Interactive comment on "Modelling methane emissions from natural wetlands: TRIPLEX-GHG model integration, sensitivity analysis, and calibration" by Q. Zhu et al.

Anonymous Referee #3

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In this paper, Zhu and colleagues display a description of a new methane module called TRIPLEX-GHG as part of the IBIS dynamic global vegetation model. They do a very thorough review on methane modelling and existing methane models, and from there describe their implementation of biogeochemical equations for methane emission modelling in global wetlands in TRIPLEX-GHG. Model behaviour is judged on a com - parison to 19 measurement sites and areas, which are more extensive than previous models have undergone. Unfortunately, there is very little new science resulting from this comparison. In addition conclusions are more general and descriptive, rather than informative. I understand that GMD is probably a journal to publish such an evaluation, but I see good potential to make it scientifically more rewarding and insightful. Thus I will add some suggestions below for a major revision.

RE: Thank you very much for your constructive comments and suggestions. This paper described the first step of our modeling framework and we mainly focused on the model development, sensitivity analysis, and calibration. We are in the process of conducting global simulation for our ongoing work. The spatial and temporal patterns of global CH_4 emissions and the relationships between different factors and global CH_4 emissions, as well as the spatial variance of methane fluxes for the different transport pathways will be conducted and reported in next step.

We have tried our best effort to improve the quality of this MS by adding more new results and revising the conclusion section.

General:

My main critics concern the comparison of model output to site data analised by con - tinent/country. I would suggest to make an analysis based on the ecosystem charac - teristics rather than the location of the wetland. This could include several aspects and groupings, such as the vegetation type (GOD, structure, PMT capability, NPP, RH, etc.), the wetland type (peatlands, marshes, flooded forest, saturated soils, etc.), or the soil type (porosity, pH, freezing depth, etc.). This information could either come from observations or from the model, and could be presented in a table. In a second phase the impact on methane emissions could be analysed from these differences and the model equations, and possibly could help to explain why the optimised Q10 is so different for all these sites. Here directly follows my second point: the reader is offered very little information from the underlying IBIS DGVM that defines the structure for the methane module. The soil biogeochemistry and derived carbon fluxes are very important for methane production and should be given a place in this study as well.

RE: Yes, we understand your concern and agree with your suggestion. Currently, we added only one more plant functional type (PFT) for wetlands simulations and we did not specify the wetland types in the current model. The soil classification for each site was based on the Digital Soil Map of the World (DSMW), in which the soil information is relatively coarse. Maybe it's not proper to group the results by vegetation type, wetland type, or soil type. However, we regrouped the study site by biome types (including tropical, temperate, and boreal), which was also suggested by another referee. The corresponding result section (section 4.2) was reorganized.

Following the suggestion, we analyzed the different patterns of the optimized parameters based on the biome types grouping. For the second point, we added detailed information for the soil biogeochemistry and heterotrophic respiration in section 2.2.

Specific:

- TRIPLEX-GHG: What does TRIPLEX-GHG mean? Is it an abbreviation for triple GHG: CO_2, CH_4, N_2O?

RE: Yes, you are right. It is an abbreviation for triple GHG: CO_2, CH_4, N_2O. Only CH_4 was focused in this paper.

- p.5425ff, There might be some updates in the new IPCC report for all places, where you use the Denman et al., 2007 reference.

RE: Yes, we checked the IPCC 2013 report and rewrote some sentences with update references.

- p.5426, l.7, also cite Kirschke et al., 2013

RE: Yes, did as suggested.

- p.5427, l.12, typo, delete"." after Petrescu et al. (2010)

RE: Yes, revised.

- p.5428, l.13, also cite Stocker et al., 2013

RE: Yes, did as suggested.

- p.5428, l. 26, typo: "a CH_4 emission model"

RE: Yes, revised.

- p.5429, I.15, Is the C3 plant the only PFT for wet lands? In Fig. 1 it is shown that there is a feedback from the wetland PFTs to the plant physiology, but if only PFT is adapted to inundation there will be no competition between PFTs. Please explain.

RE: Currently, only one PFT was added only for wetlands without considering specific wetland plants type (e.g. graminoids, sedges, sphagnum, moss etc.). Most of the parameters were adopted from the C3 PFT in original IBIS model. The competition mechanism between PFTs applied in the origin model was not fully considered in wetland area. The main purpose is to incorporate inundation stress effects on gross primary production of the plants in wetlands, as the carbon substrate supply from plant primary production is one of the important factors affecting CH_4 production. The photosynthesis capacity of the added PFT is modified following the assumption made by Wania et al. (2009).

- p.5430, l.21, What happens if the soil is partially frozen? Will water add on the top of the freezing depth?

RE: Good question. Fraction of soil pore space containing ice is considered in the simulation. Soil porosity will be reduced while more water is converted to ice, and also the WFPS will be changed. With the changing of soil temperature, liquid/ice contents will change below/above melt point. For each soil layer, liquid will be frozen to ice as soil temperature below melt point, while ice will be melted to liquid as soil temperature above melt point.

- p.5431, l.9, It was explained that the water table is dividing the soil in an anoxic and an oxic layer. How many layers are considered for the methane module? How deep goes the soil in the model?

RE: Good question. The soil layers is 6 and the soil depth is set to 4m as present in the original IBIS model. The thermal and water balance processes were inherited from IBIS model. For the wetlands methane module, to simulate the dynamics of water table, we divided the soil profile into 30 layers (1cm per layer) above the maximum water table depth (30cm). Thus 30 layers were used to simulate the water table changing and methane emission process. The soil above maximum water table depth were separated as anoxic and oxic zones by water table, where CH_4 is produced and oxidized, respectively.

- p.5432, eq.(2), Is RH already temperature dependent in the IBIS model? If yes, is this accounted for in the fST factor to prevent double temperature effect for production?

RE: In IBIS model, HR is calculated as the carbon deficit of all soil carbon pools with each time step. The decomposition process is still happen when the temperature below zero degree Celsius. In eq.(2), the f_{STP} factor is used to adjust the release ratio of CH_4 to CO_2. Methane will only be released within proper soil temperature range (above zero degrees Celsius and below an high temperature) controlled by f_{STP}. There is no "double temperature effect for production".

- p.5432, l.7, add "degrees Celsius" to "zero".

RE: Did as suggestion.

- p.5433, l.20, Please mention here that soil pH is prescribed from a map.

RE: Did as suggestion.

- p.5435, eq.(7), Are fST and fEh the same factors, or parametrization, as in eq.(2)? If not, please use a different name.

RE: Good observation. The f_{ST} and f_{Eh} are different in these two equations. Yes, as suggested, we changed f_{ST} and f_{Eh} to f_{STP} and f_{EhP} for CH_4 production process, to f_{STO} and f_{EhO} for CH_4 oxidation process in revised MS.

- p.5437, l.3, Do all wetland PFTs have aerenchyma, or do you simulate plants without, like e.g. sphagnum?

RE: The plant aerenchyma factor estimated as a function of root length density (converted from root biomass using a specific root length of 2.1 cm mg^{-1}), the area of the cross section of a typical fine root (assumed as a constant of 0.0013 cm^2), and the degree of gas diffusion from root to atmosphere (a scalar determined by the aerenchyma condition of plants). We did not specify different type of wetland plants (e.g. sedges, sphagnum etc.) currently. A simplified constant of 0.5 was used as the degree of gas diffusion from root to atmosphere in this study, since the value should be 1 for the plants with well-developed aerenchyma (e.g. grasses and sedges) and be 0 for the plants without aerenchyma (e.g. sphagnum and moss). We rewrote the part and added above details in the revised version.

- p.5437, l.15, I understand that you use 30 years of climate repeatingly for spinup, but how long is the spinup period?

RE: The spin-up period is 300 years and the soil biogeochemical process is called 40 times each day during the acceleration procedure.

- p.5438, l.3, Does your soil carbon data include peat soils with a high porosity? What are the porosity values used at the different sites? For eq.(8) this seems to be an important parameter.

RE: That's a good point. Since the specific soil information for each site was unavailable, a global soil dataset was used in this study. The soil classification for each site was based on the Digital Soil Map of the World (DSMW). The DSMW attributes were connected with the soil properties dataset contributed by Batjes (2006) that describes characteristics of soil texture (soil clay, sand, and silt fraction) and soil pH. The soil was classified using the USDA textural triangle based on the percentage of sand and clay and a soil porosity of each soil layer was assigned according to the soil type. The top layer was assigned with a high constant porosity of 0.9.

- p.5441-5456: I find it very hard to read through section 4.2, and I suggest to organize that part as mentioned above. A very interesting illustration would be to show methane fluxes for the different transport pathways, or what percentage of the production is ox -idized over the course of a year at each site. This could tell us much more about the separation of carbon fluxes during plant production, heterotrophic respiration, methane production, methane oxidation and emission. These numbers would come all differently in relation to each other at the 19 sites, or maybe not

RE: Sorry for that. Yes, we have reorganized section 4.2 by re-grouping all the study sites with biome types of tropical, temperate, and boreal which was also suggested by another referee.

We totally agree that it is important to illustrate methane fluxes for the different transport pathways. In this study, we focused on the model development, sensitivity analysis, and calibration. This is the first step of our ongoing research. We are in the process of preparing new model runs and simulations in which a global simulation will be conducted. The spatial and temporal patterns of global CH_4 emissions and the relationships between different factors (including extreme climate events, patterns of atmospheric CH_4 concentration) and global CH_4 emissions, as well as the spatial variance of methane fluxes for the different transport pathways will be investigated in next step. We added this point in the discussion section.

- p.5448, l.7, Is this a somewhat biased by the fact that most sites lie in the northern hemisphere, or is this generally true for frozen soil conditions?

RE: Thank you for this good point. Your conjectures are right.

In our previously sensitivity test, only two sites located at boreal area were included, and analysis indicated that the selected model parameters are much more sensitive during winter than summer. In the revised paper, we redid the initial sensitivity test at three sites with different biome regions (boreal, temperate, and tropical). We found that the seasonal patterns of sensitivity index of Q10 described in the original text is only true for boreal and temperate regions, while reverse patterns were found for tropical regions. We rewrote this part.

-Table 3, I find this table of little help, and maybe can be omitted. Are values given as relative changes, e.g. +/-0.05 =+/-50% of 0.2? Please clarify.

RE: Agree. This table was deleted as your suggestion and relative information is described in the text. The changing step is absolute values instead of percentages.

- Fig. 3, Please explain units in Y-axis, is it percentage or absolute values? Also give in the caption again that it is the sensitivity index that is shown.

RE: It is absolute values. Figure 3 was reproduced and shown as Figure 2 in the revised paper. The coordinate was clarified.

References:

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RE: Thanks for the suggestion. The references were added in the revised version.