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Interactive Comment

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.

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The manuscript presents a systematic factor analysis of role of different LGM boundary conditions using a climate model of intermediate complexity. Although a number of the studies of this sort have been published already, I believe, this is potentially useful paper. However, to be publishable in the CP it requires a major revision.

General comments

1) As it was already correctly pointed by the first Reviewer, the use of Stein and Alpert separation method for analysis of glacial climate is not absolutely new and we applied it in Jahn et al. (2005). And since I used both approaches to factor analysis in my

previous studies, I can frankly admit that I am still not quite sure which of two methods is more useful for this purpose. Indeed, in a linear system both methods give identical results whilst in a non-linear system the interpretation of the synergy between different factors can be rather tricky or even misleading. In any case, any factor analysis (in climate modelling) is just a technology aimed on a better understanding of the role of different factors which not yet were well-understood or incorporated into the existing models.

2) However, what is really important for the factor analysis, is that the factors should be independent. My problem with the present paper is that some of the factors considered by Henrot et al. are not independent. Indeed, whilst the radiative forcing of CO2 and radiative forcing of the LGM ice sheets are (almost) independent, the effect of the ice sheets, vegetation and elevations are not because these three factors affect surface albedo in the same grid points (covered by ice sheets) and their radiative forcings are not additive, i.e. the total radiative forcing in the IOV experiment is not equal to the sum of I, O, and V radiative forcings. Therefore, the fact that IOV experiment produces less cooling than the sum of I, O and V does not result from some mysterious interactions between these three factors but just from the fact that these three factors are not independent in terms of radiative forcings. In short, I do not believe that comparison of I, O and V experiments with IO, IV, OV and IOV makes much sense, if at all.

3) Concerning the role of vegetation. The authors cannot compare their V experiment with the previous studies because in all previous studies cited in the paper, the vegetation changes were imposed under the LGM boundary conditions (i.e. together with LGM ice sheets). The later makes much more sense to me (compared to V experiment described in Henrot et al.) because it shows the magnitude of additional cooling resulting from vegetation changes which were not taken into account in most of LGM simulations. In any case, to compare apples with apples, one should compare with the previous studies the difference between LGM and CIO experiments rather then the cooling in V experiments. The difference between LGM and CIO is about 1.1C (com-

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pared to 1.3C cooling in V experiment) which is still larger than in Ganopolski (2003) and Crucifix and Hewitt (2005). (Please note, however, that the total range of vegetation effect on the global temperature reported in Schneider et al. (2005) for a large ensemble of CLIMBER-2 runs is between 0.5C and 1C, which is not fundamentally different from the present study). In any case, a stronger vegetation effect reported in the present study has nothing to do with "rigorous analysis" but results from a larger change in vegetation cover, at least, compared to the CLIMBER-2 model. And, in any case, it makes no sense to compare global cooling simulated in the present study with early studies (Crowley Baum, 1997; Kubatzki and Claussen, 1997, Wyputta and McAveney, 2001) where SST was fixed and therefore the effect of vegetation on temperature was restricted to land areas.

4) I agree with the first Reviewer that the LGM simulation with a slab ocean model looks obsolete now-days but I would not consider this fact as the fatal for the paper. Still, a more rigorous discussion of potential caveats is required, moreover that the role of the ocean circulation for the LGM climate has been studied in a number of papers. It is true that the state of the glacial ocean circulation remains highly uncertain but still most of the models show considerable change in meridional ocean transport and in the locations of deep water formation area which cannot be captured by a slab ocean model. There were also several studies were simulations with a slab ocean model were directly compared with OGCM for the LGM climate (Ganopolski et al. 1998, Kim et al. 2003 and Hewett et al. 2003). All these three studies indicate considerable regional impact of the ocean circulation change but strongly disagree in respect of the global effect of the ocean circulation.

5) The authors should mention that they did not consider the whole set of potentially important LGM forcings. Among them is an increase of the atmospheric dustiness. For example, Schneider et al. (2005) has shown that the impact of dust on the global temperature might be even larger than the effect of vegetation changes. In addition, CH4 and N2O were lower at LGM which is taken into account in the PMIP2 protocol

but not in the present paper.

6) Due to the fact that most of previous studies (i.e. PMIP1 and published PMIP2) did not account for vegetation changes, it makes sense to compare with these studies the global cooling in the CIO experiment rather than in the full LGM run.

Additional references

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