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Climate of the Past Discussions

Interactive comment on "Impact of CO_2 and climate on the Last Glacial Maximum vegetation" *by* M.-N. Woillez et al.

Anonymous Referee #2

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General comments:

Overall, I think this is a valuable manuscript for the readers of CP. It provides a detailed analysis of vegetation changes between present day and LGM and investigates the relative importance of CO2 and climate effects, using the IPSL/ORCHIDEE model. The scientific progress is limited in that few new conclusions are presented; the introduction correctly references previous studies with overall the same findings. I see the main achievement of this study therefore in adding confidence in the robustness of earlier findings of modeling and observation-based studies by using another model. Given that the model applied here (IPSL/ORCHIDEE) is widely used, documenting its response to LGM climate and CO2 will be valuable for the community.

The methods are clearly described and appropriate (but I suggest the use of observa-

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tional data instead of model simulations for the validation of today's vegetation patter, see specific comments). The conclusions follow logical and caveats are generally appropriately discussed. The manuscript has the character of documentation, rather than a manuscript defining and then testing a new hypothesis, with much detail on results and less focus on discussion. This is fine, but the manuscript can be made more concise by removing some redundant information, in particular concerning figures. I suggest including the model name in the title to highlight the new aspect of this study.

Specific comments:

1. For the non-expert reader it would be good to clearly state in the beginning the direction of changes (e.g. that "regression" is backward looking and means less cover in LGM as compared to today).

2. The introduction gives an overview of previous studies, but lacks the overarching conclusion of the many individual studies and fails to identify which specific questions previous work has left unresolved – and thus justifies the present study (other than a check of robustness by using another model).

3. Section 2.2: Please discuss the reason why you chose prescribed present-day instead of dynamic vegetation for the LGM climate simulations (other than these simulations were already available from PMIP). I would assume that vegetation feedbacks have an effect on the LGM climate and that the setup with dynamic vegetation thus seems more consistent. I am not convinced that repeating the 1930-1980 time period (Table 2) with its substantial trend in global mean temperature is a better setup than using detrended data.

4. "Student's t-test" – in case this is not the modified test accounting for temporal autocorrelation, then the test should be modified in this direction and the text clarified.

5. Section 2.3: Why are the simulation times for the control and the LGM simulations different (300 vs 1000 years)? Please clarify if "mean" refers to monthly, daily, ...

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averages (l. 22).

6. “As the simulated LGM vegetation appears less dependent on the climatic forcing than the present-day one [. . .]” – how does this agree with the concluding statement “We can expect the vegetation to be more sensitive to cooling or drying at a low CO2 level”?

7. Section 2.3 and 3.1, discussion of biases in present-day vegetation: I agree that the biases will be of marginal importance for the general conclusions, but this could be even better justified if the discussion included the reason for the biases – are there dynamical reasons or growth limits for the bias that may play a consistently larger role in LGM than today? This may become relevant for the broadleaf/needleleaf issue (see my point 11).

8. Section 3.1 and corresponding figures: There are two sources for potential biases in the vegetation distribution: The IPSL climate model and the ORCHIDEE vegetation model. Using CRU data with ORCHIDEE only investigates the biases of the climate model. If observational vegetation maps were used instead of ORCHIDEE-CRU this would give a more complete and objective assessment of biases. I suggest replacing or extending the CRU analysis with observational vegetation data.

9. Section 4.14: “broadly find the same difference patterns” – the grass response seems substantially different.

10. Section 4.3: I see why the authors chose to place the method description together with the analysis, but really all simulation setups should go into Section 2. Readers can flip back a few pages if they have forgotten by now.

11. Section 4.3 and 5, competitiveness of broadleaf vs needleleaf. This is a less well studied point compared to most of the other conclusions and therefore very interesting. I am not aware of any physiological or experimental studies that would prove or refute this hypothesis. It would thus be good if the reasoning on p. 21, l. 4 ff is elabo-

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rated on and the parameters in the photosynthetic equations (name the schemes used in ORCHIDEE here) responsible for the change in competitiveness tracked down and reviewed for their validity. This will likely also help explain the different result as compared to the TRIFFID model (Crucifix et al.). Is water use efficiency a driving factor here? What role does the bias in the distribution of broadleaf vs needleleaf trees seen for the control simulations play? This discussion should not be placed in the concluding section.

12. Table 3: On the 17 preceding pages the reader has finally learnt what the PFT acronyms stand for, and now these acronyms are being dismissed in Table 3... I suggest keeping the PFT name in the simulation name (i.e. TROP310 would be TrBEP310, or TrBE.P.310, BBSG185 would be BoNSG185, or BoNS.G.185).

13. Fig. 6: How meaningful is this analysis? As area is rather arbitrarily fixed to the extent under present-day climate & CO2, the vegetation composition of course changes for the other simulations.

14. I see potential to shorten the manuscript without losing essential information. There is redundant information in the figures: Fig. 6 is largely a combination of Fig. 3 (panel 2), 4,7,9. Fig. 8 shows foliage coverage, which depends on LAI and area, which are both implicit or explicit in the area of presence and the LAI in Fig. 10.

15. Why is area of presence more meaningful than area of coverage?

Technical corrections:

1. Fix the citations (put in brackets).

2. Typos, grammar: mentionned (throughout manuscript); p. 12: differenciate, “this two variables”, this whole sentence does not make sense to me; p. 19: “this results”; doubled consonants in acknowledgments (Petterschmitt, usefull).

3. Section 5 brings new discussion points; “Summary and conclusion” does therefore not seem to be the appropriate title.

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4. Figure 1: The very similar colors left and right of 0 are an unfortunate choice.

5. Figure 1 and 2: The non-significant area is gray in the one and white/blank in the other. “mean annual monthly precipitation” is likely a typo?

6. Figure 5 caption: The weather generator should go into the method section.

7. Figure 14: To be consistent with Fig. 8 and 10 this figure should be in color.

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