Answers to Anonymous Referee #3

We wish to thank the referee #3 for his/her time and care in providing comments on our manuscript. We provide detailed answers below (answers are in bold):

In their study, Ringeval et al. present new approaches for wetland and CH4 emission modelling during the LGM and a generic D-O event. They compare two models of different complexity, and highlight sensitivities in paleoclimate modelling in general. The manuscript is suited for a publication in cp, and I support a publication after addressing a few points of improvement.

General points:

i) The orbital configuration i.e. the distribution of insolation at different latitudes is hypothesized to be an important factor for CH4 emissions at glacial-interglacial transitions and D-O events (Flückiger et al., 2004; Loulergue et al., 2008; Guo et al., 2012). Especially for D-O events the actual precession cycle regulates the amplitude of atmospheric CH4 concentration changes (Flückiger et al., 2004). An explanation and discussion of the actual setting used in the simulations is needed for the understanding of the simulated CH4 changes. In addition, are the orbital parameters only used for the climate simulations or are they also used to modify the light competition in plant growth for the two DGVMs?

The boundary conditions (seal level and ice sheets extents, greenhouse gases atmospheric concentration and orbital configuration) used to force FAMOUS during the LGM and the idealized D-O events are described in Hopcroft et al., 2011. The boundary condition follows the Paleoclimate Model Intercomparison Project protocol (Braconnot et al., 2007, http://pmip2.lsce.ipsl.fr/). To simulate the climate fields used in the present study, FAMOUS has been forced with an insolation corresponding to a 21 kyr orbital configuration. This information has been added in the revised version of the manuscript.

Ice sheet extent and sea level (trough their effect on the continental grid-cells available for wetlands) and atmospheric CO2 concentration could also have an effect on the wetland CH4 emissions. Regarding the orbital parameters, the distribution of the insolation at different latitudes has been kept constant as compared to the PI in the two models and thus, is not used to modify "the light competition in plant growth". However, the insolation pattern at the LGM is actually not very different from the modern, so this should not really have any significant effect.

Different FAMOUS boundary conditions in terms of insolation have been used in Hopcroft et al., 2011 in order to estimate the sensitivity of the D-O change in wetland CH4 emissions to the background conditions. This is indicated in the submitted version of the daft at p3099,L16 and p3120,L28. These sensitivity tests have not been performed with ORCHIDEE but this is discussed at p3121,L1.

ii) The terms to describe "changes/decrease/increase/transition" are inconsistent in the paper. In general they are associated with a time information running forward towards present. I thus suggest to modify terms like "LGM-PI decrease" to "PI-LGM increase", which makes it much more easier to follow the logic in comparisons between different time periods. Of course values in figures would change sign and would have to be updated. In addition, when comparing models the term "difference between model A and B" is better than "decrease between model A and B". See also specific comments to this point.

In the revised version of the manuscript, we made more consistent the different formulations used to describe the difference in wetland CH4 emissions between LGM and PI (e.g. through the use of "decrease in xx as compared to xx"). However, we would like to keep using the "LGM-PI" difference (instead of "PI-LGM") because this difference is employed in most of the previous publications quoted in the discussion of the current study: e.g. in Weber et al., 2010, JGR; in Valdes et al., 2005, GRL ("The cooler, drier LGM climate decreased the global extent of wetlands in comparison with the PI simulation") and more important (through the strong link with the current study) in Hopcroft et al., 2011 (e.g. in their Fig.2). Thus, we would like to keep using LGM-PI decrease/reduction/increase/change in the revised version of the manuscript.

We suppressed terms like "decrease between model A and B" in the revised version.

Specific points, suggestions for revision:

p3096, l16, l17, l20: use "glacial-interglacial" consistently in the MS

We modified the manuscript to use everywhere "glacial-interglacial" to be consistent to what has been described above.

p3097, l26: there are earlier studies for northern wetland emissions by vanHuissteden et al., 2004 and Berritella et al., 2011, although not in a transient simulation.

We added the words "transient" as well as "global" in the revised version of the manuscript (vanHuissteden et al., 2004 and Berritella et al., 2011 focused both on European wetlands).

p3098, l17: "PI-LGM difference" instead of "LGM-PI transition" as it is not a transient run.

We removed the "transition" term: in fact, no transient runs between LGM and PI have been performed.

p3102, l1: replace beginning with "In order to prevent ..."

done

p3102, l5: Is the wetland fraction of 4% of global area also recalibrated for LGM conditions?

The calibration has been done using a 1° resolution-simulation forced by CRU climatology against Prigent et al. data. The calibration has not been done under LGM conditions given the fact we have no information about the wetland extent at that time. We made the new version of the manuscript clearer about this:

"Briefly, the parameterization consists in a shift of the topographic index distribution in each grid-cell. The shift value is the same for all grid-cells and has been optimized to simulate a current global wetland fraction close to 4% (Prigent et al., 2007) at 1°-resolution when forced by the 1960-1991 CRU (http://www.cru.uea.ac.uk/) climatology. The reader is referred to (Ringeval et al., 2012) for more details. The coarse resolution effect on the wetland extent simulation through the coupling between ORCHIDEE-WET and TOPMODEL is illustrated in Fig. A2…"

p3102, l11-12: repeting sentences with repeated citations is not necessary, please simplify.

We made this sentence simpler in the revised version.

p3103, l10: Is the WTD calculated monthly or annually? Please clarify.

The term "continuously" has been replaced by "at monthly time scale" in the revised version.

p3105, l5: "CH4" instead of "CH40".

We replace CH40 by D0 to be consistent with the Equation 1.

p3108, l2: Please indicate the turnover time of the labile carbon pool: is it in the porder of 1 yr, 10 yrs, 100 yrs?

This information (55 days) has been added at p3102, L20.

p3109, l12: With the publication of Baumgartner et al., 2012, the Dällenbach et al. 2000 data and conclusions for LGM have been updated and partially proven wrong. Thus cite Baumgartner et al., 2012 here.

Done

p3110: What is the role of lower CO2 during the LGM, are both models equally sensitive to CO2 levels? You mention CO2 effects on p3119, l5: is wetland NPP equally reduced in the two models?

a) CO2 .vs. climate contribution to wetland CH4 emissions

In the two models, the CO2 could have an effect on both the wetland extent (lower CO2 leads to higher stomatal conductance which leads to decrease the modeled soil water content and thus lower wetland extent) and on the substrate through fertilizing effect on NPP. The contribution of CO2 .vs. climate to the LGM-PI change in wetland CH4 emissions have not been evaluated with the ORCHIDEE model. In SDGVM, the climate explains 61% of the difference between LGM and PI while the CO2 effect explains the remaining 39%. We clarified this in the text of the revised version.

b) CO2 effect on NPP

The change in NPP simulated by ORCHIDEE for two groups of PFTs is given in Fig A3. In particular, we focused on the ORCHIDEE-simulated reduction in boreal NPP due to higher vegetation moisture stress (see p3111). This explains the large decrease of boreal substrate obtained in ORCHIDEE and not simulated by SDGVM. However, no sensitivity runs have been performed to separate the NPP reduction due to lower CO2 and the NPP reduction explained by the climate with ORCHIDEE.

Nevertheless, this is discussed in Woillez et al., 2011 (while freeze/thaw processes are not accounted for in the version used in the latter study). Right column of Fig 15 in Woillez et al., 2011 shows the difference in NPP for LGM vegetation between one simulation performed with [LGM climate, PI CO2] and one simulation with [LGM climate, LGM CO2]. They showed in particular that the lower CO2 leads to 60% reduction in NPP for tropical trees. In the revised version of the manuscript, we refer the reader to the Fig. 15 of Woillez et al.

Regarding the wetland CH4 emissions, note that the proxy of substrate is even more important than the NPP itself (while the both are not independent). Change in proxy for the substrate in the two models is given in Fig. 6.

p3110, l13: which figures?

The sentence concerns the Figures 5 and 6. This has been clarified.

p3111, l6+l7: change is from LGM to PI, so it should be an increase?

Please, refer to the answer to the 2nd comment. The different formulations used to describe the difference in wetland CH4 emissions between LGM and PI have been clarified.

p3111, l21: again isn't it an decrease from LGM to PI

Please refer to the previous answer.

p3111, l12: correct sentence: "The substrate sensitivity to climate change between LGM and PI explains the different behavior of the two models."

This sentence has been removed (in order to clarify the corresponding section according to one comment of reviewer #1).

p3112, l5: singular: "event"

done

p3112: Please mention already here that changes in CH4 emissions of 25%-50% are expected from ice core data and that you will discuss this in section 4. So, one knows that simulations are underestimating emission changes during D-O events.

We do not really agree with the reviewer here: we would like to restrict the comparison with the concentration inferred from ice cores to the Section 4. In particular the contribution of wetlands to D-O events is still open for debate (as it is suggested by reviewer #2). We estimate that giving this range (25-50%) in the section 3 makes the passage more difficult to understand for the reader.

p3113, l16+l18: change to "... we computed the annual CH4 emission anomalies ..."

done

p3113, l23: use different words for "plot areas"

Done

p3113, l27: symbol in brackets is unknown, it is not clear that it refers to figure 8.

This has been clarified.

p3117, l9: Baumgartner et al., 2012 find that the interpolar difference was bigger than estimated previously in Dällenbach et al., 2000, but this does not necessary mean that boreal wetland CH4 emissions were very active. It could also mean northern low latitude wetland emissions were productive.

We agree with the reviewer. However, in the manuscript, we focused only on the interpolar difference given by Baumgartner et al., 2012 because it is more accurate than the value given by Dallenbach et al. (In fact, the Baumgartner et al., 2012 study is based on higher resolution record which allows a better synchronization between the Greenland and Antarctica ice cores.)

However, as the reviewer (as well as reviewer #2) let suggest, the rIPD has to be taken with caution in particular because its computation is based on the split of the Earth into only two boxes (corresponding to the two hemispheres). It is now discussed in the final version of the (Baumgartner et al., 2012) study. We have already demonstrated how sensitive the rIPD value is to assumptions about the limits of the two-boxes in the submitted version of the draft (see p 3117 line 24). New sentences have been added in the revised version of our manuscript:

"The rIPD value given by the equation (4) has to be taken with caution because only two source regions (corresponding to the two hemispheres) are considered. The two-box split does not therefore account for the basic atmospheric circulation patterns (e.g. Hadley cells) nor does it allow separation of emissions from boreal wetland and northern low latitudes."

p3117, l15: Please note that parameter values for Eq. 9 might change from the BGD to the revised BG version of Baumgartner et al., 2012. Please adapt accordingly to be consistent with the formulation.

We updated both the text and Fig. 9 according to the final version of Baumgartner et al., 2012.

p3131, Fig. 2: Were the LGM ORCHIDEE-WET distributions corrected to SDGVM PI or LGM?

PI and LGM ORCHIDEE emissions have been multiplied by the ratio PI SDGVM/PI ORCHIDEE. This has been clarified into the Figure 2's caption by using the term "scaling":

"Each PI ORCHIDEE-WET latitudinal distribution has been scaled to match the SDGVM PI global emissions. The same scaling factor has been applied for each LGM ORCHIDEE-WET distribution."

p3132, Fig. 3: differences as PI-LGM would be much more intuitive in terms of chronology.

Please, refer to above answer about the LGM-PI difference formulations.

p3137, Fig. 8: "dividing" instead of "divinding".

This has been modified

p3142, Fig. A3: To my knowledge C4 grasses are more competitive at lower CO2 concentrations as during the LGM. Thus I'm surprised to see C3 grasses dominating the PFT distribution during the LGM. Can you confirm that this is the case?

Fig. A3-b shows the distribution of the main supra-PFT class regarding the *maximum potential coverage*. This map is used as input of the ORCHIDEE model. In this map (coming from simulations performed and described in Woillez et al., 2011), potential maximum coverage for C3 and C4 are very close to each other in the tropics. This leads to confusion when the main supra-PFT class is plotted in the Fig. A3.

The aim of the Fig.A3-a/b was to show that the prescribed maximum vegetation does not strongly change in the boreal regions and thus, the change in boreal NPP is more related to a change in NPP flux density per vegetation type rather than by a change in vegetation coverage.

To prevent any misunderstanding when the reader looks at the tropics in Fig A3, we put together C3 and C4 in the same meta-class when the *maximum potential coverage* is plotted. Besides, we added in the same Appendix a new figure (please, see the Figure attached to these answers) which gives the *annual maximum simulated coverage* (which depends on NPP) during PI and LGM. In this Figure, C3 and C4 are separated and, as underlined by the reviewer, C4 are more present than C3 in tropics during LGM.