

***Interactive comment on* “Reconstructing glacier-based climates of LGM Europe and Russia – Part 1: Numerical modelling and validation methods” by R. Allen et al.**

Anonymous Referee #2

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General comments

This manuscript describes a simple model which can predict, from climatological averages of temperatures and precipitation, the presence or absence of glaciers in Europe and Russia. As argued by the authors, such a model can be very useful in reconstructing climates compatible with past glacier distributions. Potentially, for a climate such as the last glacial, this gives a continental scale climate reconstruction, which

can be compared with climate model results and other reconstructions such as those based on pollens. In fact, such a reconstruction intrinsically gives additional information about climate changes at higher altitudes than pollen-based climatic estimates, for regions which are particularly vulnerable nowadays. This adds to the long list of potential benefits that the authors provide in the introduction and justifies the publication of this work.

The simplicity of the model allows for a systematic study of the impact of variations in its main parameters, i.e. the temperature and precipitation lapse rates. In my opinion, this aspect should be better emphasized in the manuscript. Obviously, the authors have put a lot of effort in summarizing a large number of experiments, and have chosen to show a limited selection of their results. I think this selection is a little bit too limited. For instance, most of the results shown are for the Alps, but the authors state that the results are different for other regions, such as Scandinavia. The reader would benefit from figures from this region, to judge by him/herself.

Finally, I am ill at ease with the choice of the authors to try and reconstruct temperature and precipitation "only", which is very restrictive. Their model is based on the use of two key variables: the sum of positive air temperatures in an annual cycle (PDD, for positive degree day) and the precipitation. In pollen-based reconstructions, as explained by the authors in their introduction, key "bioclimatic" variables such as the sum of temperatures greater than 5°C (GDD5) or the temperatures of the coldest and warmest months, are estimated, because these are the most sensitive climate related variables for this type of proxy. Here, in a glacier-based reconstruction, one of the key variables is the PDD. I think that the value of a reconstructed PDD is as important as temperatures and precipitation reconstructions, even though those are easier to understand for a wider community. The authors do not hide (this is discussed at the end of Part 2) that the method to compute the temperature from their model makes the assumption that there is no change in the seasonality of temperature. But the change in seasonality is one of the driving forces in the Milankovitch theory. Therefore, a change in reconstructed PDD

can be interpreted as a mean temperature or as a seasonality change. It would be a pity if the authors did not give their reconstructed PDD along with their reconstruction of traditional climatic variables. If the authors give the maximum information that they can get from their model: primary variables such as PDD, and derived variables such as temperatures, the hypotheses they make to obtain their results can be analysed from climate model output. If they don't give the primary variables, in my opinion, the dataset such as the one they obtain for the LGM would be much less valuable.

I therefore recommend this manuscript to be published after major revisions of the text and the addition of a few figures for regions other than the Alps.

Detailed comments

Introduction:

Pollen based reconstructions for Europe have recently been updated. See (and cite!) articles by Wu et al (Climate Dynamics, 2007) and Ramstein et al (Climate of the Past, 2007). Wu et al confirm the impact of glacial CO₂ on the reconstructions.

Model data:

Does the LGM glacier altitude reconstruction take into account the isostatic effects near the Fennoscandian ice-sheet? This might actually be more relevant for Part 2 of the manuscript, even if LGM altitudes are mentioned in this section.

Page 1139: why were precipitation rates assumed to be constant throughout each month?

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Page 1139: equation 4 assumes the minimum temperature to be reached at 3 am. Is it reasonable? Is it important?

Page 1142, model adjustment: here, the authors should clearly state that they adjust their model for all regions at once. I realised this much later in the text (and I hope I correctly understood this crucial aspect of the method). Then it would be interesting to show the cost functions for other regions. Also, it would be interesting to know the number of tests on different combinations of temperature and precipitation lapse rates plotted on Fig 3.

Present day experiments

It would be nice to see the results for the other regions: Caucasus for section 4.2 for instance, but also Scandinavia, for which the results are not as good as for the Alps. There is a comment about this in section 5, but no figure to sustain it!

In this section, the figures should be commented more extensively.

Discussion

Page 1147, line 14: "positively skewed distribution of glaciers". Fig 6 shows a positively skewed distribution of the ELA, but not of the glacier distribution...

Page 1148, lapse rate discussion: it should be noted that lapse rates along the mountain slopes are not necessarily equal to the environmental free atmospheric lapse rate, especially when the surface type changes from a surface covered by vegetation to a glaciated surface (e.g. Kageyama et al, Quaternary International, 2005).

Page 1148. Have the authors considered the HISTALP data? This is at high resolution, but I do not know if it has been already integrated in the CRU data.

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Conclusions

The authors should discuss the applicability of their model to other regions, such as the tropics.

Minor comments/typos

Past tenses are used throughout the paper, which gives the impression that this work has been performed a long time ago. I would prefer the present tense to be used for the work the authors did and present in this manuscript.

The word "glacierized" is frequently used, but sometimes not spelled right (e.g. glacierized). Does this word really exist, by the way?

Abstract:

Line 2: "prescribed" to be replaced by "ascribed" or "attributed"?

Line 5: "prolonged" to be replaced by "prolonged";

Page 1134, last word: spelling of Joussaume (with a U). To be corrected in the reference list too.

Page 1135: update PMIP2 reference as Braconnot et al, Climate of the Past, 2007 (Part 1)

Page 1136, line 3: add "distribution" after "glacier"

Page 1136, line 10: what is the interest of reconstructing the climate from a future glacier distribution? (Sentence to be reformulated)

Page 1138, line 22: the definition of "hypsometry" comes afterwards!

Page 1140, line 13: "scaled up"? Do the authors mean "added up" Same line: please start a new paragraph with "The CRU dataset was downscaled", it is a new topic.

Page 1141, first sentence of section 3.2 is not a complete sentence

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