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Biogeosciences Discussions

Interactive comment on "Impact of an 8.2-kyr-like event on methane emissions in northern peatlands" *by* S. Zürcher et al.

Anonymous Referee #2

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This study investigates the climate driven perturbation of global methane during the 8.2 kyr climate anomaly, and estimates the contribution of changes in natural wetlands emissions using a dynamic global vegetation model. The quantification of the contribution of the various sources and sinks of methane to historic climate variations is still a largely unresolved scientific problem. This study concentrates on improving the representation of emissions from natural wetlands, which is a key requirement since wetlands constitute by far the dominant source of preindustrial methane. In absence of any useful proxies of historic wetland emissions it is difficult to judge the overall performance of the model for the 8.2 kyr event. Nevertheless, the estimates that are reported provide a useful reference and constraint for follow up studies. To improve the usefulness, however, several revisions and clarifications are needed as outlined below.

MAJOR COMMENTS

Climate simulation: The methane emission estimates for the 8.2 kyr event depend largely on the realism of the simulated climate anomaly. Very limited information is provided to motivate the choices that were made and to quantify the uncertainties involved. For example, what is the evidence in support of initializing the CSM climate model using preindustrial conditions? The fact that Bozbiyik et al. (2011) used these simulations to study the Younger Dryas certainly calls for further discussion about representativeness for the 8.2 kyr event. What motivates the choice of a 1 SV freshwater perturbation? How effective is it to remove the bias in CSM temperature and precipitation by using CRU data, which are representative of the period 1960-1990? Unless the period 1960-1990 is representative of the period prior to the start of the 8.2 kyr event this seems a replacement of one bias by another. More evidence should be provided to motivate these choices and to evaluate the representativeness of the climatic boundary conditions for the conditions during the 8.2 kyr event. Furthermore, the purpose of differentiating between s1, s2, and s3 is unclear. The scenarios are defined and some results are presented per scenario, but the differences are not discussed and therefore don't seem to be relevant. This should be resolved.

Fixed boundary between acrotelm and catotelm: This boundary has been fixed at 30cm. The question is whether this is a save boundary. Does the acrotelm ever dry out in the simulations? If so, what are the implications of assuming a saturated layer at depth larger than 30cm?

Model optimization and scaling: There is some confusion about which parameters were involved in the model optimization procedure. Page 13252 explicitly mentions that these parameters are the CH4 production factor, the oxidation fraction, and the tiller radius. Page 13253 lists optimized values for 5 parameters, but later it is mentioned that the oxidation was set to 0.5 as in Wania et al (2010). Else, it is not clear how the parameters were optimized, and by how much the results deviated from the first guess. If the RMSE is calculated from the daily data, then what is the purpose of the spline

fit to the observations? After the optimization process two more scaling factors were applied, that both correct the simulated wetland area (one of them through scaling of the global emissions). It is not quite clear why two scaling factors are needed instead of one. Else it should be mentioned how large the corrections were (right now only the scaled values are reported).

Added value of the extended modeling approach: Page 13260 discusses the advantage of the coupled process modeling approach over the more simple approach of quantifying methane production as a fixed fraction of respiration. However, the significance of the difference in favor of the coupled approach is not clear. How does the difference between 2.5% and 2.7% translate into methane emissions and how does it compare with the uncertainties involved?

MINOR COMMENTS

Abstract, line 22: “not completely” Does not seem an accurate representation of this fraction. I advise to quantify this contribution explicitly.

Abstract, line 23: “pointing to a significant contribution from tropical wetlands to this event” No evidence is presented that allows addressing this source in particular (as opposed, for example, to sinks)

Page 13245, line 9: An additional sentence is needed to explain the significance of the 8.2kyr event for methane. How representative is it of other abrupt climate variations?

Page 13247, line 6: "... is not sufficient for applying top-down approaches" I don’t see the limitation in applying inverse modeling techniques. Obviously the spatial resolution is limited when using only Greenland and Antarctica, but whether or not it is possible depends on the set up of the inverse problem.

Page 13248, line 2 “This approach appears ... by land ice at that time. ” This is not a proof that the 8.2 kyr peatland distribution is similar to present.

Page 13253, line 4: “We find an improved agreement between model and observations

compared to results presented by Wania et al. (2010).” By how much?

Page 13253, line 8: “The methane routine ... completely oxidized.” It seems that this problem is caused by errors in the simulated temperature profile, rather than a neglect of the heterotrophic respiration.

Page 13255, line 2: The 1048 million km2 is incompatible with the 2.06 million km mentioned later. Page 13256, line 18 and caption Figure 7: When emissions are expressed per unit area it should be made clear what area is meant: grid box area or wetland area?

Page 13259, line 14: ”The decrease in respiration ... decrease in soil carbon inventory.” This sentence is unclear. After few hundred years of simulations with perturbed climate, the carbon inventory has to change. Did the authors consider the sum of fast and slow soil carbon pools? What is the change if the authors focus on the fast pool only (HR comes mainly from the faster carbon pool)? In addition, the authors should give a map of simulated peatland soil carbon stocks, at least for the end of the spin-up.

Page 13260, line 5: “This suggests that a fixed scaling ... or glacial-interglacial cycles." Do the authors refer to previous modeling studies (e.g. Singarayer et al. (2011) and Kaplan et al. (2006) as given in the introduction)?

Page 13262, line 7: 10 Tg/yr interannual variability for northern wetlands seems quite large compared with the Spahni et al, 2011.

Page 13274, line 28: “... recent observations.” A reference is needed here.

Page 13274: It is unclear whether N2 makes any significant contribution to ebullition, and therefore if the assumption of 1% gaseous volume has any relevance to the overall gas exchange by ebullition.

Section 4: Judging the contents of this section it should rather be called “Discussion and conclusions”. Better even would be to cover the discussion and conclusions in separate sections.

Figure 2: The RMSE represents the difference of what exactly?

TECHNICAL CORRECTIONS

Abstract, line 19: This sentence is formulated unnecessarily complicated. Please rephrase.

Page 13246, line 5: “shows that also . . . all of the sites”. Sentence should be shortened and simplified.

Page 13246, line 25: “approaches” instead of “attempts”

Page 13248, line 15: “sensitivity TO”

Page 13255, line 2: “Figure 4A”

Page 13255, line 4: “input has to be . . . preindustrial conditions” please reformulate.

Page 13256, line 1: “All LPJ simulations”

Page 13257, line 8: “response TO”

Page 13257, line 19: “Fig 5” instead of “Fig. 7”

Page 13260, line 28: “However, large reductions in emissions through one pathway are only partially compensated.” The authors should explain more simply that the oxidation relative to each pathway is different.

Page 13264, line 5: closing parenthesis missing.

Figure 2: Abisko shows a spline fit to no measurement data.

Figure 8b: The line for “diffusion, large tiller seems missing”

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