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 observational 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).

We have added P3,L3 : «In all the following sections, we will always consider vegetation changes looking backward in time, i.e. at the LGM compared to present-day.»

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).

We wanted indeed to check the robustness of previous studies with a completely different vegetation model. But our study offer a more detailed analysis, with various different variables (area occupied, vegetation fraction, LAI, NPP). In particular, the use of vegetation fractions instead of biomes allow to look more accurately to vegetation changes, which might be unnoticed otherwise with the BIOME family models for instance (which provide only one single biome over a grid-cell). Our result suggesting a difference of sensitivity to CO2 between the needleleaf and broadleaf PFTs is a new result.
Our static simulations also allow us to evaluate the role of dynamics and competitivity vs photosynthesis, which had never been done so far. We have also discussed the impact of initial state and the role of the climatic background over a given area.

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.

The choice of a modern vegetation in the LGM run is indeed justified by the existence of this run in the PMIP data base. Simulating a glacial vegetation with ORCHIDEE off-line was actually the first step in order to make a new atmosphere-ocean LGM simulation, with an LGM vegetation as boundary condition. The setup with dynamic vegetation was unfortunately unavailable with this version of IPSL_CM4. However, one can notice that the presence of agriculture lead to a « LGM-like » vegetation over some areas: Europe for instance is covered by agricultural grass, and not forests, which can be considered as close to the steppic vegetation present at the LGM.

The period 1930-1980 for the setup of the control simulation was chosen because we wanted a modern vegetation (and not a pre-industrial one), as climate reconstructions based on pollen assemblages are calibrated on present-day climate. This period roughly corresponds to a plateau in the increasing trend of global mean temperature.

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.

We agree with this remark. The autocorrelation with lag 1 year varies from very small values on the continents to high values above oceans.
We just wanted to show the differences in the climatic forcing used to run ORCHIDEE off-line for

present-day conditions and for the LGM. As the focus of this paper is not the significance of temperature and precipitation changes simulated by IPSL_CM4 at LGM compared to present-day, we have redrawn the figures without the t-test. We have put in white the temperature changes smaller than 0.5 °C and precipitations changes smaller than 5%.

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... averages (l. 22).

We ran 1000 years for LGM in order to save the centennial variability of the vegetation in response to the glacial climatic variability for another future study.
« Mean » refers to monthly average, which has been specified in the text.

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”?

This first sentence is true at the global scale. We have change this passage as follows:
« At global scale, the broad vegetation pattern obtained with these two methods are quite similar (not shown). The simulated LGM vegetation appears less dependent on the climatic forcing than the present-day one: with or without corrections with an anomaly procedure, the change in climate is strong enough to drive a major forest regression in any case. For that reason, we chose to keep outputs from the IPSL model as forcing fields for all simulations, even for present day. This simplifies the forcing procedure and will also allow us to compare our results... »

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).

The reasons for the biases are presently unknown. Many variables may play a role. In the future, we want to improve the model and test the impact of different parameters (bioclimatic limits, rate of photosynthesis...etc). We have already performed some tests, but these have been unsuccessful in improving the model so far, so this will demand quite a lot of work.

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.

The precise evaluation of the biases for present-day was not the main focus of this paper, and we just wanted to point out the main ones, that the reader should keep in mind for the analysis of the LGM results. In our opinion we do not need a comparison with precise maps to recognise these main biases (overestimation of boreal forests, lack of grass and tropical forests in the Southern Hemisphere). A more detailed model-data comparison has already been done in Krinner et al, 2005, and even if the ORCHIDEE model has evolved since this publication, one can refer to this paper for a finer assessment of the biases.
Moreover, there is also the issue of the land-use in present-day maps. The present-day potential vegetation maps always use interpolation methods that we did not want to discuss here.

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

True, we just wanted to say that forets regress in both cases.
The first sentence has been replaced by: « We now compare...under low CO2 »

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.

We would prefer to let this method description here. We have added at the end of section 2.3 the following sentence: « This first set of simulations is completed with static simulations, in order to evaluate the role of dynamics and competitivity, see section 4.3 »

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-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.

The photosynthetic equations used in ORCHIDEE are based on Farquhar et al (1980) and Collatz et al (1992). Many parameters may be at stake to explain this difference of sensitivity between needleleaf and broadleaf : the way the equations are implemented in the model, the rate of carboxylation, the choice of some constant values, the bioclimatic limits, the competition for water and water-use efficiency...Therefore, other simulations will be performed to try and understand the cause of the difference. Some have actually been performed already, but this analysis is left for a future study.

The last section has been renamed « discussion and conclusion »

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).

The table, fig.14 and fig.15 have been modified.

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.

With this figure, we wanted to present concisely the changes occuring over the present-day referenced areas, and which PFTs are replacing the initial ones, and in which proportions.This figure presents the results more quantitatively than a map.

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.

This is true, but Fig.3 only shows the dominant PFTs and not the detailed composition on each grid-cell. This information is indeed present on fig 4,7,9, but fig.6 allows us to see rapidely and quantitatively the changes over a given area.

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.

The area of presence indicates only the extension of the surface where a PFT fraction is above 1%. With this single information, we have no idea of the density of vegetation. And if we look only at the global foliage projective cover, we can not know whether the changes come from changes in extension, or in the LAI (and then in the vegetation fraction), or both. These variables thus provide complementary information.

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

The area of presence shows whether the area where a PFT can establish and survive has changed or not. This cannot be seen with the area of coverage, which simultaneously depends on the area of presence and on the vegetation fraction, as mentionned previously.

Technical corrections:

1. Fix the citations (put in brackets). Ok
2. Typos, grammar: mentionned (throughout manuscript); p. 12: differenciate, “this two variables”, this whole sentence does not make sense to me;

« this two variables » has been replaced by « these two variables »

p. 19: “this results”;
doubled consonants in acknowledgments (Petterschmitt, usefull). Ok

3. Section 5 brings new discussion points; “Summary and conclusion” does therefore not seem to be the appropriate title.
This section has been renamed « discussion and conclusion »

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

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?

The figure have been redrawn, and we used white for the small changes in temperature and precipitation (see previous answer to the issue of the t-test).
The typo has been corrected, we actually consider changes in annual precipitation.

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

In section 2.1: « In this study, ORCHIDEE is forced off-line either by IPSL_CM4 outputs or by the CRU time series. In the case of IPSL_CM4 forcing, we use the high-frequency outputs (time step = 6h) for the following variables : temperature, precipitation, specific humidity, wind, surface pressure, short-wave and long-wave radiations. For the forcing from CRU, we use monthly values and activate the weather generator of ORCHIDEE, described in Krinner et al, 2005. »

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

This has been done, we just wanted to make the difference between the dynamic and static simulations.