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Climate of the Past Discussions

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Interactive Comment

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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I would like to thank the authors for trying to answer the different questions raised by the reviewers. Nevertheless, there are, to my point of view, some questions that still need to be addressed.

Reviewers (in both Part I and Part II) underlined a lack of clarity in the inference flow. You acknowledged this. However, your comment does not fully address this question. Moreover, I think that it is a drawback of the trilogy. Actually, the tool developed in one paper is used in the other but I feel that a clear overview of the whole trilogy is missing. As far as the modelling is concerned, I understand that the glacier-climate model is forced by temperature and precipitation (including lapse rates) in addition with spatial distribution and geometry of the glaciers. The model is then able to simulate several glacier features. In Part one, the model is used in what we could call a direct mode. The purpose is to show that it is indeed able to produce correct glacier features. In Part two, it is used in an ?indirect? mode. The model is forced by a large variety of temperature and precipitation values. Then the version leading to the best-simulated LGM glacier features is selected and consequently, the forcings are assumed to correspond to the climate state at the LGM. If this were correct, it would be worthwhile explaining it in the first paper (maybe also in the second). It would avoid the kind of question raised by reviewer 1, i.e. " which climate reconstruction will you use at the LGM ".

Reviewer 2 raised two technical comments, which answers remain rather elusive. First, there is the question of isostatic effects, which seems indeed relevant. You " feel that the omission of this factor would not affect the results ". I am sure that the reader would be happy to know the arguments that support your feeling. Second, there is the question about the minimum temperature occurring at 3 am. You answered that this equation has no impact on the model results. Therefore, I am wondering why it is presented in this paper (and even why it is used in the model). By the way a parenthesis is missing in equation (4).

Another question raised by both reviewers is what reviewer 2 called "model adjustments", which is the selection of the optimal value for the lapse rate. Reviewer 1 wrote "in your study, the same lapse rate is applied everywhere (but an optimal value is selected), while the lapse rate range given by the cited authors is a range of measurements taken at different locations". I think it would be worthwhile to have a discussion about selection of the lapse rate. I understood that you selected one lapse rate for each glacier. Would it be possible to select a single lapse rate for all the locations or would it be sensitive to select one for each grid cell (or group of grid cells)?

Reviewer 1 also made two comments in the discussion section. It would have been interesting to have your response.

C1. “In Northern Scandinavia, the cost function value increases significantly in sensitivity analyses where the DDM is able to simulate more positive annual mass balance compared to the control simulation. This suggests that the baseline climate across Northern Scandinavia predicts a local rather than global optimum solution”. Reviewer 1 wonders whether this “doesn’t suggest more simply that the DDF and lapse rates differ in a maritime climate (Scandinavia) to that of a continental climate (Alps / Caucasus)”.

C2. “As such the majority of glaciers are likely to be influenced by significant local topographic or climatic factors, e.g. steep sided valleys reducing direct insolation, topographically induced precipitation, or wind blown snow ”. Reviewer 1 suggested that “what you seem to discuss here are also fundamental limitations of the downscaling method (which does not include enough physics), i.e., even with a high-resolution topographic dataset (which will not be available at the LGM anyway), these processes would still be missing? ”. Do you agree with that?

I really hope that these points will be discussed in the revised version if you consider submitting one.

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

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