Review of Minallah et al. (2025) ‘A framework for three-dimensional dynamic modeling of mountain glaciers in the Community Ice Sheet Model (CISM v2.2)’

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.

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)?
- 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

Minor Comments

- p.4, l.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.
- 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?
- 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
- 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!)

References

Cook, Samuel J., Guillaume Jouvet, Romain Millan, Antoine Rabatel, Harry Zekollari, and Inés Dussaillant. "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.

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.