Supplementary December 19, 2012

Biogeosciences Discussions

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

Author Reply to Referee #2:

We would like to thank the two referees for their time and care in providing comments on our manuscript. Our comments are presented in blue font, the Referee's original comments are in black. *Italics* are used for the quotations of changed or added text in the manuscript.

Anonymous Referee #2

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.
We are pleased to hear that the referee feels our project to be of value and will discuss in the following all remarks and changes we will make.

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? 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.
We agree on the statement of both referees that the manuscript can not give enough evidence for the input data to be completely representative for the 8.2 kyr event. We will change the text accordingly and put the focus primarily only on a freshwater hosing experiment and not on the 8.2 kyr event in particular and move the comparisons of our results with the 8.2. kyr event to the discussions section and discuss the limitations and uncertainties of this comparison there.
Our motivation to take these climate anomalies was that the resulting temperature change over Greenland is comparable to the ice-core measurements (see Figure 1). Kobashi et al., 2007 found a Greenland temperature decrease by 3.3 ±1.1° C (decadal average) in less than 20 years. The Greenland temperature in our input data drops by almost 4° C. They suggest that during the 8.2 ka event average northern (90 –30 N) temperature cooled by 1–2° C as inferred from many paleo-data

from Europe (Wiersma and Renssen, 2006). These proxy data fit well with our input data (see Fig. 5). Further, Kobashi et al. mention a drying, especially surrounding the North Atlantic area. Please see also answers to Referee #1, under specific comments #2. We added in the discussion section a paragraph providing more information about the reconstructed climate anomalies, compare them to our input and shortly discuss previous modeling studies as suggested.

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.

LPJ was widely validated for CRU-data to produce a realistic vegetation distribution (similar to present). By taking CRU-data, one can avoid model-bias for pre-industrial conditions in LPJ. The simulations are idealized and considering they are not too specific for the 8.2 kyr event, it's a valuable choice. It is true that there are still uncertainties how representative the climate data is for a 8.2 kyr event comparison. We added this consideration to the discussion section of the manuscript (among many other uncertainties).

Text in section 2.3.1:

As the LPJ model is sensitive to the absolute input climatology, this procedure eliminates biases of the NCAR CSM 1.4 for modern climate relative to the CRU data set and, thus, corrects for climate-related biases in grid cell vegetation distribution and carbon stocks.

Added text in discussion:

There are uncertainties to which degree the climate variability we used is similar to the variability during the 8.2 kyr event.

We will then discuss all uncertainties in a new paragraph.

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.

It is common practice in the field of climate modeling to use ensembles of simulations to address the role of internal variability. Here, we used three ensemble runs to check for the robustness of the result (different starting point for freshwater input). The result was that the signal from the freshwater hosing is much larger than internal variability and therefore we only presented detailed results of run 1. We added some text to state this more clearly and explain the presence of three runs (section 3.2).

The results are consistent across the three ensemble members. The signal from the freshwater hosing is much larger than internal variability and therefore we will disregard a detailed discussion of scenarios 2 and 3 further on.

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?

In the newest version of LPJ, this boundary is not fixed anymore and peatland can disappear (Spahni et al., 2012). In the model version applied in this study this feature was not used. Tests with the new version show that this assumption was justified for this study. There are only very few sites where the model suggests for the acrotelm to catotelm boundary to go below 30 cm. Other studies like Spahni et al., 2010 or Kleinen et al., 2012 had similar assumptions. The resulting error for methane is small as oxidation is very big in the upper layers, and only small emissions come from the regions with changes in this boundary. We added a short remark to the appendix.

This assumption is justified as the model simulates only for very few sites ever an acrotelm to catotelm boundary below 30 cm when this boundary is not fixed.

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.

On Page 13253, line 8 ff. we now make more clear the difference between the 3 optimized values (CH_4 production factor, the oxidation fraction, and the tiller radius) and the 2 assumptions for ebullition (threshold and volume released during an ebullition event). The two parameters controlling the ebullition showed to make no significant change for the 7 tested sides or the global emissions. They only modulate the timing and magnitude of the peaks on a short timescale, not significantly changing the total emissions seen in the spline functions. Therefore, they were not used in the optimization process. But we present for all 5 parameters results in the sensitivity study (Table 1) illustrating their effect on the emissions. We reformulated the text in the manuscript in several sections to separate more clearly between the optimized parameters and the two assumptions. For example:

The optimized parameters are 0.17 $gC(gC)^{-1}$ for the ratio of 2 CH_4/CO_2 production under anaerobic conditions, 0.5 for the oxidation fraction, i.e. the fraction of available oxygen used for methane oxidation and 0.0035m for the tiller radius. Further, we make the assumptions that ebullition occurs when 15% of the available pore volume is gas and that 1% of the available volume is released as gas in a single ebullition event. The influence of these parameters on modelled changes in methane emissions after a climate perturbation will be discussed in the result section.

later it is mentioned that the oxidation was set to 0.5 as in Wania et al (2010).

We reformulated the sections about the oxidation factor to point out that we found it to have the same effect on the methane emissions as the CH_4 production factor. Therefore, different combinations of these two parameters lead to the same result in the optimization. We then chose our oxidation fraction according to literature values (which are in a broad range; p. 13253, line17ff.) and in agreement with Wania et al., 2010.

It is possible to get similar root mean square deviations between model results and observations with different combinations for the production ratio and the oxidation fraction. Therefore, we set our oxidation fraction according to literature values and in agreement with Wania et al., 2010 as a standard to the value 0.5.

Else, it is not clear how the parameters were optimized, and by how much the results deviated from the first guess.

The parameters were optimized over the range of values found in the literature. From all the combinations, we choose the one with the least RMSE of the spline fit to measurements over the time period where measurements exist. We added a sentence explaining this better in the manuscript in section 2.3.

If the RMSE is calculated from the daily data, then what is the purpose of the spline fit to the observations?

The spline fit considers also the uncertainty of the observation and averages over rapid CH_4 emissions fluctuations. It thus was preferred for being used to calculate the daily deviations. The text was clarified accordingly. (We also checked the outcome if we compare simulated CH_4 emissions to the unsplined measurements, i. e. only the days where measurements exist, to the according daily values of the model output. The same parameter set had the smallest RMSE).

Nominally daily data were applied to compute the root mean square error for each site between splined measurements and splined model output.

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).
It is actually just one scaling factor we applied. The simulated annual boreal methane emissions are scaled to 30Tg CH_4 for the end of the spin-up period. This factor is 0.6. We added this value to the manuscript. The scaling of the Tarnocai map to match an area of 1.048 mio. km^2 in the permafrost affected region in North America was not done for our study but to produce a map with realistic area and location of peat. Our scaling factor accounts for the small scale structure (lawns, hummocks).

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?
The heterotrophic respiration (RH) drops from 837 TgC to 739 TgC. A simple approach using the drop in RH as scaling for the drop in methane would therefore lead to only a decline in methane emissions by about 13%. The extended modeling approach yields about 19 % (Table 1). Furthermore, the more complex modeling approach allows us to incorporate methane isotopes in the future and to include further constraints for simulations. We clarified the text accordingly giving the percentage of change relative to initial values. We added to section 3.4:
A simple approach using the decline in RH as a scaling for the drop-down in methane would therefore lead to only a decline in methane emissions by about 13% while we find a decline of about 19% with the coupled process modeling approach.

MINOR COMMENTS
Abstract, line 22: “not completely” Does not seem an accurate representation of this fraction. I advise to quantify this contribution explicitly.
We added the number and rephrased the sentence:
If compared with the ice core record for the 8.2 kyr event, boreal peatland emissions alone could only explain 23 % of the 80 ppb decline in atmospheric methane concentration. This points to a significant contribution from low latiutude and tropical wetlands to this event.

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)
We agree that this statement needs some verification. We added in the discussion section a short paragraph for present literature discussing sources and sinks to justify this sentence in the Abstract:
This would suggest that variations in emissions from boreal peatlands contributed significantly to the CH_4 variations recorded in ice cores, but that other boreal and especially tropical CH_4 sources varied as well. Evidence for the tropical wetlands to give a significant contribution is for example discussed in Spahni et al., 2003. Levine et al., 2011 find that the change in CH_4 from the Last Glacial Maximum till the pre-industrial era was almost entirely source driven.

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?
We added some sentences to the introduction to explain the significance of the 8.2 kyr event.
The 8.2 kyr event is a unique abrupt cooling event in the records during interglacial conditions and a possibility to test our model for a similar decline in temperature. The Younger Dryas cold spell (12,800 and 11,500 years BP) was more severe than the 8.2 kyr event. Other abrupt climate variations like D-O-events are warmings.
The changes in temperature and atmospheric methane concentrations during these events are listed in the introduction.

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.
We agree on this and corrected this sentence accordingly.
[...] and is limiting the spatial resolution for top-down approaches.

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.
We agree that the minor changes in our ice mask (Peltier, 2004) do not tell that there are also just small changes in the peatland distribution. We changed the text in the introduction
This approach allows for a later comparison to the 8.2 kyr event as a comparison of the recent peatland distribution with the ice coverage 8200 yr ago (Peltier et al., 2004) shows that only very small areas of present day peatlands were covered by land ice at that time and the warm Holocene climate conditions prevailed already for more than 1000 yr allowing for a expansion of peat distribution to interglacial conditions.
Further, we mentioned this uncertainty in the discussion in the comparison between our idealised freshwater experiment and the 8.2 kyr event.

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?
We added “slightly” to the text and indicated a quantitative value over all sites and the improvement in RMSE per site. The according RMSE are the following:

RMSE of spline functions (as in Fig. 2 and Wania et al., 2010):

	Wania	Zürcher	Improvement
Michigan	226.4	209.0	8%
Minnesota	146.7	132.0	10%
Salmisuo	75.4	42.3	44%
Degeroe	50.7	43.7	14%
Boreas	61.3	59.7	3%
Abisko	62.8	41.9	33%

If we weight each site with the time length of the measurement we get an improvement of 30% for the site evaluation. We added these numbers to the revised manuscript in the appendix.

Page 13254, 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.
Correct. The described wrong distribution of substrate is originally caused by a wrong temperature distribution. We adjusted the text to make this more clear. HR is directly correlated to temperature in our model.

Page 13255, line 2: The 1048 million km^2 is incompatible with the 2.06 million km mentioned later.
Corrected; units were wrong. Not 1048 million km^2, but 1.048 million km^2

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?

This is now clarified throughout all the text (unit area always means per wetland area of the grid box).

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.
Yes, the slow and the fast pool were both considered in this number (page 13259, line 14). This change is 0.7 %. Only considering the sum of the fast pool, the litter carbon and the exudates, we get a change from 8.59 10^{16} gC to 8.37 10^{16} gC (2.6 %). The fast soil pool alone changes from 6.91 10^{16} to 6.75 10^{16} gC (2.3 %). We assume equilibrium conditions and therefore get relatively large soil pools. The temperature changes in the inland parts (not coastal) do not change 2-3° C, but more like 1° C. That is why all the soil pools do not change significantly. We added more information in the text and we will add a map of simulated peatland soil carbon stocks at the end of the spin-up (per grid-cell area).
In turn, the simulated total soil carbon inventory in boreal peatlands of 536 ± 0.1 PgC including the litter above and below ground, the exudates, and a fast and a slowly decaying soil carbon pool, remains virtually unchanged (532 ± 0.1PgC (-1%)). The changes in the fast pools only (exudates, litter and fast soil pool) are 2.6%. The decrease in respiration is thus the result of a decrease in the mean decomposition rate of organic material and not a decrease in soil carbon inventory. Note that the temperature and precipitation changes in the area of large soil carbon stocks are moderate (Fig. 9 and Fig.5a,b).

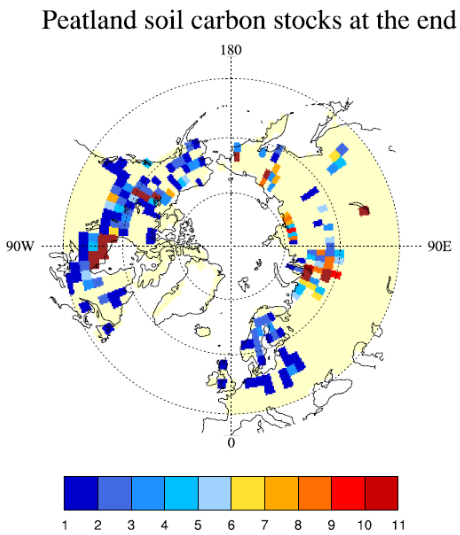

Figure 9

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)?
It is a general statement for the usefulness of including more complex processes in our model so it may better capture the full range of methane emissions. LPJ reacts more sensitive with the more complex structure (19% decline in methane emissions) than with a scaling of the RH (13% decline). We added these numbers to the text. But basically, yes it could be compared to previous modelling studies that used a fixed scaling.

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

In our study the maximum is 10 Tg/yr over the almost 700 years of the spin-up with a total source scaled to 30 Tg/yr. Spahni et al., 2011 show in their Fig. 8 anomalies in the innerannual varibility of the modelled northern peatlands emissions from -3 to 6 Tg CH_4 over 20 years. If we only considered the last 400 years of the control run, our variability would only be 7±1 Tg/yr and even smaller if we look only at a 20 yr interval.

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

We added a reference to Yu et al, 2010: Global peatland dynamics since the last glacial maximum; Geophys. Res. Lett.

Page 13274: It is unclear whether N_2 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.

There are no significant changes for global emissions or the site tuning when we change the gaseous volume to 10 % or 20 % for example. But there is a slight overall shift to higher values in emissions in general as a result. This is not visible anymore after our recalibration of the emissions after spin-up to 30 GtC /yr.

Kellner et al. 2006 for example assumed their bubbles to consist of
60 % CH_4, 10 % CO_2 and 30 % N_2.
In our runs, the partition between the gases [fraction in g] ranges from
12 % CH_4, 2 % N_2, 86 % CO_2 to
31 % CH_4, 20 % N_2, 49 % CO_2 (range over time and place)

The reason why we added N_2 to the calculation is to have a minimum gas volume in the ebullition routine independent of the variable CH_4 and CO_2 content.

We added an explanatory sentence to the appendix.

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.

We changed the title, added as suggested in both referee's comments several passages and rearranged the text.

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

The RMSE is calculated from daily differences between a spline fit through the measurements and the model output over the period where measurements exist. This has been added to the caption of Fig. 2.

TECHNICAL CORRECTIONS

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

Changed to:

Peatland emissions are equally sensitive to both, a change in temperature and in precipitation.

Page 13254, line 5: "shows that also . . . all of the sites". Sentence should be shortened and simplified.

Changed to:

However, even if we use site climate data at Sanjiang, emissions show a phase lag. This indicates that the seasonal change in the different emission processes is not accurately reflected by our model for all sites.

Page 13246, line 25: "approaches" instead of "attempts"
Page 13248, line 15: "sensitivity TO"
Page 13255, line 2: "Figure 4A"
Changed accordingly, many thanks.

Page 13255, line 6: "input has to be . . . preindustrial conditions" please reformulate.
Changed to:
The number of wet days per month is prescribed from the CRU climatology (New et al., 1999; Mitchell and Jones, 2005). Atmospheric CO_2 is set to a constant value of 279 ppm as found in ice cores for preindustrial conditions (Monnin et al., 2004).

Page 13256, line 1: "All LPJ simulations"
Page 13257, line 8: "response TO"
Page 13257, line 19: "Fig 5" instead of "Fig. 7"
All changed, many thanks.

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.
This is only part of the reasoning of the emission reduction. A too drastic reduction of ebullition would in some constellations lead to a greater build-up of methane in the soil. The other two transport ways would not be able to take away the same amount. We clarified this point and reformulated the sentence:
However, large reductions in emissions through one pathway would lead to different emissions. This has two reasons: On one hand CH_4 oxidation is different for each pathway, on the other hand high CH_4 amounts can build up in the soil in case of limited transport capacities.

Page 13264, line 5: closing parenthesis missing.
Added.

Figure 2: Abisko shows a spline fit to no measurement data.
Abisko is the only site that has daily data over the whole year available and was thus not spline fitted. The plotted line shows the measurements. It is true that we could also have plotted a spline for this site. We clarified the description.

Figure 8b: The line for "diffusion, large tiller seems missing"
We clarified in the caption that the diffusion is the same for both setups.
The total emissions and emissions by diffusion are identical for the standard run and the doubled tiller-radius run.