The review result and comments on Dec 31, 2013

Title: Modelling methane emissions from natural wetlands: TRIPLEX-GHG model integration, sensitivity analysis, and calibration

- Review decision

Major revision is needed based on updates of modeling using new input data, reorganizing sections, and improving analyses and presentation of language. The paper will be rejected if author cannot address whole details of comments.

- General comments

In summary, this paper is interesting as a new GHG model of TRIPLEX-GHG was developed and applied for the CH_4 flux simulations over many sites in the world. I found heavy work on model development and modelling studies of parameterization and evaluation were done – it can contribute to model scientific community to accounting for GHG exchange, especially CH_4, from the global terrestrial systems and the developed model can be a potential of the another method of GHG exchange estimates to better understand the change of GHG budget at a small or large scale of global territory. However, unfortunately this paper overall produces weak sciences and had serious concerns on modelling approach, organizing all sections and analyses of data, and poor presentations of writing. These all generate negative feeling on the paper although the study pursues an important topic and finished heavier tasks of simulations. Please see below all my comments on major specific comments and minor comments or corrections. I'd like to see again how this paper will be improving with accepting my inputs along with others.

RE: We'd like to give great appreciation to the referee for the detailed and constructive comments and very good suggestions. In fact, this paper described the first step of our modeling framework. We mainly concerned on modeling approaches and 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 (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 conducted and reported in next step.

According to the referee's comments, we collected new local station climate input data, instead of global climate data, for site-based simulation. The model re-ran and the text was rewritten. All relative figures and tables were revised and updated. More model evaluation indexes were calculated and added in the revised MS. The new results were reorganized based on biome type as suggested. Details are listed on the following pages.

- Specific comments

1. Major comments

1.1. Current introduction failed to state the points of why the previous modelling approaches cannot fully support CH_4 simulations of the wetlands so need a new model and apply in to the areas. Authors should develop better introduction and grab the importance of necessity of a new CH_4 model in place.

RE: Yes, we agree. We rewrote this part, deleted redundant information, addressed the limitations of previous modelling approaches, and make it concisely for highlighting the motivation of our CH_4 model development.

1.2. The model descriptions (such as equations) and methodology sections for data and site information should be more concise and be reorganized to be logical and easy understanding of the study approaches to readers. I checked up redundancy of same description for the modelling method – they should be cut out.

RE: Thank you for your good suggestion. In the revised MS, some parts of the model description section were rewrote and the redundancy text was deleted, including:

In section 2.2.1:

The first graph was rewrote;

The beginning repeat sentences were delete in second graph;

The descriptions for two parameters (temperature effects (Q10) and the release ratio of CH_4 to CO_2 (r)) were moved to the section 3.

The descriptions of the Eh effects in the penultimate graph was rewrote and some redundancy text were deleted.

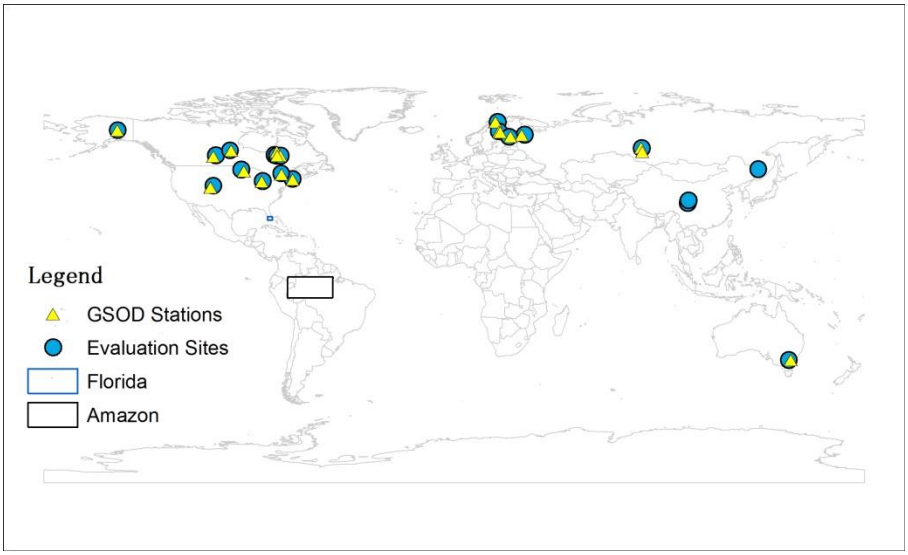
In section 2.2.2, the first graph was reworded.

In section 2.2.3, the redundancy descriptions in the first graph were all deleted

The data and site information parts (section 3.1 and 3.2) were rewrote and reorganized.

1.3. I am critical that using global climate data and running model thereafter are wrong directions. There are much local and site-special data in the web and near the measurement site and they are available. The tiny fluxes of CH_4 are very sensitive to climate conditions, so global climate cannot represent CH_4 processes from the observation sites where the model evaluations were carried out. I highly recommend using new datasets of climate and others (if available) and rerun model and include updates of comparisons with the observation.

RE: Thanks. According to your suggestions, we used new site based and local climate data (GSOD) to rerun the model as well as did new comparisons with the observations. The GSOD includes various daily summary elements such as temperature (mean, max, min), dew point, wind speed (mean, max, peak gust), precipitation, pressure, visibility, snow depth, etc. The daily climate data (mean, max, min temperature, precipitation, mean wind speed, dew point) were downloaded from the nearest stations to the evaluation sites (http://www7.ncdc.noaa.gov/CDO/cdoselect.cmd?datasetabbv=GSOD&countryabbv=&georegionabbv=). The selected stations are checked to ensure covering the observation periods of the methane evaluation sites. The distribution map and the location information of stations are shown below:



CH4 evaluation sites	GSOD Stations	GSOD station ID	GSOD Latitude	GSOD Longitude	Data available
Stordalen	KATTERJAKK	020200	68.417	18.167	19730101-20091231
Degero Stormyr	VINDELN/UMEA	022740,022860	64.217/63.8	19.717/20.283	19780101-19981231
Salmisuo mire	JOENSUU	029290	62.667	29.633	19800101-19981231
Ruovesi	HALLI	029450	61.850	24.800	20000101-20071231
Plotnikovo West Siberia	BAKCHAR/KOLYVAN	293280,296310	57.000/55.300	82.067/82.75	19800101-20031231
Fairbanks Alaska	FAIRBANKS INTL ARPT	702610	64.804	-147.876	19800101-19921231
Minnesota	DULUTH INTL AIRPORT	727450	46.837	-92.183	19790101-20091231
Michigan	JACKSON CO REYNOLDS	725395	42.267	-84.467	19810101-19941231
Sallies Fen	PEASE INTL TRADEPOR	726055	43.083	-70.817	19900101-20021231
Loch Vale,Colorado	EAGLE CO RGNL	724675	39.650	-106.917	19920101-19991231
BOREAS SSA	PRINCE ALBERT ARPT	718690	53.217	-105.667	19800101-20091231
BOREAS NSA	THOMPSON AIRPORT	710790	55.800	-97.867	19880101-20091231
Quebec	KUUJJUARAPIK ARPT	716278	55.283	-77.750	19870101-20091231
	LA GRANDE RIVIERE	718270	53.633	-77.700	19870101-20091231
	NEMISCAU ARPT(SAWR)	718113	56.700	-76.117	19940101-20091231
Mer Bleue	OTTAWA RECREATION C	710630	45.383	-75.717	20020101-20091231
Ryans 1 Billagong	CORRYONG PARISH LAN	948990	-36.200	147.883	19910101-19961231

Since model evaluation for Florida and Amazon were based on the regional simulation, the CRU gridded climate data was still used for these two areas. For the evaluation sites in China presented in our previous simulations, the daily climate data was obtained from the nearest corresponding meteorological stations of China instead of using the CRU data. These information did not state clear in our previous version of manuscript and are added in the revised version now.

New model simulations using new datasets were conducted all the sites excepting for Florida area, Amazon area, and two sites in China. All the figures were updated and all the statistical indexes were re-calculated for all the sites.

1.4. To me, grouping of the study sites by country doesn't make sense. It looks like grouping with biome type, such as tropical, temperate, boreal, etc. can generate more sense of site arrangement and the following studies of calibration and evaluations of simulations. Through the view in differences of model performance by biome types, authors can find out more on degree of model uncertainty and weakness in processes.

RE: Yes, we agree. We re-grouped the study sites with biome types of tropical, temperate, and boreal as suggested. The corresponding text and figures of this part were reorganized in the revised MS. Discussion on the different patterns of the optimized parameters based on the biome types grouping was added.

1.5. The analyses of model evaluations are very weak and I highly suggest doing further statistical analysis using few more indexes, such as bias, r2, percentage of RMSE, and index of agreement (see Willmott 1985, JGR as an example). The average numbers of simulations and observation should be added in all the figures, and the other indexes should be inclusion there.

RE: According to your good suggestions, we added more indexes to evaluate the model performance including: RMSE, R^2, and index of agreement (D). Monthly observed and simulated emission rates are generated for each individual site to calculate the indexes. The figures were reorganized and the statistical results were presented in Table 3 in our revised version.

1.6. I don't think the processes of initial sensitivity analysis are not demanded in the flow of study and they cannot cut off. The two important parameters could be selected through the initial check of large range of values and review of previous sensitivity results about original model equations. No reason of such step is required. Instead, sensitivity analysis of major inputs or parameters would be conducted and results of this can be shown in the last section of result. I highly recommend looking through papers or books of ecosystem modelling (Aber 1997, Bulletin of the Ecological Society of America – titled Why don't we believe the modeld?) and redesign the prerequisite steps of work.

RE: We totally agree with the referee's opinions and comments on normal modeling sensitivity analysis.

Here, we would like to explain our original intention in more clear way. After the development of the model, we intended to find the most sensitive parameters of the model and the fewer the better. This will make the model more applicable for different conditions and locations since only very few parameters needed to be adjusted. Based on previously studies, we selected three parameters (the release ratio of CH_4 to CO_2, Q10 for CH_4 production, and Q10 for CH_4 oxidation) which are always considered as basic but critical sensitive in the methane emission processes. Then we conducted an initial sensitivity analysis and found two of them are the most sensitive ones (the release ratio of CH_4 to CO_2, Q10 for CH_4 production). By doing the site-based simulation, we conducted the parameter fitting exercise to determine the best combination of the parameters which could lead the smallest error and the model performance was also evaluated. We found the model performed quite reasonable across different global sites under different site conditions (by adjusting only two parameters). With more site-based model simulations, we would obtain more suitable parameters sets under different site conditions and locations, and then, we can construct spatial distributed parameter sets by a specific land surface classification (wetland types, biome types, etc.). As we know, spatial distributed model parameters are still not included in the state-of-the-art Wetland and Wetland CH_4 Inter-comparison of Models Project (WETCHIMP). We believe this approach will be important for the evaluation of wetlands CH_4 emissions, especially at regional or global scale.

1.7. The calibration and evaluation period should be separate and they cannot share with the same period. I suggested the first half for calibration and the later for evaluation. The paper should reanalyse data and create figures or tables on the basis of two independent periods

RE: We agree with your good idea and appreciate your good suggestion on this. As we mentioned in the reply to point 1.6, we tried to find the best parameters combination set for different site, and then to analysis the patterns of the selected

parameters and to construct a spatial distributed parameters set. Unfortunately, we had very limited observation data with short period. So, we tried to use all the available observed data (as much as possible) to do the parameter fitting processes and did not separate the limited observations into two periods for calibration and evaluation.

1.8. Results must contain the initial conditions of soil and biomass inputs (C or N mass) that were driven by spin-up simulations, can be present as a table.

RE: Yes, as suggested, the initial soil carbon and biomass generated by spin-up procedure were added to Table 3 in the revised version.

1.9. Discussion should be concise and add only essential points in terms of the current simulation results and model developments. The potential model uncertainty and further developments and applications would be added. Authors should concentrate on discussion for the ebullition process and its contribution to the high peak. It is very common knowledge that high peaks of CH_4 emission are relevant to the ebullition and its process are hard to be predictable. I was disappointed with the fact that authors didn't describe about this and lead discussion on this and the developed model's efficiency on the bubble predictions. Some parts of discussion are not useful.

RE: Thanks for the constructive suggestion. We added discussion of the ebullition process and its effects on high emission peak modeling. We also added discussion about the next step of model application and further development. Some redundant and unuseful parts are deleted especially in the first and fifth paragraph of discussion section. Most of the discussion section was rewrote.

2. Specific comments by line

2.1. Title: suggested as 'Modelling methane emissions from natural wetlands by development and application of the TRIPLEX-GHG model'

RE: Yes, we changed the title as suggested.

2.2. P5425 L20-24: check CH_4 is 25 times stronger global warming potential referred to by IPCC report (e.g. Forster et al. 2007 from IPCC 2007 report) then rewrite

RE: Yes, we rewrote the sentence by updating the number (28 times) and the reference. The new IPCC 2013 report was cited here (Myhre et al., 2013).

2.3. P5426 L20 – P5427 L 22: mass of information on the model introduction are not useful and didn't address the weakness of these models and what updates should be followed in a new model – that is actually the initial phase of the objective of this paper. Make concise only to stress the previous model studies

RE: Yes, we agree. We revised this part, deleted redundant information, and added new descriptions on the limitations of the previous models.

2.4. P5427 L23 – P5428 L17: seems that authors failed to state why a new model is needed and gaps of CH_4 estimation should be filled with the new development. Add this clearly and make concise this part

RE: Yes, we rewrote this part and make it concisely for stating our model development

2.5. P5428 L17: start with a new paragraph
RE: Yes, did as suggested.

2.6. P5429 L2 – L1: this part is confusing. Rewrite. Suggested as delete 'framework' of L2, reference of L3, and sentence of L5-6
RE: Yes, rewrote as suggested.

2.7. P5430 L3: change as 'of natural wetlands in addition to peatlands'
RE: Yes, did as suggested.

2.8. P5430 L17: why 30 cm of max water table depth?
RE: We took this number from previous studies (Granberg et al., 1999;Zhuang et al., 2004;Frolking and Crill, 1994). At the wetland site experiment, Granberg et al. (1999) found that the lowest recorded water table position was 25cm below the vegetation. The fluctuations of water table is mainly caused by precipitation and evapotranspiration. The evapotranspiration will reduce drastically as the water table dropping to 33cm because capillary fringe cannot reach the plant roots in wetland (Boelter and Verry, 1977). The maximum water table depth is suggested to be 30 cm in those studies and we adopted this value in our study. References were updated in the revised text.

2.9. P5431 L22–27: repeat of description. Delete
RE: Yes, did as suggested.

2.10. P5432 L12: define freezing point and extremely high temp limit
RE: The value of temperature was defined in the revised text according to the comment.

2.11. P5433 L1–11: wonder why Q10 is highly varying over the ecosystems. It should be worth to say the parameter should be calibrated.
RE: Yes, observed Q10 values ranged from 1.7 to 4.7 and generally increased with increasing substrate availability(Valentine et al., 1994). This high degree of uncertainty may be caused by the influence of substrate availability, as well as plant growth and organic matter decomposition (Cao et al., 1996). We presented the highly varying Q10 here and calibrated the value to obtain an optimal value for each individual site. The part is moved to the sensitivity analysis section in the revised MS.

2.12. P5433 L21: in this paragraph, how water table depth control the CH_4 simulation should be described
RE: Yes, we added some description as suggested.

2.13. P 5437 L8: should add a section regarding explanation of input variables and parameters mainly used in simulations. They can be present in a table.

RE: Yes, we added a section 2.2.4 and present the major parameters as Table 1 here.

2.14. P5437 L15: if spin-up runs were done, the results should be in place in the result sections. I am very interested in seeing the spin-up initial values.

RE: The initial soil carbon and biomass generated by spin-up procedure were added to Table 3.

2.15. P5438 L8: detailed information of study sites are placed here; delete site description in the result section (P 5439). 3.2 titled as 'study sites'

RE: Thanks for the good points. We moved the study sites information from the result section to section 3.1.

2.16. P5438 L14: revise this section with deletion of unnecessary parts and the section titled as simply 'senility analysis'

RE: Yes, we rewrote this part as suggestion.

2.17. P5439 L3: section of detail model evaluation (calculation of statistics index) should be follow here

RE: Yes, we added description of evaluation index including RMSE, R2, and index of agreement (D) as a new section of 3.4.

2.18. P5439 L6: revise the entire section and delete the sensitivity parts

RE: Yes, we rewrote this section as suggested.

2.19. P5439 L7: Table 2 should be reformatted and move to the methodology section

RE: Yes, the Table 2 has been moved to the section 2.2.4.

2.20. P5440 L7: many of this section include descriptions on method. Delete and move to the method section.

RE: Yes, we have moved the first two graphs to the section 3.1 and rewrote this part.

2.21. P5441 L22: how to say good agreement? What criteria of it? Define it

RE: Good point. We added the criteria index values here.

2.22. P5444 L17: Table 5 can be replaced with a figure. Table 6 is as well.

RE: We changed and combined Table 5 and Table 6 into Figure 5 as suggested.

2.23. P5446 L 12: T g C yr-1 should be converted as the unit of g C m-2

RE: This number is the total annual CH_4 emission for the Amazon Basin and the unit TgC/yr is correct.

2.24. P5448 L25: try connection to the oxygen profile in addition to temp and include the discussion about the effects of oxygen on CH_4 emission

RE: Yes, we added the effects of oxygen condition here and added some discussion about effects of soil oxygen profile on modelling in CH_4 emission processes.

2.25. P5449 L9: show NPP and HP data if simulated, and have a brief comparison between simulations and measurements

RE: Good point. Here, we just wanted to discuss that the biases in net primary productivity and heterotrophic respiration could have effect on CH_4 production. We did not output them in our study for comparison analysis because the observed NPP and HR are not available for each evaluation site.

2.26. P5449 L20–24: unnecessary part. Delete

RE: Yes, did as suggestion.

2.27. P5450 L12: in the next paragraph, authors can summarize the next stages of model applications and further developments to have better simulations of CH_4 (remove uncertainty)

RE: Thanks for the excellent suggestion. We added a paragraph here to discuss our next step of modelling and data collecting works to improve the model performance.

- Technical corrections (minor comments)

There are lots of writing errors and typos. I'd like to correct only a small part of them. It's suggested using professional editing or more checks with English.

RE: Thanks for the careful reading and good suggestion! We revised as suggested.

1. P5425 L16: delete 'despite ~ in certain cases'

RE: Revised as suggestion.

2. P5425 L26: rewrite 'to the role ~ budget'

RE: Yes, we rewrote the sentence.

3. P5426 L1: owing to this -> therefore

RE: Yes, revised as suggestion.

4. P5426 L7: check out the arrangement of multi references: I think should be listed with a chronological order.

RE: Yes, we change the reference list style.

5. P5426 L20: delete 'previously'

RE: Yes, revised as suggestion.

6. P5427 L12: delete '.'

RE: Yes, revised as suggestion.

7. P5427 L15: emissions -> emission
RE: Revised.

8. P5428 L21: phenomena -> functions
RE: Yes, revised as suggestion.

9. P5428 L23: C cycling -> C exchange (or processes)
RE: Yes, revised as suggestion.

10. P5428 L29: throughout -> over
RE: Yes, revised as suggestion.

11. P5429 L12: delete 'being a DGVM'
RE: Yes, revised as suggestion.

12. P5429 L1: rewrite this sentence
RE: Yes, we rewrote this sentence according to specific comment of 2.6

13. P5429 L25: applied in -> applied to
RE: Revised.

14. P5430 L21: or -> and
RE: Revised.

15. P5431 L9: all acronyms are confusing. Suggested as a format of Pro_{CH_4}
RE: Yes, we revised this according suggestion.

16. P5431 L14: in ~ as wetlands -> under anaerobic conditions
RE: Yes, revised as suggestion.

17. P5432 L3: unit of R_H? what is each time step?
RE: Yes, we added the unit in the revised text, as well as other variables in the paper. The words "each time step" were deleted here.

18. P5433 L1: high degrees of -> large
RE: Yes, revised as suggestion.

19. P5433 L21: delete 'will'
RE: Yes, revised as suggestion.

20. P5435 L15: were varied -> varied
RE: Yes, revised as suggestion.

21. P5435 L21: examples -> example
RE: Yes, this part was rewrote.

22. P5436 L15: exceeds -> exceed
RE: Revised.

23. P5436 L18: umol -> μmol
RE: Revised.

24. P5438 L24: according -> according to
RE: Revised.

25. P5441 L16: delete this sentence
RE: Yes, revised as suggestion.

26. P5443 L1: CH_4 flux from a study -> a CH_4 flux study by
RE: Yes, revised as suggestion.

27. P5445 L19: rang -> range
RE: Revised.

28. P5448 L3: methane -> CH_4
RE: Revised.

29. P5451 L3: methane -> CH_4
RE: Revised.

30. P5468: delete Fig. 2
RE: Figure 2 was deleted.

31. P5469–5473: add the label of x-axis
RE: Yes, the figures are reorganized and labels of x-axis are added.

References:

Boelter, D., and Verry, E.: Peatland and Water in the Northern Lake States, 1977.

Cao, M., Marshall, S., and Gregson, K.: Global carbon exchange and methane emissions from natural wetlands: Application of a process-based model, Journal of Geophysical Research D: Atmospheres, 101, 14399-14414, 1996.

Frolking, S., and Crill, P.: Climate Controls on Temporal Variability of Methane Flux from a Poor Fen in Southeastern New-Hampshire - Measurement and Modeling, Global Biogeochem Cy, 8, 385-397, Doi 10.1029/94gb01839, 1994.

Granberg, G., Grip, H., Ottosson Löfvenius, M., Sundh, I., Svensson, B. H., and Nilsson, M.: A simple model for simulation of water content, soil frost, and soil temperatures in boreal mixed mires, Water Resour Res, 35, 3771-3782, 10.1029/1999wr900216, 1999.

Myhre, G., Shindell, D., Bréon, F.-M., Collins, W., Fuglestvedt, J., Huang, J., Koch, D., J.-F. Lamarque, D. Lee, B. M., Nakajima, T., Robock, A., Stephens, G., Takemura, T., and Zhang, H.: Anthropogenic and Natural Radiative Forcing, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 731-738, 2013.

Valentine, D. W., Holland, E. A., and Schinel, D. S.: Ecosystem and physical controls over methane production in northern wetlands, Journal of Geophysical Research, 99, 1563-1571, 1994.

Zhuang, Q., Melillo, J. M., Kicklighter, D. W., Prinn, R. G., McGuire, A. D., Steudler, P. A., Felzer, B. S., and Hu, S.: Methane fluxes between terrestrial ecosystems and the atmosphere at northern high latitudes during the past century: A retrospective analysis with a process-based biogeochemistry model, Global Biogeochem Cy, 18, 10.1029/2004gb002239, 2004.