

## ***Interactive comment on “Strength of forest-albedo feedback in mid-Holocene climate simulations” by J. Otto et al.***

**J. Otto et al.**

juliane.otto@lsce.ipsl.fr

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Reply to Referee 3

We thank the referee for providing his/her review and suggestions which we would like to reply to.

Major comments:

(1) “This reviewer regards they are fundamentally incomparable because result in this manuscript does not include the ocean feedback and previous reconstructions/simulations include the ocean feedback.”

In a previous study (Otto et al 2009a,b) we used the factor separation method by Stein

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and Alpert (1993) to explore the relative contributions of the atmosphere-vegetation feedback, the atmosphere-ocean feedback and their synergy to the difference in temperature patterns north of 45°N between mid-Holocene and pre-industrial climate. Here we extend our earlier study to focus on the pure contribution by the atmosphere-vegetation interaction only. Hence we have excluded any synergistic effects by and with the ocean dynamics.

For the estimation of this pure vegetation contribution, it is sufficient to perform simulations with the same prescribed ocean (see also the book by Alpert and Sholokhman (2011), chapter 4.3 by Berger et al.). We perform four simulations (AV6k, AV0k, A6k, A0k), all with the same prescribed monthly mean of SST and sea-ice cover from a pre-industrial AOV0k control run. On the basis of these four simulations we determine the pure contribution of the atmosphere-vegetation interaction to the mid-Holocene climate signal.

This implies that we can compare our results with previous studies only, if these studies explicitly address the pure contribution by atmosphere-vegetation interaction in a setting that is similar to ours. Such studies have been provided by Ganopolski et al. 1998, Crucifix et al 2002, and Wohlfahrt et al 2004. We will revise our manuscript to discuss this point in more detail, and we will compare our results only with studies as given above that use the same method as we do.

The referee claims that we cannot compare our results with temperature reconstructions of the mid-Holocene. This is a valid statement. However, we believe that we can compare at least the AV simulations with reconstructions. Previous modelling studies have revealed that the interactions with the ocean are not decisive for spring temperature over land (Otto et al. 2009a,b, Wohlfahrt et al. 2004). Thus, the comparison of the AV simulations with reconstructions gives at least an indication on the validity of our results and if the new parametrisation improves the simulations (e.g. increase in boreal forest, temperature signal) in the right direction. In the revised manuscript, we will emphasise this point that we use this comparison only as an indicator but not for

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an elaborated validation.

(2) "...related discussion about EMICs are not directly based on the result in this manuscript because it can only explain the effect of strength of snow masking."

We agree that the current discussion about the resolution dependence of the models is not straight forward. In a revised version we will restructure our discussion. It is striking that the previous studies (Ganopolski et al. 1998, Crucifix et al 2002, Wohlfahrt et al 2004) simulated a stronger contribution by the pure atmosphere-vegetation interaction to the spring temperature. This stronger anomaly goes along with a stronger increase in boreal forest compared to our study. We tentatively suggest that this deviation might be model-dependent and we suggest that new simulations with new model set-ups should be undertaken to address the validity of our suggestion.

In summary, your comments showed that we have to revise our manuscript with special attention to the discussion of our results.

Minor comments:

We will follow your suggestions in a revised version of the manuscript and we will especially take into account your comments on snow definition.

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