

Comment on „Strength and limits of transient mid to late Holocene simulations with dynamical vegetation” by Pascale Braconnot et al .

In their manuscript, the authors present the first transient Holocene simulation with the IPSL model, which also contains dynamic vegetation. They discuss the influence of the model setup on the results and work out possible challenges when comparing transient simulations with reconstructions. The authors have once again put a lot of effort into the manuscript and have completely restructured it, which makes the motivation for the extensive discussions much more obvious. The revised version is much clearer and more comprehensible than the first version. The readability has considerably been improved. Nevertheless, I still have a few comments on the general structure:

a) I still find the first part about the different model versions tedious and don't really see the advantage of discussing them so extensively. This does not really fit into this study and the authors do not discuss them further in the course of the article. It is always good to know how changes in the model setup affect the representation of climate and vegetation, and I also think that changes in the model need to be documented and compared with previous versions to better assess the model results, but I think it might as well be shifted to the appendix. Then the focus would be even more on the transient simulation, as the title promises. It would also reduce the number of figures in the main text.

b) The main objective of the study is not fully clear, whether the focus is more on introducing the transient simulation or on the challenges of comparing such simulations with reconstructions, or maybe on both. And the main topic should be reflected in the title. I think the current title is not appropriate, because the strength and limits of transient simulations are only shortly addressed. The 'limits' (or better challenges) are discussed, but most of these limits also exist for time-slice simulations. The „strengths“ compared to time-slice experiments should be more strongly emphasised. An important, albeit trivial, aspect in this respect is also that reconstructions usually only exist punctually in large parts of the earth and also in a very coarse temporal resolution. The transient simulations have the great advantage that - as described in the article - they show no fixed climate and vegetation state but also the transition to this state and are therefore (at best) suitable to close the gaps in the reconstructions and may also be used to check the consistency of the records. The records are also not 'perfect' and without biases. With transient simulations more opportunities exist for the model-data comparison.

c) I like the main questions raised in the introduction. The individual chapters are very well aligned to these questions. I think it would increase the clarity of the paper and also 'round off' the paper if these questions were also answered systematically. They could either be picked up (and even repeated) in the conclusion, or a small summary could be added at the end of each chapter to answer the question and summarize the main, relevant result.

Minor and technical remarks:

- I do not list any spellings or grammar mistakes, these will be fixed during the CP copy-editing.

- L 85: ... the timing of the major vegetation change in different regions

- chapter 2.1: If I understand it correctly, the test simulations ran with PMIP3 protocol and the transient simulations with PMIP4 protocol. I still find the large number of simulations confusing, or find it difficult to get what was used in which simulation. Maybe you could extend tables 1 and 2 by

a few columns and thus classify more clearly which model version, which PMIP protocol and which initialization were used. I find the table confusing. For example, actually all simulations with PMIP4 forcing are marked with FPMIP4, but TROHOLV is not, you can only guess the information. For MH-Vmap and MH-Vnone it says that the model ran with 11 layer hydrology and Aerosols and Evaporation factor, for others it doesn't or only indirectly. You could simply add the columns L11, Aer, Ev and then check which components are used, unless this model version has been used in all simulations in table 2, but then you could write it down centrally and not mention it in any simulation. Additionally a column for forcing (PMIP3 or PMIP4) and for initial state (bare soil or map) would be helpful.

L187: Do you mean MH-PMIP3?

L205: it is PI-FPMIP4.

L209-211: Is this sentence complete?

L240: Why have you chosen MH-Vnone as initial state?

L.260: Please define seasonality also in the main text.

L264: I would add (Fig.5) at the end of the sentence.

L271: This statement is too general. According to Fig.6, precipitation and temperature during summer follow the insolation change in the NH, this is also true for NH winter temperature, but not for the other curves.

L277-278: This sentence sounds odd.

L296: only 40% or rather 50%?

L354: there is no substantial reduction of tree cover in Eurasia. These sentences are only valid for the region north of 60°N.

L510-511: I do not understand this sentence.

L530: reconstructions instead of observations

L533: reference of Marsicek et al is missing in the reference list.

L573-574: This sentence sounds odd.

L651-654: The table showing the metrics of the climate-based biomisations only occurs in the Discussion Paper and not in the finally revised version.

Table 2: The heading: Sensitivity experiments to dynamical vegetation seems to miss a verb?

Figure 1: ...modified version of the IPSLCM5A-MR model in which or something similar.

Figure 3. Description of Panel a) and b) are switched. And ta_200 is probably the temperature on the 200hPa level and not on the 300hPa level.

Figure 4: I wonder if vegetation in MHVnone would converge to the vegetation in MHVnone_FPMIP4. The 'jump' from the grass and tree fraction in MHVnone to MHVnone_FPMIP4 is very large. This is confusing.

Figure 5: The label of the red axis is wrong.

Figure 6, 3rd line: 6000 years BP.

Figure 8: I'm confused why the spread in minimum and maximum temperature increases from 6ka to 0ka for Eurasia and the region north of 60°N.

Figure 10: The last sentence is incomplete.

Figure 11: Please add coordinates for the WAfrican region. And the green curve for north of 60°N is not completely printed on the plot.

Figure 15: The colour bars of the temperature plots are cut off. Line 4: it is c) and d) and in line 5 it is TRHOLV.