotti product, but there's no particular reason to assume that it is a completely accurate reflection of reality. In fact, we know it isn't. My point is that deviations between the results presented here and the Farinotti product are not necessarily a bad thing. It would be instructive to also compare the thickness results to the products in Millan et al. (2022) and Cook et al. (2023) to see where the results here fall within the range of existing global-regional modelled Alpine thickness products, rather than just picking one and assuming it's the best representation. All three products work better in some places and worse in others, so a wider comparison might be more useful for the

community to understand how the method presented here performs and what it offers that isn't already on the table. I would also make a similar point about Section 6.1 – the differences may be more or less marked if other products are also considered, which might say something useful about the method presented here

We used the Farinotti product (F19) as the target thickness for calibration (Section 3.1), and these results (Figure 2 and Figure 10 in the revised manuscript) are discussing how well CISM reaches this target thickness.

We agree that the F19 data is far from accurate. To ensure that our model simulations are within the range of existing estimates, we compared our output with the Grab et al. thickness data (Figure 3) and with the ITS-LIVE and Millan et al. 2022 velocity profiles in Figure 5 (instead of their thickness, which was a derived product).

- p.21, l.395: OK, both resolutions produce stable runs. Are there any significant differences in the actual results? That seems a critical point that the authors should address in this section (I assume the differences were pretty minor, but it should be clearly stated here, given the section title!)

We agree that it is useful to compare results on both 100- and 200-m grids. For the revised submission, Section 3.2 (*Model domain and resolution*) and Section 6.1 (*Spatial resolution and computational efficiency*) provides further comparison and results for a spin-up and forward runs between the two grids, with the same model settings apart from resolution.

Indeed, the differences between the two runs are small. The 200-m run has a slightly larger total ice area and volume at the end of the spin-up (2530.8 km<sup>2</sup> and 137.9 km<sup>3</sup>, respectively, compared to 2497.2 km<sup>2</sup> and 137.3 km<sup>3</sup> for the 100-m run), however its sensitivity to warming is very similar.

The main advantage of the 100-m grid is that we are able to resolve more of the small glaciers. The 200-m grid has a much larger number of unresolved (subgrid) glaciers and glaciers occupying just one or two grid cells (table below).

Total glaciers in RGI: 3892		
	100-m	200-m
Unresolved	6	259
1 grid	42	782
2 grid	182	493

## References

- Cook, Samuel J., Guillaume Jouvét, Romain Millan, Antoine Rabatel, Harry Zekollari, and Inés Dussailant. “Committed Ice Loss in the European Alps Until 2050 Using a Deep-Learning-Aided 3D Ice-Flow Model With Data Assimilation.” *Geophysical Research Letters* 50, no. 23 (2023): e2023GL105029. <https://doi.org/10.1029/2023GL105029>.
- Goldberg, D. N.: A variationally derived, depth-integrated approximation to a higher-order glaciological flow model, *J. Glaciol.*, 57, 157–170, <https://doi.org/10.3189/002214311795306763>, 2011.
- Millan, Romain, Jérémie Mouginot, Antoine Rabatel, and Mathieu Morlighem. “Ice Velocity and Thickness of the World’s Glaciers.” *Nature Geoscience* 15 (February 7, 2022): 124–29. <https://doi.org/10.1038/s41561-021-00885-z>
- Pattyn, F., Perichon, L., Aschwanden, A., Breuer, B., de Smedt, B., Gagliardini, O., Gudmundsson, G. H., Hindmarsh, R. C. A., Hubbard, A., Johnson, J. V., Kleiner, T., Kononov, Y., Martin, C., Payne, A. J., Pollard, D., Price, S., Rückamp, M., Saito, F., Souček, O., Sugiyama, S., and Zwinger, T.: Benchmark experiments for higher-order and full-Stokes ice sheet models (ISMIP—HOM), *The Cryosphere*, 2, 95–108, <https://doi.org/10.5194/tc-2-95-2008>, 200