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Climate of the Past Discussions

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

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Review of "Reconstructin glacier-based climates of LGM Europe and Russia — Part I : Numerical modelling and validation methods".

1 Resumé

The authors present a *glacier-climate model* that uses, as input, a regional climate dataset, a 1-km resolution topography dataset and a glacier extent database and predicts, as output, the average snowline altitude and the climate at that altitude. The

process is validated against observations and there is also a series of sensitivity experiments aiming at demonstrating the robustness of the approach against uncertainties in the input datasets. The purpose of the paper is to set the basis for a methodology aiming at reconstructing the LGM climate from glaciers palaeo-reconstructions (subsequent articles submitted by the authors).

2 Overall evaluation

The introduction of the paper makes a rather convincing case of the potential utility of the approach : there is a need for palaeoclimate reconstructions complementary to "biological proxies" (pollen and macro-fossils). Being not an expert on glaciers models, I will not question the adequacy of the technical assumptions related to the glacier model itself [1]. One major critic I would like to address to this paper is a constant discomfort, by the reader, arising from the difficulty to correctly distinguish hypotheses (the "input") from the result (the output). There is indeed a constant impression of being trapped in a circular argument, even if it is not necessarily the case. A number of awkward sentences (highlighted in the attached annotated document) reinforce this impression. At the very least I would certainly encourage the authors to state more rigorously the inference flow, possibly with the help of a sketch but more ideally by means of a mathematical analysis using, for instance but not necessarily, pseudo-data and Monte-Carlo sampling. Furthermore, the rationale behind the form of the cost function is not established. The second problem with this paper is that the output — compared to present-day observations — depends directly on the CRU dataset. Therefore, what I would be tempted to call into question here is the nature of the inference process itself. If you need CRU to reconstruct climate, what will you do at the LGM ? Perhaps it would

[1] even if it may seem odd, section 2.3, to warn against using the methodology to study recent climate change and then validate it against the 1961-1990 time span, a difficulty alluded to in the discussion section.

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have been more appropriate to calibrate a reconstruction process for one region of the world (e.g.: the Alps) and test it on another one (e.g.: Scandinavia). Admittedly the Alpine and Scandinavian climates differ (one is more continental, the other one is more maritime) but surely, for a given region, the LGM and present-day climates will differ, too.

As a consequence, the reader is left with the impression that the adequacy of the method to reconstruct past climates has not been established, which is unfortunate given that two other papers, based on this one, were already submitted to Climate of the Past Discussion.

3 Detailed comments

A number of comments on details are formulated in an annotated document sent to the authors via the editor. Perhaps the most important one concerns the conclusion extracted from Figure 7 ("it was found that the results presented here were the otimum results achievable using the outlined modelling approach") while what Figure 7 seems in fact to tell us is the robustness of the optimisation process against uncertainties of the input dataset.

Interactive comment on Clim. Past Discuss., 3, 1133, 2007.