Clim. Past Discuss., 9, C1774–C1778, 2013
www.clim-past-discuss.net/9/C1774/2013/

Climate of the Past Discussions Open Access

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 #1

Received and published: 21 August 2013

In their study Hopcroft et al. carefully compare the simulated CH4 emissions for peatlands and other northern wetlands mainly during periods of the last glacial. The freshwater forcing experiments allow for simulations of abrupt CH4 emission changes over D-O events with two models. Allthough the study is well written and presented, I find that the resultss are not ground breaking. I thus suggest major revisions for a publication in CP.

General:

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.

Specific:

p.3521, l.28: There is a reason why the new analysis by Baumgartner et al., 2012 is more reliable than the analysis by Dällenbach et al., 2000: they used CH4 measurements series produced in the same lab and in the same time period in higher resolution to exclude artificial offsets directly affecting the CH4 gradient. Please rephrase the sentence by saying that Baumgartner et al., 2012 is an update of Dällenbach et al., 2000, rather than a contrast.

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

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.

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.

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.

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. Are peatland carbon pools in equilibrium? Wania et al. (2009b) showed that spinup can take several 10000 years for equilibration, which in turn matters for heterotrophic respiration that serves as the key variable for methanogenesis. 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) ?

p. 3530, l. 21: Maybe I misunderstand the results for CH4 simulated in peatlands during the glacial period, but they seem trivial to me. As shown land surface temperature in high northern latitudes are several degrees cooler and thus the growing season is much shorter, if soils are not permanently frozen and vegetation grows at all. Thus this raises the question again if peatlands were present at this location and not shifted further south also in Siberia, similar to NA and EU?

p. 3531, l. 2: Are "Asian sources" the same areas as "Siberian peatlands"? Please clarify.

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. If peatlands change location, also the signature and thus the CH4 budget may change. I thus wouldn't call the results surprising given the assumptions in the simple box model and those about the location of peatlands in your model.

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

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.

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.

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.

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.

p. 3534, l. 16: This statement sounds not very robust to me, given the fact that LPJ-WhyMe 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.

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? Along the same lines there might also be a hydrological influence that could explain the lower NPP in the LGM for LPJ-WHyMe: 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?

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 indiciate beforehand that you will analise the processes in detail and already anticipate your conclusion. That would help the reading through this section.

p. 3538, l. 2: Are the extra grid cells in Europe and North America (<45°N) during the glacial period reserved to LPJ-WHyMe or to SDGVM?

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

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

Figures:

Fig. 1: The bars for EQ runs look somehow "green" not "grey" in my pdf.

Fig. 2: Most interestingly would be to see regional differences in Siberia. Please add an additonal colour level between 2 and 5 degree C.

References:

Sowers, T., Atmospheric methane isotope records covering the Holocene period, Quaternary Science Reviews 29 213-221, 2010.

Spahni, R., Joos, F., Stocker, B. D., Steinacher, M., and Yu, Z. C.: Transient simulations of the carbon and nitrogen dynamics in northern peatlands: from the Last Glacial Maximum to the 21st century, Clim. Past, 9, 1287-1308, doi:10.5194/cp-9-1287-2013, 2013.

Interactive comment on Clim. Past Discuss., 9, 3519, 2013.