Dear Editor,

We would like to thank the two reviewers for their detailed appraisals of our work. We give below our responses, with the reviewers' comments in bold font. We have also revised the manuscript according to the suggestions of both reviewers.

With kind regards,

Peter Hopcroft and co-authors.

Review 2

Main points:

I could not find any discussion about how the areas of presently submarine continental shelf were treated with respect to soil texture and topography.

In SDGVM the soil texture for presently submarine continental shelf is extrapolated from neighbouring gridcells. The topography used is calculated from global ETOP dataset. LPJ-WHyMe does not currently make use of topography. The soil type takes one of 9 classes, one of which is peatland and this is prescribed as described in the text. The peatland soil type is new in LPJ and is described by Wania *et al* 2009a. We have added a description of this to the text.

If SDGVM can reproduce orbitally-induced changes in methane concentration (Singarayer et al. 2011), why can it not do so on a shorter-timescale?

We agree that this is an interesting point and this was part of the motivation for the current work, given similar conclusions in Hopcroft et al 2011 and in a separate modelling study of Ringeval et al 2013 (*Climate of the Past*) . However, in the work of Singarayer et al 2011, a reasonable proportion of the amplitude of the glacial-interglacial change in atmospheric CH4 concentration is due to atmospheric lifetime variations. The work of Levine *et al* 2011 suggests by contrast, that the CH4 lifetime was likely relatively constant. Hence though SDGVM can explain relatively well the temporal changes in emissions, their full magnitude is likely underestimated. This underestimation is more pronounced for abrupt events as simulated here and in Hopcroft et al 2011, and this was a motivation for including different soil physics and carbon cycling (i.e. by using LPJ-WHyMe). It is not really possible to answer this open question in any more detail without performing further climate model simulations, for example employing a different scenario of climate forcing for the abrupt warming (i.e. not solely based on AMOC changes), or including new physics of wetlands, for example hydrological transport, coastal inundation effects etc.

If low-latitude sources are fingered as the dominant contributor to abrupt changes in atmospheric CH4 (as suggested by Baumgartner et al. 2012 and Brook et al. 2000), why is so little discussion given to the low-latitude SDGVM results?

The initial aims of this work were to explicitly test the assertion that high-latitude peatlands are not important during an abrupt warming. This also leads on from our previous work in which we demonstrated that neither SDGVM nor ORCHIDEE could not replicate the required emissions changes globally (Hopcroft et al 2011, Ringeval et al 2013). Ice-core records have shown for example (Fluckiger et al 2002) that there is significant variability in the amplitude of the CH4 change during the abrupt phases of Dansgaard-Oeschger cycles. This is thought to reflect changes in background conditions arising from different orbital parameters and atmospheric CO2. Here we explicitly account for these in our climate model simulations. Subject to these more realistic climate simulations, we reach similar conclusions as in our prior work and the work of Ringeval et al 2013. We believe that a thorough analysis of low-latitude sources is beyond the scope of this work and requires some additional model development, for example introducing more sophisticated representations of wetland area (e.g. Fan and Miguez-Macho 2011) as recommended by Ringeval et al 2013.

While this is not a very instructive comment for the authors, I think it would be good to try and ensure throughout the MS that the distinctions between the models remain clear. I believe much of the problem lies in the sections detailing the extensive tests where outputs of LPJ-WHyMe are used by SDGVM are described.

Although not clear here what exactly the reviewer is suggesting, we decided to change the wording. Thus we now refer to peatlands for LPJ-WHyMe and wetlands for SDGVM. For the modified versions of SDGVM we use the term CH4-producing areas.

<u>**Specific comments:**</u>

Figures/tables
We have implemented all of the changes requested by the reviewer 2 for the figures and ordering of data in tables 2 and 3. In doing this we found a small error in the way ice-sheet area was implemented in SDGVM. We have corrected this and included revised values in table 4 and figures 6-8. This only affected SDGVM and not the hybrid model versions.

GCM grid size
We have added this information.

Could it be elaborated on how the vegetation distribution was changed?

The vegetation distribution is not prescribed at all in SDGVM, it evolves dynamically according to the prescribed atmospheric CO2 concentration and climate inputs. LPJ-WHyMe follows a similar principle except that only the 2 wetland plant functional types are allowed in gridcells designated as peatlands. The vegetation distribution is prescribed within the climate model simulations and this follows the pre-industrial distribution except where there is land ice or new land, in which case it is extrapolated from nearby gridcells. We have clarified this in the text.

What does the +/- in front of the 0.5Sv represent?

This is because a the freshwater forcing applied to the ocean in the coupled GCM is first positive and then negative. This follows the forcing employed in Hopcroft et al 2011. We will clarify this in the revised manuscript. This has been clarified in the text.

p.3525 l.3 - Can you explain what you mean by 'the prescribed CO2 level generally takes the same value as in the respective GCM simulation'

This is because in one sensitivity simulation with LJP-WHyMe the CO2 level applied to the is different from that prescribed within the climate model. This is done to test the fertilization effect on wetland emissions. We have clarified this in the revised manuscript.

l. 4-9 - I didn't fully follow this. Can you please rewrite this to make it clearer what was done here.

This has been reworded. We hope that it is now clear what was done here.

A reference for the PI AMOC value
We have added a reference to the inferences about the modern Atlantic MOC and how this compares with the model results which we quote here.

p. 3528 l.11 and Figs 2 and 3 - Annual mean values are not useful since most wetlands have little-to no CH4 production in the winter. Both plots should show summer values. Please change these.
Done.

Figures 2 - 5 - Can an outline of the ice sheets be added to these plots? It would help the reader interpret the images. The grey scale (for ex. on Fig 4) helps to let the reader know what grid cells contained some land but without information on the ice sheets, it is hard to determine what land is then available for wetlands (in the case of SDGVM).
Done.

p. 3530 l. 17. - Given the grid cell size used here, I would assume it would be difficult to match the Santa Barbara basin record. I imagine the basin would take up a small part of one of your cells.

The point here is that the significant warming inferred in this region of the west coast of North America is not replicated in the model. The original reconstruction was interpreted as evidence for an atmospheric driver of abrupt climate change (as opposed to the prevailing theory of ocean circulation change). It seemed therefore worth noting the model-data discrepancy in this case.

l26 - Maybe add what [CO2] was used here?
We have added this to the text.

p. 3531 l. 5-17 - Could this section be more quantitative, i.e. add in the numbers that are discussed.
We have added the relevant values here.

p. 3532 l. 2 - I don't see the magnitude of change decreasing with latitude as mentioned.
This is not clear, so we have removed this sentence.

p. 3534 l.5 - add 'for these models' after emissions.
Done.

p. 3541 l.9 - Melton et al. 2012 also use ice-core isotopic evidence but suggest biomass burning was important during at least one abrupt warming (YD termination), also see a role for thaw lakes.

The Younger-Dryas is poorly understood and not generally seen as equivalent to a D-O event. We have added this reference for completeness.

p. 3542 l. 1-5- but your study only talks about boreal changes so this sentence is too general.

We analysed in detail the boreal changes, but also discuss and quantify the global effects on the CH4 cycle by using SDGVM and SDGVM in combination with LPJ-WHyMe. We have nevertheless slightly reworded this sentence to be more specific to the results presented.

l6 - missing words before 'but'?
We have corrected this.

Table 3. Give a reference for the peatland area of LPJ-WHyMe during PreIndustrial.
This is already given as a footnote to the table. (Spahni et al, and Yu et al).

Figure 1 - The overshoot on the turnover is pretty big. Is there any way to check if this is in any way realistic? Also looking at this it is easy to see why the different time periods produce such similar results...

There are proxies for some of the physical changes in the ocean circulation during the D-O events, but there is considerable uncertainty associated with the interpretation. This is one of the reasons for trying to understand other quantities such as CH4. The climatic changes in response to these AMOC changes have previously been compared with model results from the study of Liu et al 2009 and show broadly similar global patterns.

Figure 2 - Please make the LGM, 14k etc. labels bigger. The colour scale choice is strange. From my copy, white is -1 to 0(?) then green from 0(?) to 2. I assume it is just the labels? The white should straddle 0. Same for fig 3.
Done.

Fig 7 - Some of these plots have so few LPJ-WHyMe cells that I wonder how robust any results can be given that it looks like you can have only around a dozen cells for a run. How do you ensure that the LPJ-WHyMe results are not somehow biased by such a small number versus the SDGVM results?

The low number of grid-cells is indicative of the high sensitivity of CH4 emissions in LPJ-WHyMe to the glacial boundary conditions. In the model comparison, the area available for emissions is the same in each model. Comparing LPJ-WHyMe and SDGVM-LPJh (as plotted) we see a big difference in both the CH4 emitting area but also the CH4 flux in each grid-cell. This difference is minimised in the further versions of SDGVM-LPJh,Rh,T as documented in Table 4. This suggests that we are not biased in the comparison.
The low number of gridcells is also most extreme in the LGM case. In the 14kyr case for example the number of gridcells with significant CH4 emissions is much higher, and is actually close to the PI distribution.

References:

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Fluckiger, J., Blunier, T., Stauffer, B., Chappellaz, J., Spahni, R., Kawamura, K., Schwander, J., Stocker, T. F., and Dahl-Jensen, D.: N_2O and CH_4 variations during the last glacial epoch: Insight into global processes, Global Biogeochem. Cy., 18, GB1020, 10.1029/2003GB002122, 2004.

Hopcroft, P., Valdes, P., and Beerling, D.: Simulating idealized Dansgaard-Oeschger events and their potential impacts on the global methane cycle, Quaternary Sci. Rev., 30, 3258–3268, 2011.

Levine, J., Wolff, E., Jones, A., Sime, L., Valdes, P., Archibald, A., Carver, G., Warwick, N., and Pyle, J.: Reconciling the changes in atmospheric methane sources and sinks between the Last Glacial Maximum and the pre-industrial era, Geophys. Res. Lett., 38, L23804, 10.1029/2011GL049545, 2011.

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Ringeval, B., Hopcroft, P. O., Valdes, P. J., Ciais, P., Ramstein, G., Dolman, A. J., and Kageyama, M.: Response of methane emissions from wetlands to the Last Glacial Maximum and an idealized Dansgaard-Oeschger climate event: insights from two models of different complexity, Clim. Past, 9, 149–171, doi:10.5194/cp-9-149-2013, 2013.

Wania, R., Ross, I., and Prentice, I. C.: Integrating peatlands and permafrost into a dynamic global vegetation model: 1. evaluation and sensitivity of physical land surface processes, Global Biogeochem. Cy., 23, GB3014, 10.1029/2008GB003412, 2009a.

Review 1

Main points:

From this paper one can conclude that both models, SDGVM and LPJ-WHyMe, can partly explain observed emissions over D-O events, but each model has some limi-tations in the current setup. While SDGVM can calculate "wetlands" dynamically, it lacks soil freezing that is essential for the simulations of peatlands, especially in the glacial period. On the other side LPJ-WHyMe can simulate CH4 emissions also in the permafrost area, but is bound to a fixed peatland distribution, which certainly was different during the glacial period. Thus the results are constricted to these limitations and sometimes taken with a grain of salt. In the end, the main findings describe a model intercomparison, which I find myself very interesting, but maybe is less appealing to the general CP readership.

Actually we argue that neither model, either in combination or separately, can fully explain the D-O CH4 changes. We add that the primary aim of this work is to investigate the contribution of high latitude peatlands during D-O events. Intuitively an abrupt warming in the Northern Hemisphere could be thought to cause carbon rich peatlands to rapidly release a large amount of CH4. Our process-based model results support recent ice-core data in suggesting that the majority of CH4 produced during D-O events was emitted at lower latitudes. Our results also raise the question about the climatic scenarios used, since the global signal simulated in SDGVM is too small to satisfy the ice-core record. We accept that there are limitations to the models we have employed, and we have tried to be clear about this in the manuscript. We have also emphasised that there are limitations to the climate scenarios employed. We would anticipate future work could revisit these time periods as understanding of wetland and peatland dynamics advances and as our understanding of the drivers of Dansgaard-Oeschger events improves.

p. 3530, before section 3.2: I miss an entire description of the spinup features of LPJ-WHyMe and general soil carbon cycle information that are very important for the understanding of CH4 emissions later.

The equilibration time for the soil carbon in LPJ-WHyMe is of the order of 1000s of years as pointed out by the reviewer and as shown by Wania et al 2009b. We tested LJP-WHyMe under LGM simulations, with a 2000 and a 10,000 year spinup length. We then forced the two resultant model states with the transient LGM climate changes including the cooling and abrupt warming. The resultant CH4 emissions time series showed no significant differences. We thus employed a 2000 year spinup for each simulation used in this work.

What vegetation is growing in peatlands for the different time periods (percentage moss/grasses)? Which part of the peatland grid cells lies within the permafrost area? What is the average water table and thaw depth? Are values similar to what was obtained by Spahni et al. (2013) ?

We added a new figure showing changes in water table depth, thaw depth and coverage of the 2 peatland PFTs. We also added some discussion of this and related it briefly to parts of the model intercomparison section.

p. 3530, l. 21: Maybe I misunderstand the results for CH4 simulated in peatlands during the glacial period, but they seem trivial to me.

Whilst this may be trivial for the case of the LGM, during the other time periods, the CO2 level was higher ranging from 211 to 237 ppmv. Crucially the orbital insolation was also very different during each of the time periods other than the PI and LGM, with much stronger summer insolation at high latitudes. We believe that these very different boundary conditions mean that the system responses are not obvious and our modelling with multiple coupled GCM simulations allows this to be tested.

Specific comments:

Please rephrase the sentence by saying that Baumgartner et al., 2012 is an update of Dällenbach et al., 2000,
We have changed the wording of this sentence.

p. 3525, l. 15: Wouldn't that imply that peatlands were not present before 16 kyr BP at the core site?

This implies that there were no northern peatlands at the site implying a strong contraction in the northern peatland source during the last glacial period. We have attempted to represent this in our sensitivity simulations with LPJ-WHyMe. The distribution inferred by MacDonald et al 2006 for the period of the Bolling-Allerod at 14kyr is actually very similar to our modelled distribution for the simulation with extra peatland prescribed in Europe and North America (given limitations in this type of model-data comparison).

p. 3525, l. 25ff: Please cite Spahni et al., CP, 2013, who did exactly that for the transition from the LGM to the Holocene period. They also used the LPJ-WHyMe model for peatlands.
We added this reference.

p. 3526, l. 2ff: Assuming that present day peatlands started to form after 16 kyr BP, what is the reason to assume there have been peatlands at the same location during the glacial period?

Your additional scenarios suggesting peatlands are located much further south sounds convincing.

Indeed a primary aim of this work was to test the hypothesis that high-latitude wetlands were the dominant source of CH4 during D-O events (Dallenbach et al 2000, Bock et al 2010). Although recent improvements in the ice-core data (Baumgartner et al 2012) showed that for the more recent D-O events this is not the case, the D-O events considered are the Bolling-Allerod and D-O events 2-4. In our work we expand this question by considering abrupt climate transitions during different points of Marine Isotope stage 3. The actual distribution of peatland used is therefore intended as a sensitivity test, and is not meant to be a best estimate what actually occurred during each D-O event.

p. 35 27, l. 16: Please indicate that these are Greenland averages and not Northern Hemisphere land area, which are probably more relevant for CH4.
Done.

p. 3531, l. 2: Are "Asian sources" the same areas as "Siberian peatlands"?
We have changed the wording here.

p. 3531, l. 6ff: Please also mention that Fischer et al. (2008) and Bock et al. (2010) assumed a constant isotopic 13C signature for peatlands in their simple box model approach.
We included this information.

p. 3531, l. 26: typo? "from is"
Corrected.

p. 3533, l. 3: This implies that northern peatlands are the only "northern" source for CH4 emissions, i.e. no other wetlands are present in this case? Please clarify.

We are trying to put the modelled changes in northern areas simulated here into context of the emissions budget derived by Bock et al 2010. This has been clarified in the text.

p. 3533, l. 11: I fully agree with your logic. Please also mention the possibility that peatland area may have shifted in latitudes without a big change in net "active" peatland area. Also transitions from fens to bogs may be possible as suggested by Sowers, 2010.

We included these two additional considerations in the text.

p. 3533, l. 22: Is the assumption behind the comparison that the two model setups (LPJ-WHyMe and SDGVM) represent the same source category for CH4 emissions, or is it thought to be used complementary as the areas are not identical? Please clarify your assumptions.

The two models simulate wetlands with different physics and biogeochemistry allowing complementary testing of the plausible sensitivity range. LPJ-WhyMe represents peatlands only, whilst SDGVM does not make a distinction between wetland types. The point of the comparison is to put these LPJ results in the context of previous work (Hopcroft et al 2011, Ringeval et al 2013) and as a means to understanding the behaviour of the more complex model. We have added a couple of sentences about this.

p. 3533, l. 7: Is this caused mainly bye the fact that in SDGVM there is no soil freezing, which would prevent CH4 emissions? Fig. 5 at least suggests that there is a fundamental difference in soil processes regulating CH4 emissions in Northern Siberia.

There are no CH4 emissions in SDGVM when the surface temperature is below 5C, so this is not the direct reason for the difference between the models. The differences are fully explained in the following section 4.2. We added a sentence to clarify this.

p. 3534, l. 16: This statement sounds not very robust to me, given the fact that LPJWhyMe simulates emissions for only 11 grid cells in Siberia and 3 coastal grid cells (Fig. 7), while SDGVM has a much larger number of grid cells for averageing. I also have similar reservations about the robustness of percentages in Table 4.

This statement is revisited in the more detailed model intercomparison that follows. Regarding the values in table 4, the relative changes shown in table 4 are sensitive to any small value in the EQ phase of the simulation. However, the values are simply used to illustrate the relative sensitivity of each model (or model version). Whilst the relative sensitivity of LPJ-WHyMe is much higher than SDGVM, the absolute sensitivity is of a similar magnitude.

p. 3535, l. 17: The importance of the N cycle might be a good point for low temperature effects in the LGM. Does the CENTURY model have any temperature effects on N processes, e.g. N uptake by plants? … freezing soil water limits plant available soil moisture in cold areas and leads to water stress of plants (drying). Is this feedback included in SDGVM?

SDGVM does include temperature dependence of nitrogen uptake by plants as described in Woodward et al 1995. The uptake is also dependent on soil carbon content. We have added this to the text as suggested by the reviewer. We discuss the influence of soil water stress on plants later in this section already.

p. 3535, l. 26: Could also be CH4 production which is parametrised differently, once more related to NPP, once more related to respiration in the two models, true?

p. 3536-3537: Ok, here the implications of the different processes are now fully discussed, after some hypotheses have been made (see previous points). I would suggest to indicate beforehand that you will analise the processes in detail and already anticipate your conclusion. That would help the reading through this section.

We have added that the conclusions are revisited later in the subsection.

p. 3538, l. 2: Are the extra grid cells in Europe and North America (<45N) during the glacial period reserved to LPJ-WHyMe or to SDGVM?
These extra points are added in LPJ-WHyMe only, except where the modified runs of SDGVM follow the peatland areas in LPJ-WHyMe. WE have clarified this in the text.

p. 3540, l. 16: typo: "CSM1.4"
Corrected.

p. 3542, l. 9ff: I think this is the key message of the paper and should go into the abstract as well.
We added this message to the abstract.

Fig. 1: The bars for EQ runs look somehow "green" not "grey" in my pdf.
Looking at the online *Discussions* pdf, these bars are definitely grey.

Fig. 2: Most interestingly would be to see regional differences in Siberia. Please add an

additional colour level between 2 and 5 degree C.

We have changed this figure in response to comments from reviewer 2 who asked for summer (JJA) means. The contours now allow regional changes over Siberia to be seen.

References:

Baumgartner, M., Schilt, A., Eicher, O., Schmitt, J., Schwander, J., Spahni, R., Fischer, H., and Stocker, T. F.: High-resolution interpolar difference of atmospheric methane around the Last Glacial Maximum, Biogeosciences, 9, 3961–3977, doi:10.5194/bg-9-3961-2012, 2012.

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Dallenbach, A., Blunier, T., Fl ̈uckiger, J., Stauffer, B., Chappellaz, J., and Raynaud, D.: Changes in the atmospheric CH4 gradient between Greenland and Antarctica during the Last Glacial and the transition to the Holocene, Geophys. Res. Lett., 27, 1005–1008, 2000.

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Woodward, F., Smith, T., and Emanuel, W.: A global land primary productivity and phytogeography model, Global Biogechem. Cy., 9, 471–490, 1995. 3522, 3526.