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Interactive comment on "Limited response of peatland CH_4 emissions to abrupt Atlantic Ocean circulation changes in glacial climates" *by* P. O. Hopcroft et al.

Anonymous Referee #2

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Dear Editor,

General Comments:

This paper presents results from two wetland/peatland models, LPJ-WHyMe and SDGVM, driven by FAMOUS model climate for 5 different D-O events. The models differ substantially in their ability to simulate different aspects of peatland soil physics and hydrology. The models are unable to fully replicate the large shifts in atmospheric methane concentration observed in ice core records.

While this paper is interesting, I had some difficulties with it. I am not sure if CP is the ideal journal, as the paper does veer a bit into model-intercomparison. However, the

paper should still be of interest to CP readers as the main point of the paper is within CP's scope. I also feel that some important information was missing to allow me to easily judge the robustness of the results as presented (see my points below).

My main questions/concerns are as follows:

1. I could not find any discussion about how the areas of presently submarine continental shelf were treated with respect to soil texture and topography. Presumably these areas could have importance for the SDGVM runs in particular. It is mentioned on p.3525 l. 22 that areas of new land get some peatlands. However, with no information about how these surfaces were treated, it is hard to understand if it was done appropriately. As stated in Berrittella and van Huissteden (2009): '{Glacial CH4} emissions are also highly sensitive to assumptions about the extent of ice cover and exposed seafloor. Wetland expansion over low relief exposed seafloor areas have compensated for a decrease of wetland area due to continental ice cover'. So I think it should be stated clearly how these areas were treated.

2. If SDGVM can reproduce orbitally-induced changes in methane concentration (Singarayer et al. 2011), why can it not do so on a shorter-timescale? Is this i) intrinsic to the model, ii) from the driving climate, or iii) demonstrating that, indeed wetlands are important on orbital timescales but less so for abrupt changes? Some discussion was made of this (e.g. p. 3538 l 18-24) but I wonder if this could be expanded upon or made more explicit. This is a point that could be of most interest to CP readers and one that I was very interested in, but finished the paper not wholly sure of the answer.

3. 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? Yes, the paper is mostly directed at the peatlands, which are primarily boreal. But, if

SDGVM was already run globally, it might be interesting to comment on whether the model response in the tropics backs up Baumgartner et al. 2012 and Brook et al. 2000.

4. After reading the paper I was left with the impression that what the SDGVM and LPJ-WHyMe models attempt to simulate is somewhat equivalent, and that the differences between the two is mostly related to soil physics and hydrology. This is, of course, not the case as the WHyMe is a peatland specific model and SDGVM simulates some sort of generic wetland. SDGVM can't really simulate a peatland, it can only generically simulate a 'wetland', even with more accurate soil moisture and temperature. 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.

Even with those concerns, I believe that in general this paper is worthy of publication. However it needs some revisions before that should occur. In particular I would like to see some of the figure changes as detailed in the specific comments. The text is generally well written with good style.

Specific Comments:

p.3523 l.14 -pls state the GCM grid cell size

l.18 - pls put time periods in chronological order (switch 14 and LGM)

l.24 - Could it be elaborated on how the vegetation distribution was changed? Was the vegetation distribution changed solely due to land and ice area? No influence of climate or [CO2]? If so, is that reasonable given that SDGVM does not have wetland specific PFTs and thus the vegetation choice directly determines what plants are in the wetlands?

p.3524 l.8 - What does the +/- in front of 0.5 Sv represent? Was the simulation not forced with a set maximum freshwater input (thus exact)?

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'

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

p. 3527 l.26 - add reference for PI AMOC value

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.

Figures 2 - 5 - Can an outline of the ice sheets be added to these plots? It would help the reader interpret the images. The gray 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).

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.

l. 26 - Maybe add what [CO2] was used here?

p. 3531 l. 5-17 - Could this section be more quantitative, i.e. add in the numbers that are discussed.

p. 3532 l. 2 - I don't see the magnitude of change decreasing with latitude as mentioned.

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

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.

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

l. 6 - missing words before 'but'?

Table 3. Give a reference for the peatland area of LPJ-WHyMe during PreIndustrial.

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...

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.

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?

References:

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Brook, E. J., Harder, S., Severinghaus, J., Steig, E. J. and Sucher, C. M.: On the origin and timing of rapid changes in atmospheric methane during the Last Glacial Period, Global Biogeochem. Cycles, 14(2), 559–572, 2000.

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Singarayer, J. S., Valdes, P. J., Friedlingstein, P., Nelson, S. and Beerling, D. J.: Late Holocene methane rise caused by orbitally controlled increase in tropical sources, Nature, 470(7332), 82–85, 2011.