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 CH4 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 CH4, 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.

**- Specific comments**

      1. Major comments

      1.1. Current introduction failed to state the points of why the previous modelling approaches cannot fully support CH4 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 CH4 model in place.

      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.

      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 CH4 are very sensitive to climate conditions, so global climate cannot represent CH4 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.

      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.

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.

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.

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

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.

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 CH4 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.

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'

2.2. P5425 L20-24: check CH4 is 25 times stronger global warming potential referred to by IPCC report (e.g. Forster et al. 2007 from IPCC 2007 report) then rewrite

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

2.4. P5427 L23 – P5428 L17: seems that authors failed to state why a new model is needed and gaps of CH4 estimation should be filled with the new development. Add this clearly and make concise this part

2.5. P5428 L17: start with a new paragraph

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

2.7.  P5430 L3: change as 'of natural wetlands in addition to peatlands'

2.8.  P5430 L17: why 30 cm of max water table depth?

2.9.  P5431 L22–27: repeat of description. Delete

2.10.  P5432 L12: define freezing point and extremely high temp limit

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.

2.12.  P5433 L21: in this paragraph, how water table depth control the CH4 simulation should be described

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.

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.

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'

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

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

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

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

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

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

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

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

2.24.  P5448 L25: try connection to the oxygen profile in addition to temp and include the discussion about the effects of oxygen on CH4 emission

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

2.26.  P5449 L20–24: unnecessary part. Delete

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 CH4 (remove uncertainty)

**- 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.

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

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

3.  P5426 L1: owing to this -> therefore

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

5.  P5426 L20: delete 'previously'

6.  P5427 L12: delete '.'

7.  P5427 L15: emissions -> emission

8. P5428 L21: phenomena -> functions
9. P5428 L23: C cycling -> C exchange (or processes)
10. P5428 L29: throughout -> over
11. P5429 L12: delete 'being a DGVM'
12. P5429 L1: rewrite this sentence
13. P5429 L25: applied in -> applied to
14. P5430 L21: or -> and
15. P5431 L9: all acronyms are confusing. Suggested as a format of $Pro_{CH4}$
16. P5431 L14: in ~ as wetlands -> under anaerobic conditions
17. P5432 L3: unit of $R_H$? what is each time step?
18. P5433 L1: high degrees of -> large
19. P5433 L21: delete 'will'
20. P5435 L15: were varied -> varied
21. P5435 L21: examples -> example
22. P5436 L15: exceeds -> exceed
23. P5436 L18: umol -> µmol
24. P5438 L24: according -> according to
25. P5441 L16: delete this sentence
26. P5443 L1: CH4 flux from a study -> a CH4 flux study by
27. P5445 L19: rang -> range
28. P5448 L3: methane -> CH4
29. P5451 L3: methane -> CH4
30. P5468:  delete Fig. 2
31. P5469–5473:  add the label of x-axis