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Climate of the Past Discussions

Interactive comment on "Impacts of land surface properties and atmospheric CO_2 on the Last Glacial Maximum climate: a factor separation analysis" *by* A.-J. Henrot et al.

A.-J. Henrot et al.

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We thank Andrey Ganopolski for his constructive comments on our paper. We will address his questions and suggestions in the following.

General comments:

Comment 1: The paper by Jahn et al. (2005) as well as the report of Berger et al. (1996) both of which we had, unfortunately, overlooked have been considered for the discussion in the revised manuscript.

The Stein and Alpert (1993) factor separation method is indeed first of all a technical procedure for the analysis and assessment of the impacts of several factors in the framework of sensitivity studies. However, we find that it has undeniable advantages over the sequential addition method, where factors are added in some—generally unjustified—order. The Stein and Alpert (1993) method offers a rigorous and systematic framework for the comparison and quantification of the relative importance of factors at work, as well as the effects resulting from the interactions between different factors in a non-linear system. This is clearly not the case for sequential addition method, where the impact of any given factor is assumed to be given by the difference between the first simulation experiment where it was considered and the one immediately before. Obviously, such an estimate will depend on the order used to set up the sequence, except for linear systems, where both methods should give the same results. Although it might be argued that a linear approximation might hold for small perturbations in the climate system, the glacial-interglacial perturbations such as those studied here drive the system out of the linear domain. This is also demonstrated by our results.

Comment 2: We agree with A. Ganopolski, that all of the factors considered here are not rigorously independent. However, in the climate system more or less all variables are correlated with each other, at least to some extent. It should be emphasized here that vegetation changes do *not* affect ice covered pixels. In the discussion paper, we omitted to mention that we only changed the vegetation cover on the land points *not* covered by ice at the LGM, in order to not overestimate the vegetation impact. This has been clarified in the revised manuscript.

We do not entirely share the reservations of A. Ganopolski regarding the applicability of the Stein and Alpert method with our setup. For perfectly correlated factors, we would indeed expect the contributions to be fully additive, in a linear system. However, the climate system is certainly not linear and the obtained contributions are not infinitesimal

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either; two factors may well be correlated, but not perfectly. We feel that even for such factors, the factor separation method gives useful results. Regarding the additivity of several factors: taking the reservation expressed by A. Ganopolski at face value, we must not even apply the factor separation method to analyse the impact of two different uncorrelated greenhouse gas changes. Indeed, if their absorption bands overlap, their radiative forcings are not additive.

We concede that experiment O does not reflect a realistic state of the LGM climate system and that it is of entirely theoretical nature. Our simulation experiments nevertheless emphasize the important role that the orographic change associated with the ice-sheet growth plays alone for the dynamics of atmospheric circulation in the Northern Hemisphere on the circulation, even without the concomitant albedo change. We find that this is an interesting result in itself. Finally, let us recall that Referee #1 has singled out in his "Comment 13" the findings related to experiments I, O and IO as very interesting and worth highlighting in the abstract and the conclusion.

Comment 3: Referee #1 put forward similar concerns. We have revised the corresponding part of the discussion (please refer to our response to "comment 12" of Referee #1 for more details).

Comment 4: We have added in the experimental setup (section 3) of the revised manuscript a paragraph discussing the use of a slab model in this study. We have also discussed potential missing feedbacks due to the lack of oceanic circulation changes in our experiments and compared our results to those of Jahn et al. (2005), who included oceanic feedbacks in their analysis. The shortcomings related to the use of a slab-model and possible consequences are also presented in details in our response to Referee #1's "Comment 4".

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Comment 5: As suggested, we have clarified this issue and mentioned in the revised version that we did not consider the effects of CH_4, N_2O and atmospheric dustiness changes at the LGM. We have referred to the quantified effect of dustiness (Schneider von Deimling et al., 2006) and compared it to the contributions of the factors considered here.

Comment 6: Thank you for this suggestion. It is indeed more consistent to compare the PMIP1 and PMIP2 coolings to the cooling produced in our experiment CIO (-4.3°C), which does not include vegetation changes, rather than to our LGM total cooling (-5.2°C). We have included a more detailed comparison in section 4.3 of the revised paper.

References

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