(1a) However, to me it is unclear why these particular values for ablation and accumulation gradients, and these equilibrium line altitudes (ELA), are used? The values seem almost randomly chosen, and the five simulations have no logical sequencing of changing one parameter at the time (which would help to better understand the impact).

Values of ELA and accumulation and ablation gradients were not chosen at random. A starting point was the estimated LGM ELA altitude of 1200 m by Haeberli and Penz (1985) and Haeberli and Schlüchter (1987), and the value of about 1000 m by both Benz-Meier (2003) and Keller and Krayss (2005). Thus we choose a range of ELA between 1000 and 1200 m. The realistic range of accumulation/ablation gradient together with the ELA value that yields a reasonable Rhine glacier is actually relatively small and our values bracket that range. Given the computation time (weeks) necessary for a Stokes flow simulation at such a high spatial resolution, we only show 5 simulations that reached over 1500 years. Other simulations with shorter simulation times were started but not completed, for the most part because the parameters rapidly yielded too large and unrealistic ice extents. We think that, given the difficulty in obtaining these simulations (time constraint), our sampling of five is more than is usually obtained for this type of ice flow simulation. A systematic investigation of parameter space is unfeasible for such a large computing problem, at least today.

(1b) Related to Page 12, lines 19-28: This section is slightly confusing. Why is simulation s1 referred to as the cold simulation, while it actually has the lowest ELA of all five simulations? The surface temperature is defined by the ELA, or?

We think the reviewer meant that s1 had the highest ELA (1200 m). Simulation s1 has the smallest surface melt rate at the terminus owing to the smallest ablation mass balance gradient. This necessarily arises from colder conditions in comparison to other simulations with higher ablation gradients. The surface temperature is indeed defined by the ELA (and thus colder with decreasing ELA), but because we have decoupled the surface temperature from the mass balance, and mass balance effects on glacier flow surpass those of temperature, we rank this simulation as the coldest one based on melt rate at the terminus alone. All simulations are labeled cold or warm based on the mass balance values, not the surface temperature. We will state this point in our revision.

(1c) Also the directional component is confusing: wetter climate in the south cold and dry in the north. I thought that the surface mass balance and the surface temperature both only depend on elevation, not on wind or moisture supply direction? Actually, including a directional component might improve the modeled glacier shape to the observations. By imposing a South-North gradient in accumulation, it might become more difficult to glaciate the Hornli ridge (as is now the case in s2, s4 and s5), better fitting the geomorphological observations.

Indeed our surface mass balance and temperature only depend on elevation and not on direction. However, since north-south gradients in accumulation are not known precisely, and since elevation generally increases southward in the model, our high accumulation mass balance gradient simulation could be the result of a combined increased in mass balance with elevation and with southern orientation. Our comment was meant to be conceptual and not quantitative.

(1d) Page 25, lines 23-26: It is unclear how you can calculate temperatures from your surface mass balance, if these are uncoupled. Please explain this more carefully.

The mass balance and temperature are uncoupled in the numerical model for the purpose of computing the ice flow (we believe this is ok because the effect of temperature is of a lower order than the mass balance). However, we can still compute melt rates (and using a PDD factor recover summer temperatures) from the imposed mass balance gradients and mass balance at the terminus. This is what is done in that section. We will try to make that clearer.

(2) Initial conditions for ice surface: To me it is not entirely clear which initial ice surfaces are applied. For simulation s1 the reconstruction of Benz-Meier is used, and for simulations s2-s5, other simulations that ran 440 and 907 years provided the ice surface. In the latter case, are these also based on the Benz-Meier reconstruction? In other words, is the reconstruction always used as basis, followed by 440 or 907 years of simple climate forcing (before simulating s2-s5)? What is the reason for using different initial conditions? I am asking this, because I think that the initial conditions possibly have a strong impact on the model results. But it

is difficult to extract this impact due to the (to me random) set-up of the model simulations.

The reviewer is correct. For the simulations indicated to use a previous simulation, these previous simulations always started from the Benz-Meier (2003) ice thickness and extent. We will make that clearer in our revision.

(3). Geothermal heat flux: I agree that adjusting and interpreting the geothermal heat flow data available is beyond the scope of this work, but it would be good to see a map of the values used in the simulations. How much does the basal temperature depend on the geothermal heat flux applied? And in how much does this boundary condition of geothermal heat flux define the basal conditions simulated in this study? In other words, does the geothermal heat flux pattern predefine the basal temperature pattern?

Although the geothermal heat flux varies between 60 and 120 mW/m^2 over our model area of the Rhine glacier, the high values of geothermal heat flux are not, to first order, correlated with temperate basal conditions. Ice flow and climate are the first-order controls. Temperate basal conditions are found upvalley in the Alps where the geothermal heat flux is significantly lower than in the Swiss lowlands occupied by the Rhine lobe. We could include the map of geothermal heat flux we used to argue this point but that would add an extra figure. Instead we propose to add a few explanation in the text.

(4) Steady state: I agree that you should not aim for reaching steady state with your simulations, as indeed climate and ice rarely reach a steady state due to the long response time of the ice compared to climate variability (DO and other variability). It would therefore indeed be unlikely that the Rhine glacier would be in equilibrium with the LGM climate. The argumentation for this (page 23-24) can be written more concisely. Also some studies suggest that DO1 occurred during the last deglaciation, so rather write: . . . called Dansgaard-Oeschger (DO) events occurred repeatedly during Marine Isotope Stage 3 (MIS3, 60-30 ka BP). Also, it is difficult to define the duration of the LGM, so I suggest deleting the sentence That period lasted around 2000 years . . . Bernese Alps.

Points well taken. We will modify the text accordingly.

Specific comment

Page 1, line 3: fully-coupled; what do you mean with this? Readers might think that the model is coupled to a climate model which it is not. Here, and generally in glaciological modelling, the term is meant to indicate the coupling between ice flow and ice temperature (thermo-mechanically coupled) because sometimes ice temperature is assumed (e.g. constant). We will make that point clearer.

Simulated time; why did you not run all simulations the same length of time, or until they reached the same rates of (dis)equilibrium?

The main reason is due to the long computational time necessary for the simulations. Each simulation takes several weeks of computer time in a parallel processing environment. Also, in simulations 3 to 5, ice extent increased past the LGM margin, the solution became unreliable because of lateral boundary effects, and the simulation was terminated.

TECHNICAL COMMENTS

Figures 1-3 are difficult to compare for non experts of this region. Could you indicate the overlap in the figures, by for example, outline boxes? Point well taken. We will add the outline of the Rhine glacier on Figures 1–3.

Page 10 and Table 2: Please note that the notation of the upper bound for the accumulation rate is not the same. Correct. Will be changed.

Fig. 4: would it be possible to indicate the location of the terminal moraines in this figure? Figure 4a, which reproduces the geomorphic reconstruction of Benz-Meier (2003), follows the terminal moraine. We will make this point clear in the figure caption.

Fig. 4-16: The double color scale makes some of the figures difficult to understand. I would suggest to discard the ice-free topography, as this is the same in all figures; and make that white. If you do decide to keep the ice-free topography, than please label the colour scales in the figures, and possible use a more dissimilar colour spectrum for the ice-free topographies, as the brown and

red are difficult to distinguish.

We don't find the two color schemes confusing except perhaps in Fig 14 (reviewer's last point below) but we will take this comment into consideration for the final version of the manuscript.

Fig. 4-16: please delete (Table 2) from the caption, not necessary.

We will delete these words.

Page 15, line 32: similar instead of nearly identical

We will modify the text.

Page 22 and fig. 11: Please use either ratios (0-1) or percentages (0-100), for consistency.

We will change the text.

Fig. 14: This is an interesting figure to compare with Fig. 8. However, it would be clearer if only the extent and thickness of the temperature basal ice was shown, not the basal topography as well.

Point well taken. We will modify the figure accordingly.

References

Benz-Meier, C.: Der würmeiszeitliche Rheingletscher – Maximalstand. Digitale Rekonstruktion, Modellierung und Analyse mit einem Geographischen Informationssystem, Ph.D. thesis, Universität Zürich, 2003.

Haeberli, W. and Penz, U.: An attempt to reconstruct glaciological and climatological characteristics of 18 ka BP Ice Age glaciers in and around the Swiss Alps, Zeitschrift für Gletscherkunde und Glacialgeologie, 21, 351–361, 1985.

Haeberli, W. and Schlüchter, C. (1987): Geological evidence to constrain modelling of the Late Pleistocene Rhonegletscher, Swiss Alps. The Physical Basis of Ice Sheet Modelling. Proceedings of the Vancouver Symposium, August 1987. IAHS, 170, 333–346.

Keller, O. and Krayss, E.: Der Rhein-Linth-Gletscher im letzten Hochglazial. 2. Teil: Datierung und Modelle der Rhein-Linth-Vergletscherung. Klima-Rekonstruktionen, Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich, 150, 69–85, 2005.