Answers to Anonymous Referee #2

We wish to thank the referee #2 for his/her time and care in providing comments on our manuscript. We provide detailed answers below (answers are in bold):

General comments:

This paper uses two wetland CH4 models of differing complexity to look at wetland methane emissions during the LGM and an idealized Dansgaard-Oeschger (DO) event. The differences in model response are investigated in terms of the model parameterizations and comparisons are made between the model results and previous bottom-up and top-down estimates for the LGM and some DO events. Much of the focus of the paper is on the influence of model parameterizations between SDGVM and ORCHIDEEWET.

I have some concerns about this manuscript that I would like to see addressed.

First, I have some difficulty in understanding the approach given to some variables in Equation 2. In equation 2, a Tref value is derived for the ORCHIDEE model run (V0). The value is defined for that model version as the mean surface temperature computed by ORCHIDEE-WET when forced with the 1960-1991 CRU climate. In other model versions (V1,V2, and opt), Tref is set to 30 degrees C everywhere. If this Tref is intended to represent some sort of local adaptation of the Q10 formulation for local conditions and climate, does this make sense to parameterize it for modern CRU climate for a study where all runs are performed for paleoclimate conditions? To my mind, the value of Tref should be different during the LGM (assuming it represents an adaption for conditions) than modern, thus parameterizing it for modern conditions does not make sense. Perhaps the authors just need note that the approach is intended for modern conditions and just applied as-is to the paleo?

Indeed, the current manuscript is the 1st study where the ORCHIDEE-WET model is used for paleo time periods. For studies relative to non-current time periods (i.e. for both future - Ringeval et al., 2011, Koven et al., 2011- and paleo), our strategy has been to keep the optimization realized under the current time period. We explicitly state this in the revised version of the manuscript.

"The strategy followed here has been to perform the optimization under the current time period then to apply this model configuration in conditions representative of the LGM and the idealized D-O event."

More than the value used for Tref (e.g. 30°C in V1 and V2; please, cf. also next answer), the thing pointed out by the reviewer concerns the fact that Tref is, for a given ORCHIDEE version, the same for the PI and the LGM run. However the Tref does not change in SDGVM between the PI and LGM. And as explained in the Methods section, the aim of V1 and V2 is in particular, to estimate the contribution of different parameterizations to potential differences between ORCHIDEE-V0 and SDGVM. However we agree that some sensitivity tests about Tref would be interesting for the ORCHIDEE-Vopt version whose the purpose is to give our best estimates of the wetland CH4 emissions change in order to compare with the atmospheric concentration data. A sentence in the discussion already underlined this (p3118, l19). We added a sentence to strengthen this point in the revised version of the manuscript. Nevertheless, we would like also to underline that there is

still a debate about how influential a microbial community temperature adaptation would be for soil organic matter mineralization.

"Additional sensitivity tests with a time-variable Tref as in (Ringeval et al., 2012) could be performed to evaluate the effect on the simulated change in CH4 emissions between LGM and PI. However, there is still a debate about how influential a microbial community temperature adaptation would be for soil organic matter mineralization. E.g., Rousk et al., 2012 showed that a change in the microbial community (i.e. an adaptation) would be minor as compared to the direct effect of temperature on microbial activity and the indirect effect on the quality of the soil organic matter. Besides, discontinuity in the mineralization sensitivity to temperature around 0°C (Koven et al., 2011) could have a strong effect on the LGM-PI change in emissions and calls for additional tests."

Second, the same equation has a parameter, $\alpha 0$, which represents both the fraction of labile C pool that can be used as a methogenesis substrate and a tuning parameter for Tref that is optimized against 3 field sites. For each model version, α 0 is retuned alongside each change in the other parameters. I find this to confuse the influence of changes in the other parameters (which are the only ones that are discussed). For example, changing between model version ORCHIDEE-WET V1 and V2, the important changes for the sensitivity test is to set the soil water to the maximum, to remove any water stress on the vegetation growing in wetlands. The purpose of this test is to understand if a model shortcoming (the lack of PFTs that grow only in wetlands, thereby only being influenced by wetland hydrology – not a mix of upland and wetland) has a strong influence on the model result. However, the values of 0 change from 5.5,8.5, 20.1 (V1) to 6.9, 5.4, 24.5 (V2) for the different latitudinal bands; making it difficult to understand what is the primary influence – the lack of water stress on plants or the changes in $\alpha 0$?

We will answer to this comment at 3 different spatial scales.

α_0 is chosen at site scale for V0 and V1 (cf. Table 1). For each version, α_0 has been chosen to reduce the mismatch between the observed and modeled seasonal cycle of emissions on 3 sites (Abisko, Michigan and Panama). As underlined at line 15 of page 3107 in the current draft, the mismatch between the observed/simulated seasonal cycles is bigger with V1 than with V0 because the Q10 in V1 is low. However, for each site, from V0 to V1, the change in the difference between the modeled/observed annual emissions does not exceed 15% of the observed annual emissions.

α_0 is then extrapolated to large latitudinal band scales. As in (Ringeval et al., 2011), identification of each grid-cell to a wetland type (i.e. sharing the same optimized parameter as Abisko, Michigan or Panama) is based on a criteria of vegetation type (this has been clarified in the revised version of the manuscript). These three latitude bands are called hereafter L_{Abisko}, $L_{Michigan}$, L_{Panama}. The assumption made regarding this extrapolation differs between V0 and all others versions (V1,V2 and Vopt) because Tref varies in space in V0 and is constant for all other versions. In fact, a classical Q10 formulation would be: Prod(Tref).$Q_{10}^{((T-Tref)/10)}$ where Prod(Tref) is the base methanogenesis rate at Tref. Instead of this formulation, only the $Q_{10}^{((T-Tref)/10)}$ term is kept in Walter and Heimann, 2000 and in the current manuscript. As described at p3102,L22, the parameter $\alpha 0$ contains two informations: "both the fraction of the labile carbon pool which could be used as methanogenesis substrate and the base rate at Tref". In V0, the implicit assumption that the base rate at Tref varies with the amount of substrate is implicitly made. This could lead to

difference in the spatial distribution of emissions into a given latitudinal band (L_{Abisko}, $L_{Michigan}$ or L_{Panama}) between the ORCHIDEE versions. This has been clearly stated in the revised version of the manuscript.

Finally, the amount of α_0 change from one ORCHIDEE version to the other one is not the same for the 3 big latitude bands (L_{Abisko}, $L_{Michigan}$ or L_{Panama}). This could contribute to modify the contribution of each latitude band to the global signal. And as underlined by the reviewer, this could make difficult to understand what the primary influence of the change between the versions is. To give estimates of the sensitivity to this, we applied *a posteriori* different correcting factors to obtain in each version exactly the same contribution of each big latitude band (L_{Abisko}, $L_{Michigan}$, L_{Panama}) to the global PI emissions as in V0. The LGM – PI difference has been computed using such "correcting" emissions and are given in green in the Table 2 (these numbers are now in the revised version of the manuscript):

LGM – PI (%)	SDGVM	ORCHIDEE-WET - V0	ORCHIDEE-WET V1	ORCHIDEE-WET V2	ORCHIDEE-WET opt
Global	-46%	-67%	-51% -50%	-32% -35%	-36% -38%
>30°N	-41%	-87%	-75% -73%	-52% -64%	-45% -58%
30°S-30°N	-48%	-57%	-39% -39%	-25% -23%	-32% -30%

We concluded this *a posteriori* "correction":
- leads to no significant changes in the simulated *global* LGM – PI differences whatever the version is
- tends to diminish the difference regarding the LGM - PI change for the >30°N band between V0,V1,V2 and Vopt. Thus, this reinforces the risen conclusion in the submitted version of the draft (p3109, L25): "the decrease in the > 30°N region is higher than the one in 30°S–30°N whatever the ORCHIDEE-WET version and in contrasts with SDGVM". This is even truer with the newly given numbers. Thus, we conclude the difference in the wetland CH4 emissions between the ORCHIDEE versions could not be attributed to change in α_0 values.

Third, the analysis of idealized DO event presents some problems. The authors use a two-box model to determine the relative interpolar CH4concentration difference (termed rIPD). While I understand that the rIPD approach was taken from Baumgartner et al. (2012), I do not feel it is a realistic approach. Splitting the atmosphere into two separate boxes at the equator ignores the most basic atmospheric circulation patterns (a threebox model would at least allow a rough approximation of Hadley cells). The authors do alude to this by demonstrating how sensitive the rIPD value is to assumptions about the limits of the two-boxes (see p 3117 line 24). Given the uncertainties with this calculation, I would prefer to see it left out.

In the submitted version, many sentences already showed the rIPD computation has to be taken with caution:

"Given the uncertainties linked to the latitudinal change of emissions, it is not possible to unambiguously discriminate between SDGVM and ORCHIDEE-opt." (p3117,L26)
"However, it should also be noted that a 2-box model of the global CH_4 sources and atmospheric mixing may not discriminate adequately between the tropical and boreal source regions. This limitation will need to be addressed in future work." (p3121, L25)

Moreover, as underlined by the reviewer, we already demonstrate how sensitive the rIPD value is to assumptions about the limit of the two boxes (p3117 and error-bars in Fig. 9):

"However, the value of rIPD is very sensitive to a small difference in s_n and s_s. This is underlined on Fig. 9a by the error-bars that give the range of rIPD for SDGVM and ORCHIDEE if 25% of the closest grid-cells of South Hemisphere to the Equator are accounted for in sn instead of into s_s (or vice-versa)."

However, we think it is still additional value to discuss about comparison with inter-hemispheric gradient and to put our current results in regards to the most recent literature about the ice core data. Note also that this problem is now discussed in the final version of Baumgartner et al. study (see the new section 4.2 in the final accepted version of the publication: "Two-box model versus three-box model"). That's why we would like to keep the discussion about the rIPD. However, we add a new sentence to further underlines the uncertainties around the rIPD computation that we used:

"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. However, it has the advantage of allowing a simple analytic computation of the rIPD (Baumgartner et al., 2012)."

Fourth, the grid cells of the models are assumedly the same as the FAMOUS climate data (5 degrees x 7.5 degrees). At such a coarse resolution, I have some concern about how realistic the treatment is for the land exposed on the continental shelves due to low sea level. The approach of taking the same topography of the nearest land cell for newly exposed land would be possibly okay if the cells were small, but at this large resolution, I wonder if these (possibly) flat continental shelf regions are given far rougher terrain than in reality, thus biasing the model results low at the LGM due to less area suitable for wetlands. I would like to see some demonstration that the very large grid cells don't create artifacts resulting in lower than reasonable area suitable for wetlands.

From PI to LGM, the land area increases following decrease in sea level. A new land/sea mask is used during the LGM run as compared to the PI simulation. Thus, new information about the sub-grid distribution of topographic index is required. Two distinct cases could happen as explained at p3106,L15:

- **For the grid-cells for which the continental fraction increases from PI to LGM, the same statistical variables as for the PI are used to extend the sub-grid distribution to the new land part of the grid-cell.**
- **For entirely new grid-cells under LGM conditions, we use the sub-grid topography distribution of the closest grid-cell existing under PI conditions.**

We agree with the reviewer the assumption done in the 2nd case is a strong assumption. However, this case is minor as compared to the 1st one. In fact, e.g. below 40°N, it has to be applied for 20 grid-cells (against ~100 grid-cells for the 1st case). This has been clarified in the revised version. The area of these new grid-cells (<40°N) corresponds to 5 millions of km2. The mean fractional wetland extent at annual maximum over these grid-cells is ~10% and they emit 6.7 Tg/yr during the LGM.

If we recalculate the total wetland flux at the LGM after multiplying by a factor 2 the wetland extent in these new grid-cells, the LGM emissions increase from 89. 9 Tg/yr (as in the current submitted version of the manuscript) to 92.3 Tg/yr. Of course, this factor 2 does not properly allow correcting the potential bias from using the topography of the closest grid-cell. However, it allows estimating the weight of these grid-cells in the total global emissions.

As we already suggested in the manuscript, the best treatment for these new grid-cells would be to use bathimetry information. According to next comments, some sentences have been added in the revised version of the manuscript to temper the results:

"However, the simple treatment used to estimate the sub-grid topography of the new land surface during the LGM (i.e. the extrapolation from nearby land grid-cells) does not allow a comprehensive analysis of the role of coastal shelf regions in LGM-PI wetland CH_4 emissions."

The manuscript also requires a thorough proof-reading as there are numerous typographical and grammatical errors.

The typographical and grammatical errors have been corrected in the revised version of the paper.

Overall assessment:

This paper is well within the scope of Climate of the Past. I think readers will find it of interest. If my concerns can be addressed, I believe the paper is publishable, albeit with revisions. Given my concerns stated above, I obviously have some reservations about this paper, but those aside, I do think it is an interesting paper and well worth seeing through to publication.

Detailed review:

p. 3097 line 3-6: Processed-based wetland CH4models have been around a fair while. While it is subjective, over a decade does not seem 'recent' (e.g. Walter et al. (2001)). However I do agree that a reasonable approximation of LGM climate is only very recent (if indeed we are there yet).

We modified the sentence: "Until relatively recently, bottom-up modelling approaches over D-O events were limited not only by the lack of process-based representation wetland CH4 emissions <u>in Land Surface Models</u>".

p.3097 l 14: Reading Hopcroft et al. (2011), I don't get a sense of how well the LGM climate of FAMOUS has been checked against terrestrial temperature proxies. A recent example for checking FAMOUS against would be Bartlein et al. (2010) although this also suffers from lack of proxies in the tropics where perhaps they are needed most for wetland studies. Most of the discussion relates either the Atlantic meridional overturning circultation (AMOC) or global mean temperature. AMOC is essentially irrelevant to wetland simulations, its influence on the climate is important but we are given no information about the spatial distribution of the climate change. The global change of 4.1 degees C from preindustrial gives an idea. Plotting FAMOUS AMOC and Greenland temperature in Fig A1 do not give relevant information about the conditions the wetland regions are experiencing. While I do understand that the actual forcing data is a secondary concern in this paper, the main being the differences between the wetland models, I think it is relevant to demonstrate how the climate was spatially and how it differs from present. Fig A1 should be replaced with a figure showing a map of anomalies from present day for temp and precip. Fig A3 is somewhat useful for the climate changes, but more is needed.

There are two different things here: is FAMOUS relevant to simulate i) the LGM climate and ii) the climate during D-O event.

About the LGM: as described in Hopcroft et al., 2011, FAMOUS corresponds to a low-resolution version of the UK Met Office's HadCM3. It has been tuned to mimic the climate simulation of HadCM3. A figure is enclosed to this document (Fig. A1). Top row of this Figure will be added in the Appendix of the revised version of the manuscript as well as the following sentences in the section 2.3.1:

"FAMOUS is a reduced resolution configuration of the HadCM3 coupled atmosphere-ocean general circulation model (AO-GCM). The LGM climate of FAMOUS bears many similarities to that simulated by HadCM3 (Singarayer and Valdes, 2010), with a global mean cooling of 4.6C, that is similar to HadCM3 and intermediate in the range of cooling magnitudes simulated with other coupled AO-GCMs analyzed in PMIP2 (Braconnot et al 2007a,b). The simulated cooling is more intense over Greenland than in HadCM3, in better agreement with ice-core reconstructions. It also shows warming over the Northern Pacific and Alaska, the latter in reasonable agreement with terrestrial pollen-based mean annual temperature reconstructions (Bartlein et al 2011). Over the tropics where proxy-based reconstructions are sparser, differences with HadCM3 are less pronounced, though regional differences in the patterns of the change in the ITCZ and hence precipitations are prominent, particularly the eastern Pacific and South America."

For the climate relative to the D-O, we now refer the reader to Figure 5 of Hopcroft et al., 2011 which gives anomalies of precipitation/T as compared to LGM climate fields. Many uncertainties remain about the climate of the D-O and in particular as explained by Hopcroft et al., 2011 about the underlying climatic drivers of D-O variability. This has already been underlined in the submitted version of the draft (p3122, L2)

p. 3101 l 10: How was -3 cm chosen?

As explained at p3102, L10, the wetland extent with -3cm are taken as extents given by TOPMODEL with a deficit between 0 and −6 cm. These extents will be combined with the CH_4 flux densities computed for a WTD=-3cm to approach the emissions of the non-saturated wetland. As showed by

Christensen et al., 2003, the water table act more as an "on-off switch". Also, in the ORCHIDEE model, the CH4 flux densities decrease quickly as soon as the WTD is low below the soil surface; i.e. the relationship between CH4 flux densities .vs. WTD is not linear.

That means we need to have a small space-step in our methods to be able to approach the mean CH4 flux densities over a range [x,y] of WTD by the CH4 flux densities computed for a WTD= (y-x)/2. That's why the [0,-6cm] has been chosen here.

Christensen, T. R., A. Ekberg, L. Stro¨m, M. Mastepanov, N. Panikov, M. Oquist, B. H. Svensson, H. Nykänen, P. J. Martikainen, and H. Oskarsson, Factors controlling large scale variations in methane emissions from wetlands, Geophys. Res. Lett., 30(7), 1414, doi:10.1029/2002GL016848, 2003.

l 15: Does the model include canopy drip?

If the water intercepted by the canopy is larger to a given threshold (maximum capacity of intercepted water; defined for each PFT), the water goes to the soil surface.

p. 3103 l 15: 2m air or soil temperature?

It corresponds to the surface air temperature; this has been clarified in the revised version.

l16: Why above 5 degree C? Is this the monthly, annual, mean, max value? Please specify.

This has been specified: "the monthly surface air temperature above 5°C according to Fung et al., 1991".

p 3105: How is the soil texture of the continental shelves handled? I repeat my concern as stated in the general comments that this approach of using the neighbouring cell is not valid for such large grid cells (assuming the soil texture information was treated the same as topography).

The soil texture is used in ORCHIDEE to compute the potential soil water content. The soil texture used by ORCHIDEE has been made consistent to the soil texture maps used as input of SDGVM. The soil/silt/clay fractions are derived from a lookup table and using ISLSCP1 soil textures, which have been expanded out to the LGM mask using interpolation.

p. 3106 l 21: So how does the bathymetric information differ? I would like to see a discussion of how this would influence the results as I am not convinced the present approach is appropriate.

Please see the answer to the 4th major comment.

p. 3107 line 12: I do not understand why $\alpha 0$ would require a new optimization for each change in Tref or Q10. If it is indeed required (please add in some further justification), why is it then required between V1 and V2? The values of Tref or Q10 remain the same between these model versions. As I note in General Comments, this just confuses the impact of the main change in V2. On page 3108 l. 7, apparently this was not a re-optimizaton but a correction. What is the difference and why is it needed?

As described at p3102,L22, the parameter $\alpha 0$ contains two informations: "both the fraction of the labile carbon pool which could be used as methanogenesis substrate and the base rate at Tref". This explains why when the simulated carbon pool changes (e.g. from V1 to V2 due to the fact that

the water limitation on the soil carbon degradation is removed), the $\alpha0$ needs to be re-optimized even if Tref and Q10 are the same (as in V1 and V2).

Instead of doing a new optimization on site, we just applied a correcting factor equal to the ratio of carbon between V1 and V2 to estimate the new value of $\alpha0$ for V2. In fact, as explained in the current version of the manuscript (p3108, L7), the saturated conditions will have an effect on the CH4 flux densities mainly through the change in substrate (in all versions, the computation of the CH4 flux densities is done using a WTD situated at the soil surface).

Please refer also to the answer of the 2nd major comment.

p. 3108 l. 10-12: I would like to see a plot showing how the influence of constant soil field capacity conditions is more through effect on substrate than methanogensis or transport. This is interesting in its own right.

We think there is a misunderstanding here. Indeed, in ORCHIDEE-V2, the soil water content is prescribed during the computation of the methanogenesis substrate. However, whatever the ORCHIDEE version is (V0,V1,V2), the value of the water table used to compute the CH4 flux densities (i.e. to estimate in which soil layers there is production/oxidation, what are the different kinds of transports, etc/) are the same (cf. equation 1, WTD=0 and -3cm). We clarified this in the revised version (we modified p3107,L19 and added a new sentence).

p. 3108 l 20: I wouldn't say an 'over-estimation', these values are very poorly constrained and thus very much open to debate. Look at how much trouble we have with modern estimates (Denman et al. , 2007; Melton et al. , 2012).

We agree with the reviewer. That's why we used the term "<u>apparent</u> over-estimation" and we wrote: "This underlines the uncertainty linked to the contribution of the wetlands to the global CH4 budget (Kirschke et al., 2012)." (p3109, L1). We have now added references to Denman et al., 2007; Melton et al., 2012.

p. 3109 l 9: I think this section deserves more comment on the influence of using just one modelled climate (with no information given on how well it compares to proxies).

We already discussed the fact that only one modelled climate has been used in the Discussion section (p3116,L12). We have added information about the FAMOUS LGM climate in the revised version of the manuscript. Please refer to previous answer about the FAMOUS LGM climate.

p. 3110 second para: How much land gets added at such large grid cell sizes? Do you allow for fractions of grid cells to be added due to newly exposed land at LGM? I would suggest that the discussion of how much exposed continental shelves contribute to wetland emissions should be tempered by how realistic the treatment was of the exposed shelves within the model.

Please see the answer to the 4th major comment.

p. 3111 line 19: No discussion is made of the change to vegetation in SDGVM, which were dynamic. Could this have much influence on the results compared to the prescribed PFT distributions of ORCHIDEE-WET. Please discuss this.

Again we think there is a misunderstanding here: the problem relative to the static vegetation in the ORCHIDEE model concerns only the transient D-O runs. In fact, differences in PI/LGM vegetation distribution are accounted for by prescribing HYDE0.3/Woillez et al.,2011 vegetation maps as input of the ORCHIDEE model.

During the idealized DO event, the global GI-HS change in NPP simulated by SGDVM reaches 15% of the LGM NPP (see Fig.4 of Hopcroft et al., 2011). It seems that the majority of the NPP changes in SDGVM are driven by changes in productivity, particularly in the tropics, whilst dynamic shifts in vegetation appear to have a smaller impact.

A sentence has been added in the section 3.2.2: "Besides, (…) the accounting for the dynamic in vegetation in SDGVM during the D-O run appears to have a small impact on the change in productivity and could not explain differences between the two models."

However, note also that longer persistent changes in the freshwater forcing could induce further changes in forests as compared to the ~100-200 year timescales examined here.

p. 3112 l 11-15: Confusing, please reword.

Done: "The amplitude of the change in wetland CH_4 emissions between the warm and cold periods of the D-O is very similar between the two models (SDGVM and ORCHIDEE-V0, Figure 7 top panel). During the cold period of the D-O (HS), the wetland CH_4 emissions are 5.7% lower in comparison to LGM values for SDGVM, while this decrease is of 3.3% for ORCHIDEE."

p. 3113 l 18-26: Confusing, please reword.

Done: "We have carried out sensitivity analysis to assess the contribution of wetland extent .vs. CH_4 flux density to the difference in CH_4 emissions between LGM, HS and GI. In each latitude band, we computed the annual CH_4 emissions anomalies relative to the mean global LGM value for the HS and GI periods using the simulations described above. These anomalies are called VAR in the following. We have also computed the annual CH_4 emissions anomalies in the case where the wetland extent is prescribed and equal for each grid-cell to its mean LGM value in the respective ORCHIDEE and SDGVM simulation. These CH_4 emissions anomalies are denoted as FIXED. Figure 8 displays scatter plots of FIXED against VAR. In Figure 8, the two triangles delimited by the X-axis and the 1:1 line encompass model behavior in which both the flux density and the wetland extent anomalies have the same sign. In these triangles, the closer a given point is to the X-axis, the higher the contribution of wetland extent in the emission anomaly. For points that fall outside of these two triangular areas, the models are showing competing influence of wetland area .vs. CH_4 flux densities, namely whilst one is acting to increase the net CH_4 emissions, the other is acting to cause a reduction."

p. 3114 l 16: Please do not use this form '(respectively ...)'. It is consistently confusing, unnecessary, and much simpler to just write the sentences like: 'the sourthern tropical band is characterized by an increase in CH4emissions, while the northern band sees a decrease'.

Ok. This has been corrected in many places in the revised manuscript.

p. 3116 l 25: The match at the top end of the range from Weber et al. (2010) is pretty unsurprising as ORCHIDEE-WET was optimized to be within the range.

The LGM – PI decrease (in percent of PI emissions) simulated by ORCHIDEE was not optimized. The ORCHIDEE LGM decrease is compared to the SDGVM decrease in the Table 2 for instance and during a long part of the discussion.

p. 3117: My objection to the two box approach is in the general comments. I think this part of the discussion is too uncertain to add to the paper and could be removed.

Please see our answer to the 3rd major comment in which we addressed this problem.

p. 3118 l 22: I think the authors place a very large emphasis on the Q10 formulation. Much of the discussion focuses on this one parameter. I think that it is likely worthwhile to discuss other parts of the model parameterization rather than putting such weight on this one parameter.

We modify the Q10 term to "sensitivity to temperature" because we modified not only the Q10 but also the Tref (please see the previous answers). The emphasis about the sensitivity to temperature is from the difference between V0 and V1. Nevertheless, we added a sentence to explain that many others parameters are still uncertain and in order to temper the emphasis on the Q10 formulation:

"Others processes are also relatively simply represented in the models (e.g. the constant oxidation related to the plant-transport of CH_4 in ORCHIDEE) and could have an effect on the simulated LGM-PI change in wetland CH_4 emissions."

p.3119 l5-10: How did the influence of CO_2 versus soil freezing get quantified? Are those results presented?

a) CO_2 .vs. climate

In the two models, the CO_2 could have an effect on both the wetland extent (lower CO_2 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 CO_2 .vs. climate to the LGM-PI change in wetland CH_4 emissions have not been evaluated with the ORCHIDEE model. In SDGVM, the climate explains 61% of the difference between LGM and PI while the CO_2 effect explains the remaining 39%. We clarified this in the text of the revised version.

b) CO_2 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 substrate obtained in ORCHIDEE and not simulated by SDGVM. However, no sensitivity runs have been performed to separate the NPP reduction due to lower CO_2 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 CO_2] and one simulation with [LGM climate, LGM CO_2]. They showed in particular that the lower CO_2 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 CH_4 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.

p. 3119 l 24-25: But inundation datasets have problems of their own, e.g. discussion in Melton et al. (2012).

We agree with the reviewer that inundation datasets have problem of their own. However, we wanted to mean here that the global wetland extent should be scaled to the global wetland extent given in inundations datasets. Melton et al., 2012, Table 2 gives the mean annual <u>maximum</u> wetland extent. We have clarified the sentence in the manuscript.

p. 3120 l 14: It is possible that the value of 15 Tg yr-1 is reasonable. For the Younger Dryas termination (if that can be assumed similar to a generic DO event), Melton et al. (2012b) estimated that tropical wetlands would increase a maximum of 14 Tg for a global increase of 64 Tg yr-1, with a very minor increase in boreal wetlands. My point being that the contribution of wetlands to DO events is still pretty open for debate.

We agree with the reviewer and added a sentence:

"However the contribution of wetlands to D-O events is still open for debate. For instance, Melton et al., 2012 estimated that tropical wetlands would increase a maximum of 14 Tg/yr for a global increase of 64 Tg/yr in case of the Younger-Dryas termination, though this is not usually considered a Dansgaard-Oeschger event."

–Did SDGVM use it's N-cycle components? If so, how would that influence the results compared to ORCHIDEE-WET (which I believe has no N-cycle)?

In fact, the used version of ORCHIDEE does not incorporate *explicit* N cycle (as developed in Zaehle and Friend, 2010, GBC). However, as described in Krinner et al., 2005 (GBC), the role of nitrogen is represented implicitly in the photosynthesis limitation (using an exponentially decreasing function of canopy depth) and carbon allocation (parameterizing the nitrogen limitation as a function of soil humidity and soil temperature).

SDGVM includes the Nitrogen-cycle as described in the CENTURY model. To our knowledge, there is no study separating out the affects of the N cycle in SDGVM. In addition, no studies focused on the effects of the N-cycle at the LGM or for CH_4 emissions. We considered that assessing this is beyond the scope of the current work.

p. 3122 l. 13: Yes, I agree that flood plain processes are needed. There is a recent paper that models groundwater contribution to wetlands in the Amazon that would be worth refering to here (Miguez-Macho and Fan , 2012).

We added this reference in the revised version: "These processes are particularly relevant in regions such as the Amazon basin (Miguez-Macho and Fan , 2012)."

Table 2: Confusing caption, please reword

Done: "Table 2: PI, LGM and LGM-PI wetland CH_4 emissions for SDGVM and ORCHIDEE. In the top row, the global PI and LGM emissions are given in Tg/yr. For ORCHIDEE-WET, the 1st number in brackets corresponds to the emissions from saturated wetland while the 2nd number refers to the emissions from non-saturated wetlands. In the bottom row, the LGM – PI change (in percent) is given for different latitudinal bands."

Fig 1: The multiple WTD arrows for SDGVM make it look like there are multiple WTPs, not a variable one, perhaps revise to make it less confusing.

This has been corrected.

Fig 2: What is mean that each PI ORCHIDEE-WET lat distribution has been normalized to match SDGVM PI global emissions? This point might have been lost in the text but I think it is interesting that changes from ORCHIDEE V0 to V2 are of opposite direction for the boreal region than the tropics. Has this been noted and discussed in the text?

According to the comment of reviewer 1, we modified the caption of the Figure 2.

"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."

We added a sentence about the difference of latitudinal distribution between V1 and V2 ("This leads to a modification of the latitudinal distribution of the wetland CH_4 emissions as compared to V1 and in particular to lower boreal emissions (cf. Fig.2).")

Fig 3: Please add plots of the climate changes between LGM and PI so there is something to reference against.

Please refer to previous answer about the FAMOUS-simulated LGM climate.

Fig 8: What does 'SDGVM -non divinding by f_wtp' mean? Please move the legend out of the top left box and into the open space bottom right. Please darken the yellow to make it easier to read.

According to this comment and the comment of reviewer#1, we modified the mention relative to f_wtp in Fig 8 and the corresponding caption by adding the following sentence:
"The SGDVM plot in the top right corner corresponds to emissions from saturated wetlands alone while the left plot represents emissions from all kinds of wetlands. The saturated wetland emissions have been approached by using the simulated CH4 flux densities divided by f (WTD) (cf. the end of the section 2.2.2 and Equation 3)."
We have moved the legend and darkened the yellow symbols in the revised version.

Please give a thorough check of the MS for typos, grammatical errors, and general flow. I do not list them but they are extensive.

This has been done.