Answers to Anonymous Referee #1

We wish to thank the referee #1 for his/her time and care in providing comments on our manuscript. We provide detailed answers below (answers are in bold):

In their manuscript, Ringeval et al. investigate the response of wetland methane emissions from the ORCHIDEE-WET model to simulated Dansgaard-Oeschger and Heinrich events and compare the response to previously published results obtained with the Sheffield SDGVM.

Their manuscript provides substantial insight into the causes of the different responses of the two models to identical climate forcing, thereby facilitating the further improvement of models for wetland methane emissions. In large parts the manuscript is well written, though some passages could be formulated clearer. I therefore recommend publication with minor revisions.

Overall I do not have any major points for improvement of the manuscript, though several minor points do remain.

In a substantial number of places, plural has been used instead of singular, which would have been correct, for example “wetlands emissions” instead of “wetland emissions” – I realise it’s tedious to check such things, but it would be a substantial improvement.

We agree with the reviewers and replaced “wetlands emissions” by “wetland emissions” everywhere in the manuscript.

Page 3102, line 7: There is no discussion of Fig. A2, therefore the effect of resolution is just shown in the figure, but not discussed.

In the revised version of the manuscript, we added some sentences to discuss the Fig. A2 (cf. the below underlined lines).

“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. In particular, the loss of information in sub-grid topographic index distribution due to the coarse resolution (Fig. A2-d) leads to substantial difference in the wetland extents when compared to the *a posteriori* regrid of the 1° simulated wetland extent (Fig. A2-b). Nevertheless, the global wetland extent simulated at FAMOUS resolution is close to the value given by (Prigent et al., 2007) dataset at the same resolution (Fig. A2-c).”

Page 3103, line 11: "its" instead of "is".

This has been corrected as well as the following three syntax errors.

Line 24: "density" instead of "densities".

Page 3104, line 3: "estimated from by" seems funny – I assume you want to remove either from or by?

Page 3105, line 5: CH40 should be D0

Page 3106, line 1-3: the reader is left wondering what the effect of static / dynamic vegetation is...

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.

Page 3107: There is no mention at all of the "opt" model configuration. Instead it is introduced much later. It should be mentioned here.

In the submitted version of the manuscript, ORCHIDEE-opt is introduced in the discussion (p3116). We move the below sentences from the page 3116 to the page 3107. Some minor modifications have been done.

"In order to better compare our simulation results with wetland CH_4 emissions estimates from ice-core data, a final ORCHIDEE simulation, hereafter called ORCHIDEE-opt, has been performed. In this simulation, as in ORCHIDEE-V2, the substrate sensitivity to precipitation is removed. However, a space-constant T_{ref} is used as well as a Q_{10} equal to 3. The aim of the previous configurations (V1 and V2) was either to estimate the contribution of different parameterizations to potential differences between ORCHIDEE-V0 and SDGVM, or to better understand the wetland CH_4 emissions sensitivity to climate in ORCHIDEE-WET. The aim of ORCHIDEE-opt is to provide our best ORCHIDEE estimates of the change in wetland emissions. These estimates will be exclusively discussed in the section 4."

We will answer to the two next comments at the same time:

Page 3107, lines 25/26: You mention a bias in ORCHIDEE without really explaining what exactly you mean. Please elaborate.

Page 3107, line 27 – page 3108, line 12: the moisture dependence of decomposition is hinted at by the reference to precipitation changes, but not mentioned explicitly in eq. 3 or in this paragraph. This will strongly confuse the uninitiated reader... I suggest you rephrase and clarify this paragraph.

The explanation is given right after the mention of the bias, from the line 26. We made the transition clearer and clarified the corresponding paragraph.

"In fact, in ORCHIDEE-WET, a sub-grid approach (TOPMODEL) is used to diagnose the wetland fraction of each grid-cell. But this sub-grid treatment has no effect on the carbon cycle

computation and in particular on the moisture dependence of decomposition. Indeed, there is no wetland PFT and thus no sub-grid wetland/non-wetland distinction to compute the carbon cycle variables. Instead of this, the mean value of the labile soil carbon content over the grid-cell (C_L in the Equation 2) is used as the wetland substrate. Thus the methanogenesis substrate is sensitive to change in precipitation in the model while it would be less sensitive in the reality for a continually *saturated* wetland."

Page 3110, line 10: SDGVM, not SGDVM...

This has been corrected at line 10 as well as in the other sections where this error has been made.

Page 3111, line 3-25: This section is rather difficult to understand, and it takes quite a while for the reader to discover that it's NPP that has the strongest effect. Please rephrase and clarify.

The paragraph pointed out by the reviewer has been clarified in the new version of the manuscript:

"The lower LGM emissions in boreal regions in ORCHIDEE-WET as compared to PI values, are mainly explained by a drop in the CH_4 flux densities (Figure 5). In ORCHIDEE, the main driver of the LGM – PI CH_4 flux densities change is the decrease from PI to LGM in substrate supply (Figure 6). Indeed, the effect of the change in temperature on the methanogenesis rate is low (compare ORCHIDEE-V0 and ORCHIDEE-V1 in Figure 5). The large decrease in the substrate availability computed by ORCHIDEE is not simulated by SDGVM and explains the difference of behaviour between the two models. Between the two equilibrium states (LGM and PI), the change in substrate supply is caused by a change of input, reflecting change in the Net Primary Productivity (NPP). In ORCHIDEE, the large decrease in NPP seems to be mainly driven by a change in NPP flux density per vegetation type rather than by a change in vegetation coverage (see Fig. A3). In particular, the increase in summer vegetation moisture stress in ORCHIDEE-WET from PI to LGM is a major contributor to the decrease in NPP (Fig. A3). Soil freezing processes which limit the availability of liquid water to plants is accounted for in ORCHIDEE and not in SDGVM, which could explain the lower NPP and substrate availability in ORCHIDEE compared to SDGVM. It seems that the difference of chosen proxy for the substrate between the two models (heterotrophic respiration (HR) in SDGVM .vs. labile carbon pool in ORCHIDEE) plays a minor role: in ORCHIDEE, the LGM – PI change in HR is similar to the change in active carbon pool (not shown)."

Page 3115, lines 1-24: It took me quite a while to understand what you mean, the passage is rather difficult. Please rephrase and extend the discussion of Fig. 8.

We rephrased the part of the discussion relative to the Fig. 8:

"Besides, Figure 8 also demonstrates that the HS CH_4 flux densities anomaly in the northern tropics (blue $\bigtriangledown$) in ORCHIDEE-V0 is mainly due to the substrate sensitivity to change in precipitation (see the difference of the FIXED values between ORCHIDEE-V0 and ORCHIDEE-V2). In fact, the decrease in precipitation occurring in the northern tropics during HS leads to a decrease in the ORCHIDEE-simulated NPP leading to a drop of the methanogenesis substrate supply.

Fig. 8 allows us to identify which latitudinal band and process drives the main differences between SDGVM and ORCHIDEE as described in the section 3.2.1; namely: the larger positive GI emissions

anomaly in ORCHIDEE-V0 than in SDGVM and the positive HS emissions anomaly obtained with ORCHIDEE-V1.

Regarding the CH_4 emissions during the GI period, the difference between the two models described in the section 3.2.1 is driven by the changes in the band 0-30°N ($\bigtriangledown$). The emission anomaly in this region is about 5.3 ± 2.9 Tg/yr where 88% can be explained by the expansion of wetland in ORCHIDEE-V0 against 2.5 ±2.6 Tg/yr and 63% in SDGVM.

The positive tropical anomaly obtained in ORCHIDEE-V1 is explained by the fact that a weak change in the magnitude of the anomaly of a given tropical sub-region could strongly modify the net magnitude over the tropics as a whole, given the compensating effect described above. The Figure 8 shows that the change in the temperature sensitivity formulation from V0 to V1 leads to a small decrease of the positive anomaly in the northern tropics (from -8.7 ± 1.8 to -6.9 ± 1.3 Tg/yr for FIXED, i.e. a decrease of ~20%) but with no modification in the southern tropics. This small change is strong enough to disrupt the balance of the compensating effects between the two sub-tropical regions, resulting in a positive anomaly averaged over the tropics as a whole. This underlines an increased sensitivity in ORCHIDEE-WET compared to SDGVM and thus a potential larger sensitivity to smaller local changes; while this is without any substantial change at global scale in the case described above."

Page 3116, line 11/12: Do you mean a reduction in wetland area or in wetland emissions? "LGM wetland reduction" could be wither or, though I assume it's a reduction in emissions, since area couldn't be diagnosed via atmospheric modelling and ice cores...

We replaced "finding a LGM wetland reduction by" by "finding a reduction in the LGM wetland CH_4 emissions by"

Table 1: ORCHIDEE-WET-opt should also be mentioned in the corresponding paragraphs in the text.

Done; please refer to the reply of a previous comment (about page 3107).

Fig. 1: "Proxy for substrate" would be better than "substrate's proxi"

Done

Fig. 2 and 3: The normalisation of emissions is rather problematical, especially since emission units are still listed as Tg. If you normalise, I suggest you use a 0-1 scale instead, since the normalised plots do NOT show emissions in Tg any more.

We replaced the confusing term "normalized" by "scaled". Thus, the scaled ORCHIDEE emissions are still in Tg/yr.

Fig. 4: Are you sure you used the right plots? I would assume the greyed-out areas would be identical for a and b, as well as c and d, but they aren't. b) rather looks like LGM instead of PI, since all areas covered by ice sheets appear to be greyed out... For c) it's rather interesting to see that there is no vegetation in the Hudsons Bay Lowlands, supposedly one of the major methane emission areas... Please check.

In fact, there was a problem in the ice sheets file used for this plot. We did again this Figure. The flux densities given in c) were correct in the submitted version of the draft. The CH4 flux densities simulated by SDGVM are low for the grid-cells corresponding to the Hudson Bay Lowlands but it does not mean there are no emissions (the wetland extent simulated by SDGVM are large leading to emissions; cf. Fig 3a).

Fig. 8: What's the difference between the two SDGVM plots? "divinding" by f_wtp is mentioned in the figure, but this is rather uninformative... Should be improved along with the discussion of Fig. 8.

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 right top corner corresponds to emissions from saturated wetlands alone while the left plot represents emissions from all kind 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)."