

## ***Interactive comment on “Numerical reconstructions of the flow and basal conditions of the Rhine glacier, European Central Alps, at the Last Glacial Maximum” by Denis Cohen et al.***

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This is a carefully written and beautifully illustrated manuscript that revisits LGM reconstructions of the Rhine Glacier and its catchment region. It is a well-studied glacier and region of Switzerland, which strengthens this study - Cohen et al. build on past LGM reconstructions of the Rhine Glacier, but applying a sophisticated 3D flow model in this case. There is certainly value added in this, with detailed dynamical reconstructions available from the suite of simulations presented in this paper. While the results are mostly of regional interest, the glaciological methods are state-of-the-art and are transferable to other glaciated regions, and there are some robust, general conclusions that

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may be of broader interest. In particular, the implications for LGM climate in the area are interesting, and the results also provide a provocative challenge to interpretation of trimline records in alpine regions. If the authors are correct that 100s of metres of inactive, cold-based ice may have been present, with no trace, then icefields in most of the world's mountain regions could have been much thicker than has been assumed or reported elsewhere, on the basis of trimline records.

The climate and mass balance treatments here are not as state-of-the-art as the glaciological modelling, which limits the conclusions about LGM climate. It would be interesting to explore further, e.g. in an RCM, what LGM mass balance gradients and precipitation amounts might have looked like. But that is beyond the current scope. I find this study to be thorough, careful, and well-presented, and most of my comments are included in the attached pdf: highlights for spelling/grammatical errors, and comments/requests for clarification as points arose during my read of the manuscript. I will repeat here a couple of points that I would like to see addressed in the revised text:

1. The question of disequilibrium is difficult in these simulations. I agree with the authors that there is no reason to believe the Rhine glacier complex would have been at equilibrium at LGM or at any time during the glaciation, so the trimlines and moraines rather just represent the maximum thickness (perhaps) and extent. On the other hand, it is hard to interpret the simulations, since they are essentially snapshots along a continuum of glacier/icefield evolution in the region. Numbers in the tables and the thermal and dynamical fields in the various plots are sampling five of an infinite number of potential states, depending on when the simulations were terminated, so what do they mean exactly? It would have been interesting to carry one of the 'most likely' climate/mass balance scenarios out to equilibrium, but I understand the technical constraints. Also, the thicker icefields in runs 3 to 5 may even thicken to where they start to overwhelm the upper topography and challenge the boundary conditions on the upper glacier (i.e. require a larger domain). I would be interested to read a brief discussion of this issue and how the authors interpret their results, perhaps emphasizing that these

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are five glaciologically-sensible configurations within a continuous spectrum, but that these do not bound or constrain what is likely or possible.

2. I am a bit uncertain of the sliding treatment and associated discussion. The authors will agree that this treatment, sliding that is linearly proportional to the basal shear stress, is not necessarily the way that large-scale basal flow occurs. For instance, in ice shelves or in water-lubricated environments like ice streams, basal friction and shear stress approach zero as basal flow increases. I appreciate that this is a standard treatment and in the absence of a coupled hydrological model it seems fine, but I would suggest not to over-interpret the basal sliding results. Also, I was confused in places as to the discussion and interpretation on this (p.34, l.15; p.22, l.5); how is basal shear stress calculated in the model? Is it the residual of  $\tau_d$  - lateral drag - longitudinal stress gradients in the stress balance? Then sliding is calculated from the resulting value of  $\tau_d$ , per Eq. (6)? (Iteratively). Just a few lines of clarification would help here.

3. p.25, l.27. The climate inferences are interesting. I am not sure about the argument that case 1 is too cold. Values of 4-6 degC are similar to present-day summer temperatures on mid-latitude alpine glaciers, at elevations of 2000-2500 m (e.g., Greuell and Bohm, 1988; Marshall, 2014; Ayala et al., 2015). A temperature of 0 degC does not seem unreasonable for LGM and would represent a cooling of about 5 degC (it is necessary to compare glacier environments rather than the present-day low-elevation temperatures in a non-glacial environment, for the temperature anomaly).

Related to this, the general climate conclusions are of broad interest, I suspect, and I would be interested to read what the authors believe to be most likely for the LGM climate conditions here. How can this be explored further? Is the cold-dry case possible, or can it be ruled out? What is the basis for ruling out a (south)westward source of moisture to the region, from North Atlantic storm tracks displaced to the south relative to present-day, but along the LGM polar front?

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Most of these queries are just looking for additional information and insight from the authors, who have thought about this carefully. Thanks for this interesting contribution.

Please also note the supplement to this comment:

<https://www.the-cryosphere-discuss.net/tc-2017-204/tc-2017-204-RC1-supplement.pdf>

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-204>, 2017.

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