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

We generally agree with the reviewer: our 5 simulations are snapshots along a continuum of transient states of the Rhine glacier around the LGM. Simulation 1 is close to equilibrium (net mass balance of 0.02 m a^{-1}) and the glacier extent and the ice thickness do not change significantly in the last 500 years of the simulation. Simulation 2 is still slightly out of equilibrium but the rate of ice thickness change is small. This is not the case for the other simulations (3 to 5) where the glacier mass balance is still clearly out of equilibrium. For simulations 3 to 5, we chose a configuration of the Rhine glacier whose ice extent was relatively neaer the ice extent obtained from the geomorphic reconstruction. The flow pattern in these simulations is not affected by the lateral boundaries. Had the glacier extent increased further north than in simulation s4, lateral boundaries would have influenced the numerical solutions for the velocity and the ice thickness, and the results would have indeed become unreliable.

Like both reviewers, we believe that the Rhine glacier at the LGM was not at equilibrium with the climate. The five solutions shown are meant to represent five possible maximum extents before the beginning of a climate warming that would have caused the Rhine glacier to retreat from these maximum extents. Our five simulations are part of a continuum of possible state of the Rhine glacier at the LGM. However, these simulations, with a range of ablation and accumulation mass balance gradients, bracket, to some extent, the characteristics of the ice flow for the Rhine glacier at the LGM. Significantly lower or higher mass balance gradients are very unlikely based on what is known about the LGM climate. We do not show or discuss temporal changes in geometry during the simulations. This could fill the work of another paper but we will make clearer in the manuscript that these five simulations are part of a continuum of solutions and illustrate possible states of the Rhine glacier before a climate warming turning point.

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 overinterpret 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 τ_d - lateral drag - longitudinal stress gradients in the stress balance? Then sliding is calculated from the resulting value of τ_d, per Eq. (6)? (Iteratively). Just a few lines of clarification would help here.

The reviewer is correct in that our sliding law is a simplification. A linear sliding law does not apply to places like ice streams of where ice separates from bedrock bumps and forms water cavities between ice and rock. Our sliding law, standard in many numerical models of ice flow, uses a linear sliding law as a first order approximation of how a glacier slides on its substrate. Other more complex models of sliding (e.g., Gagliardini et al., 2003) are more realistic but also would have required (to be realistic) some information about subglacial hydrology, which we lacked. Also, these more complex laws, in combination with Stokes flow, are largely untested and we thus preferred keeping the model simpler. A more complex sliding law could be used in follow up studies, keeping in mind that realistic sliding parameterization is hampered by lack of data against which to verify models. We will check our manuscript to make sure we do not over-interpret results based on sliding. Our results regarding basal temperature, a key parameter that indicates the possibility of glacial erosion, however, appear robust.

The basal shear stress, τ_b (shown in Figure 10), is calculated from Equation (6) using the computed velocity field at the bed, v_s, which is tangential to the bed. The basal drag boundary condition (Equation (6)) adds non-linearity to the Stokes equation and the velocity solution is indeed calculated iteratively.

On page 34, our use of the term 'basal friction' may be confusing. We meant there the effect of the friction between ice and rock on the thermal boundary condition at the base (Equation (8)) due to the sliding speed. We did not mean the frictional sliding law. We will rephrase this statement to make this clearer.

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 deg C 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 deg C does not seem unreasonable for LGM and would represent a cooling of about 5 deg C (it is necessary to compare glacier environments rather than the present-day low-elevation temperatures in a non-glacial environment, for the temperature anomaly).

Our paragraph on page 25 on LGM climate may need some clarifications, but we are not sure we understand the comparison of our LGM summer temperature in the glacier forefield at about 400 m a.s.l. with the present-day summer temperatures at 2000–2500 m in the Alps. The reviewer refers to an elevation range (2000–2500 m) that is roughly 800 m below today's ELA (about 3000 m a.s.l.). Similarly, the LGM glacier forefield (400 m a.s.l) is also roughly 800 m below the LGM ELA (about 1200 m a.s.l.), and this may be the analogy made by the reviewer. In that case, the LGM cooling for our simulation with 0 deg C summer temperature (simulation 1) in the glacier forefield would be between 5 and 10 deg C because present-day temperatures in the Swiss Alps (1961–1990 average) at 2000–2500 m a.s.l. are in the range of 5 to 10 deg C. The cooling could thus be significantly larger than 5 deg C. Also, one should not forget that, even though paleoclimatic reconstruction of summer LGM temperatures at 400 m a.s.l. based on vegetation (in the range of 3–7 deg C) are not much colder than they are today (5–10 deg C) at 2000–2500 m a.s.l. (in both cases 800 m below the ELA), there was less melt at the LGM because the melt season was significantly shorter due to the stronger annual temperature amplitude at the LGM. Finally, and this was our main point, a summer temperature of 0 deg C at the LGM in the foreland is significantly colder and is in conflict with vegetation reconstruction for the same time period. The reconstructed values of 3 to 7 deg C are from the literature and not our own calculations. Assessing the reliability of these values in the context of paleoclimate reconstruction is beyond the scope of this manuscript.

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?

The primary goal of our numerical simulations was to reconstruct past ice conditions rather than past climate. Our assumed climate are meant to roughly cover possible climate conditions (specifically mass balance), as input to driving the ice dynamics model, not to make statements about which climate is most likely. Yet, the conclusions presented in our paper indicate a dichotomy between model results and geomorphic reconstructions that is very likely due to climate (specifically mass balance), and that we could not resolve. We are unable to choose a most-likely climate scenario and a cold-case scenario cannot be entirely ruled out at this stage. Further modeling studies, perhaps with different spatial parameterization of mass balance gradients and different sliding laws, may help bring models and geomorphic reconstructions into better agreement. A south-west source of moisture was also not ruled out in our analysis. Indeed more moisture arrived from the south-west at the LGM than today. Under present-day conditions, humidity source is mainly from the northwest. We will check our manuscript to make sure these important points are made.

Also, climate proxies at the LGM, which are rare, may help better bracket the mean annual temperature, winter and summer temperatures, and humidity, at the LGM. Finally, dating of exposure of rocks above the reconstructed trimline should help elucidate whether the trimline was an englacial feature and bring light on the thickness of the Rhine glacier (and other glaciers in the Alps) at the LGM.

Other comments in annotated manuscript

Too high value for PDD factor

We use a PDD factor of 6 mm day^{-1} C^{-1}. This is a normal value for melting ice on Arctic glaciers. PDD factors can be a bit higher on mountain glaciers (between 6 to 8 mm day^{-1} C^{-1}), but we believe that the climate at the LGM was more similar to present-day Arctic conditions. Furthermore, using a higher PDD factor increases the mass balance gradient and makes it more difficult to model a glacier that is in agreement with glacier reconstructions based on geomorphological evidence. We will better indicate in the manuscript the reason for our particular choice of PDD factor.

Binary map for basal temperature in Figure 8

Indeed this is like a binary map and we will modify the figure to make that clearer.

Non-intuitive: increase in friction would produce increased slip

We agree that increasing friction should decrease slip but in the sliding law we are using, sliding speed increases monotonically with basal shear stress. An alternative explanation, that could also apply to more realistic sliding laws where friction plateaus with increased sliding, is that, with more frictional heating due to higher basal shear stress, a larger area of the bed reaches the melting point, removing potential cold sticky spots and reducing basal resistance, allowing the ice to slide more rapidly.

Reference

Gagliardini, O., Cohen, D., Raback, P., and Zwinger, T.: Finite- element modeling of subglacial cavities and related friction law, J. Geophys. Res., 112, F02027, doi:10.1029/2006JF000576, 2007.