Author reply to referee comments on
'Uncertainties modelling CH_4 emissions from northern wetlands in glacial climates: the role of vegetation'

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We thank the referees for their constructive comments to our article. Following the referee comments, the paper has been thoroughly rewritten. Most changes are indicated below in our reaction to the referee comments, although not all text changes could be copied literally below for the sake of brevity. After the discussion of referee comments, a list has been added with further changes to the article.

Referee 1.

General.

The referee emphasizes the scientific merits of our paper, which we greatly appreciate. We also understand the criticism of the referee on the structure and clarity of our paper. Reading the review remarks, the model experiments should have been described more clearly. The suggestion to re-order sections 2 to 5 indeed improves the structure of the paper. Based on the remarks of the referee, we have rewritten and restructured the paper.

Major remarks

Sections 2, 4 and 5, i.e. the experiment description and the results section need to be rewritten more or less completely. So far they are inconsistent and the presentation of results is somewhat confusing. In table 3, the authors imply they do experiments on four classes of vegetation, shrubs, Carex, Sphagnum and grass, while the text in section 4 mentions only three dominant covers, Sphagnum spp., Cyperaceae spp., and shrubs. What happened to Carex? What oxidation rate / shoot factor can be expected for Cyperaceae? In Section 5, Fig. 2, this finally becomes Eriophorum, Sphagnum, Carex and shrubs. This ABSOLUTELY needs to be done consistently, the current inconsistent mixture is confusing and makes the reader wonder whether experiments were performed as carelessly as their description. Similar problems plague the presentation of results in Section 5, for example the “plant types” mentioned in Fig. 5 are explained nowhere.

Unfortunately, indeed inconsistencies have crept into the paper. This resulted in part from inconsistencies in the description of the vegetation classes. We have resolved these inconsistencies as follows.

1. *The number of vegetation classes has been restricted to three: Carex type, Sphagnum type and Shrubs. These are treated as end members of the wetland vegetations, and represent plant functional type rather than single species. For instance Carex includes other graminoid species like grasses and Eriophorum, which behave similarly with respect to CH4 fluxes.*
2. *Because of this reduction of the vegetation classes, and the availability of new data on vegetation-related model parameters, we have redefined the classes in table 1 and table 3.*
3. *Experiments involving the microbial methane production rate and its temperature sensitivity have been removed from the paper. These experiments were also included in*

Berrittella and Van Huissteden (2009). This helps to restrict the paper to a single theme, and simplifies the description and discussion of the experiments.

4. *Instead, we have included a Stage 3 Stadial experiment for the vegetation types, which is more illustrative of the effect of vegetation assumptions.*
5. *The number of figures is reduced from ten to seven. We have improved the figure captions.*

It might be a good idea to combine section 2 with section 4, since section 2 explains some of the experimental setup, while section 4 describes the other parts. In addition, table 1 should really be part of the section describing the experiments, i.e. section 4, since these are the parameters that are varied in the sensitivity experiments and results can only be understood with table 1 in mind. I therefore suggest to move section 3 in front of section 2 and to combine sections 2 and 4. Section 5 then would need to be checked for consistency with the experiment description. Much of section 2 and 5 is difficult to understand for someone who is not intimately familiar with the Walter/Heimann model, so some elaboration on the uncertain model parameters that are investigated would be warranted.

We have reordered section 2-5 as suggested by the referee. Section 3 has now become section 2, section 2 and 5 have been combined into a new section 3.

To section 3, a more extensive model description and discussion of vegetation-related parameters has been added. However, a full model description including the model equations is beyond the scope of the article and would increase its length considerably. We refer to the original model descriptions by Walter and Heimann, and Van Huissteden.
Also the description of the climate model input the methane model has been extended.

Minor remarks.

Table 1: Move Shaver et al. reference to bibliography and just cite it in the table. Parameter max primary productivity: I don't understand the explanation. Is something cut off? -
References are moved to the table caption and bibliography; explanation of max primary productivity is improved.

Q10 is not a temperature correction, but rather a factor describing the temperature dependence of the reaction. Units should be set nicer, for example kgC/m2/day should really be kgC/m2/d, and MicroM/h should be μM/h.

Corrected

Section 2: Lines 138-143: The climatic boundary conditions for the experiments are unclear. You compare what exactly to modern climate? "Stage 3 climate model runs" is not referenced, and a sentence clarifying that it is MIS3 interstadial conditions that are imposed would be really helpful. In addition, the reader is left wondering how much of a difference this different climate actually makes, so some numbers characterising the differences would be good, i.e. the change in annual mean temperature, summer temperature, winter temperature and annual total precipitation as a mean for the model domain.

We added to the description:
The model experiments link the PEATLAND-VU model to output of the 'STAGE 3' climate model experiments (Barron and Pollard, 2002). These climate model experiments simulate

the Last Glacial stadial and interstadial climates, together with a modern climate control experiment, at a high spatial resolution over Europe. These simulations focus on the climate of the marine isotope Stage 3 (MIS3) stadials and interstadials.

We also added the mean annual air temperature, January temperature, July temperature and annual precipitation, calculated over the land grid cells of the model domain, to the text.

Line 142: "the LG interstadial" is rather misleading, since there were more than one interstadial during the last glacial cycle.

With the LG interstadial, we do not indicate a particular interstadial, but rater the average MIS 3 interstadial as modeled with the climate model experiments. This has been cl;arified as follows:

We use here the climate model simulations for stadial and interstadial conditions, representing a typical MIS 3 stadial and interstadial (MIS3COLD and MIS3WARM simulations respectively), and the present-day climate control experiment (MODERN herafter).

Section 3: Lines 175-176: the authors seem to be implying that there is a systematic flaw in the concept of PFTs. Is that the case, or do you just mean that the PFTs, as they are usually defined, simply lack the information on CH_4 transport? Please clarify.

We did not mean to imply a systematic flaw in the PFT concept, but rather wanted to emphasize the lack if information on CH_4 process-related properties. We rephrased this section as follows:

Only limited quantitative data are available on vegetation characteristics that influence CH_4 emissions. For global scale modeling of methane emissions it would be useful to extend the widely used concept of Plant Functional Type (PFT) with these fundamental aspects of the carbon cycle. Wania et al. (2010) introduced two PFT's specifically for wetland methane modeling (flood-tolerant C_3 graminoids and Sphagnum mosses). Similarly, we grouped wetland plants, according to their CH_4 transport and oxidation capacity, suited to model CH_4 emissions.

Section 4: Line 268: Fig. 1 does not contain a map of wetland distribution. Either add a map or remove the reference in line 268.

This is an error, the wetland distribution map has been published in Berrittella and Van Huissteden (2009). Removed.

Section 5: Line 287/288: It would be preferable to have emissions in SI units, which in the case of Gt/yr would be Pg/a.

The referee is right that SI units would be preferable. However, Gt/yr was used in our previous article, and we prefer to use the same units throughout both articles.

Line 301/302: Sentence unclear. Please rephrase.
Line 330-333: Sentences unclear. Please rephrase.

Rephrased. The entire section has been revised thoroughly.

Bibliography:
Formatting of the bibliography is not done consistently. Petrescu et al. 2010 is set

single spacing, not double spacing as the rest of the document, the same goes for van Huissteden et al. 2009; for Valdes et al. 2005 the journal name is misspelled and should always start with capital letters (i.e. “Geophysical Research Letters” instead of “Geophisycal research letters”); Raghoebarsing et al. 2005 contains a spurious “|” character instead of a comma in front of the doi section.

The bibliography has been checked thoroughly

Figure 1: The colours appear not to be chosen optimally. Under less than perfect lighting conditions, it is hard to distinguish green from light blue, making it difficult to see the additional land area

A different color scheme has been chosen, the Stage 3 stadial climate model domain has been added.

Figure 2: “Warm Climate” in the figure heading is misleading and is inconsistent with other figures as well. Why don’t you change that to “ST3 warm” as in Fig. 1? Emissions units abbreviated as Gtons/Y for Gigatons/year is rather unusual. Usually that would be abbreviated Gt/yr, and the corresponding SI unit would be Pg/a. Since SI units usually are preferrable, I’d suggest using the latter.

Swapping the land and sea floor fluxes (i.e. sea floor flux on top of land flux) would make comparison between ST3 warm and modern easier, since modern doesn’t have the sea floor fluxes. “Scrubs” should really be “shrubs”, I guess, and finally the reader is confused by the sudden appearance of Eriophorum, which is never mentioned in the text (except for Table 2 – it took me quite a while to find it...). Please do one of two things. Either call it “grass” in the figure, or mention Eriophorum in the text, for example in Section 4 and in table 3.

Figure 2 has been replaced by the current figure 7; titles and units corrected.

Figure 3 and 4: Emission units
- added; new figure numbers are 2 and 3

Figure 5: Emission units, it should be “flux” instead of “fux”, and since the Walter/ Heimann “plant types” shown on the x axis are never mentioned in the text, the figure cannot be understood by the reader. Please redo.
- corrected; new figure number is 4

Figure 6: Units; it would be less confusing if the x axis showed the actual exudation value, not 10 times the value.
- corrected, new figure numbers is 5

Figure 7: Flux units. *Figure removed; it is obsolete, since a similar experiment was also included in Berrittella and Van Huissteden (2009); see also 'further changes' below.*

Figure 8: Flux units, “warm climate” should be “ST3 warm”. In addition, the values shown on the x axis will be confusing for the anglo-saxon readers, since they expect a

decimal point, not a comma. “roots depth” is usually called “rooting depth”

corrected, new figure number is 6.

Figure 9: Flux units, “Shrubs” instead of “Scrubs”

Figure 10: Flux units.
Both figures are removed, see explanation under 'Major remarks'.

Referee 2 (Editor's comments).

Referee 2 has similar remarks on our paper as referee 1. We seriously apologize for having submitted a paper in which there are several inconsistencies in the text. There has indeed been an element of haste in the preparation of the paper, which we hope to avoid in the future.

Specific comments.

Abstract. The abstract should be completely rewritten. The main qualitative summary of the paper is in the first two sentences, while the rest of the text rephrases the introduction section. What are the main paper results? What is a relative importance of vegetation types, root exudation, oxidation rate, Q10, water table level, etc. for the methane emissions? What is a difference in model sensitivity between present day and glacial conditions? The model findings regarding sensitivity should be clearly highlighted in the abstract, as well as in the conclusion section, preferably in quantitative and not qualitative terms.

We agree that the abstract needed improvement. It has been revised thoroughly and expanded with the conclusions of our model experiments.

Results. The results section is a compilation of figures with very little analysis done. The language is sloppy, e.g. section 5.1 is called “sensitivity OF vegetation” while it should be a “sensitivity TO vegetation” if I understand the paper correctly. Many figures with few data points (e.g., 2, 4, 6) could be effectively replaced by tables. Sensitivities to different parameters much be compared among each other so that a general conclusion about most sensitive model parameters could be reached.

This sections has been revised and expanded as well. We added for each experiment a description of the model structural features that cause the observed model response. Some experiments and features have been deleted because they are ubiquitous and partly published before (see answers to referee one, and 'Further changes' below). We prefer figures over tables, even if the amount of data points is small. Tables do not take less space and figures are more easy to interpret visually. We added a comparison of the sensitivities to the different parameters.

Discussion. I recommend to combine results and discussion sections since there is little observation data or other studies to compare with the model results. There is not much what can be discussed beyond analysis of the results.
What I miss in this section is a discussion of model limitations. For example, does the model accounts for the permafrost carbon? If not, what is a possible effect of this on methane fluxes and hydrology? What are other known model deficiencies essential for different model applications?

We preferred to keep the results and discussion sections separate. Both sections have been expanded cansiderably. The Discussion section now starts with a discussion on the model limitations, including how permafrost carbon is treated in the model.

Conclusions. What are the main findings of the paper? What are the most important model parameters? As I said above, a comparative analysis of effect of different factors

on CH4 emissions should be included.

The conclusion section has been modified to properly highlight the main findings of our paper

Further changes

Reference Goel et al, 2008 replaced by Schaepman-Strub et al., 2009 (more relevant reference)

Section 2 (old section 3), more extensive description of the CH_4 transport processes in the first paragraph.

Figure 2 (former figure 3): last values for f_{ox} ($f_{ox} > 0.9$) deleted (these are unrealistically high)

Deleted: the experiments with methane production rate and Q10. These experiments have been done also in the first article, the only difference is that here we include the effect of the water table model. This effect proves to be negligible.

Data from new literature has been added to the quantification of vegetation parameters in section 3. For that reason, also the experiments with the vegetation types have been re-run.