Biogeosciences Discuss., 7, C3649–C3654, 2010
www.biogeosciences-discuss.net/7/C3649/2010/

Biogeosciences Discussions

Interactive comment on "Quantifying wetland methane emissions with process-based models of different complexities" *by* J. Tang et al.

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Received and published: 7 November 2010

General authors' comments: We sincerely thank the anonymous referee for the insightful comments to help us improve the manuscript.

Our specific responses to the comments are documented below.

General Comments – an assessment of the overall quality of the discussion paper TEM CH4-submodel configurations of increasing complexity are described and compared with experimental CH4 observations at two sites. A complex, four-substance model is developed, as well as a novel ebullition algorithm that allows ebullition events in response to atmospheric pressure variations to be captured in a physically realistic way. This is work that will be of great interest to modelers and experimentalists alike.

The authors addressed most (though not all – see below) of the initial concerns about the manuscript that I raised in the initial Quick Access Review. The paper is now much easier to read, and the figures are easier to interpret.

I recommend that this interesting paper be published subject to the authors replying satisfactorily to the issues I raise below, as well as other issues or questions that may have arisen in the interactive online discussion.

Specific Comments – a discussion of individual scientific questions/issues My main concern with that paper concerns the comparison between models, S1, S2 and S4. I would like to see an explicit defense of the method used, and the conclusions that can legitimately be drawn, in particular in regard to the following two issues: First, as I noted previously, I think it's hard to compare the performance of the models when they have different (calibrated) parameter values – see Tables 1 and 3. Having different parameters is unavoidable when comparing different models, but for a site specific, model comparison study such as this one it would have helped to see the differences between models if they had the same, shared parameter values, at least for each individual site.

Furthermore, the same model can also have different parameter values depending on whether a linear or exponential root distribution is assumed (Table 3). This makes it hard to tell whether the results discussed in Section 3.4 depend on the root distribution or on the parameter values chosen. Please defend, explain or justify this approach in the text.

Our reply: We agree that it is hard to compare the performance of the models when they have different (calibrated) parameter values. What we did in the paper was to consider the parameters as part of the model structure and to allow different models perform as well as possible through calibration. Therefore, the final comparison would show the benefit of using different models. When the model formulation is changed, such as the adoption of a different root distribution function, the parameters should be

changed accordingly in order to get a good fit compared to the observations. With this rationale, the discussion in Section 3.4 for various models is centered on effects of different root distribution functions on methane dynamics.

Second, though I really like the ebullition algorithm the authors have developed, I do not think it is possible to draw firm conclusions regarding the improvements it brings using the results of the modeling experiments presented in the paper. This is a shame, because it's novel and interesting. What it needs, in my opinion, is a direct comparison between S4 and a simpler four-substance S4-like model that uses the same, fixed CH4 and O2 concentration thresholds as S2 to calculate ebullition fluxes. The benefits of the novel algorithm would then be easier to distinguish and defend.

Our reply: Follow the reviewer's suggestion, we showed this comparison between models S2 and S4. By adopting the conventional threshold function-based approach for ebullition modeling, the S4 model reduced to S2 model exactly since N2 is not involved in any chemical reactions in the current model configuration. To obtain a good performance of the S2 model, different parameters should be used rather than simply using same parameters obtained from the S4 model. This indicates when the ebullition process is not properly described in the model, the CH4 efflux can be modeled apparently well but with improper processes formulation and parameterizations. Also, we showed, in the sensitivity of the different models to water table dynamics, S1 and S2 models failed to obtain the expected burst of bubbling due to a fall of standing water, when there is CH4 trapped in the soil column. In contrast, the S4 model simulated such a significant burst.

Finally, could the authors consider or discuss how the S4 model could be applied to "quantify global wetland CH4 emissions," as stated in the Abstract? Parameters are site-specific, so how can this be addressed? NPP, vegetation type, pressure, water table and soil temperature inputs are also required.

Our reply: Since the paper is already quite long, we will document the detailed regional

study in a follow up study. But we added a new section - Section 3.5 to discuss some issues we identified in our study. It is quoted as below.

"**3.5 Issues for regional application of the different CH_4 models**

In applying the models of different complexities to regional quantification of wetland methane emissions, we found that the way to upscale the parameters of maximum CH_4 production potential ($\hat{P}_{CH_4}$) and maximum CH_4 oxidation potential ($\hat{Q}_{CH_4}$) from the calibrated sites to a region is critical. Currently, we use the maximum monthly NPP derived from a 50-year historical TEM simulation to scale the parameter $\hat{P}_{CH_4}$ and the maximum monthly soil respiration to scale the parameter $\hat{Q}_{CH_4}$. Both NPP and soil respiration are simulated with TEM. The extrapolation is based on the fact that methane productivity is usually positively related with NPP (e.g. Chanton et al., 1995), and methane oxidation is positively related with respiration (e.g. Nakano et al., 2004). The scaling is done based on the vegetation cover data. The remaining model parameters at the calibrated site are used for our regional extrapolations. Thus, as a next step, we will test how different ways in extrapolating the site-level parameters to a region affect the uncertainties in the wetland methane emission quantification with the methane models of different complexities.

Another finding is that the regional water table dynamics are another major source to the uncertainty in quantifying regional wetland methane emissions. Standing water depth on top of the soils is also essential to a proper quantification of regional CH_4 effluxes. In particular, when the S4 model is used in regional simulations, there are grid cells, where vegetation is sparse, emitting CH_4 mainly via the pathway of ebullition. In contrast, the S1 and S2 models greatly underestimate the CH_4 emissions (Fig. 10) in such case. In these simulations, water table depths play a significant role in affecting CH_4 production, oxidation, soil pressure profile, and diffusion process. To more accurately simulate water table dynamics, we are currently doing vigorous testing of different algorithms (e.g. Granberg et al., 1999; Weiss et al., 2006). The methane models with different complexities will be further coupled with existing soil physics models (e.g.,

Zhuang et al., 2001, 2003; Tang and Zhuang, 2010) and the tested water table depth model to conduct regional and global analyses of wetland emissions. ”

3. Technical Corrections

Units: The table on page 6146 with symbols and definitions should have an additional column for units to make it easier to read. However, a more serious point is the fact that there seem to be differences between the units given in the main text and those given in this table. For example, on page 6125, the units for yi,w and yi,a do not match those in the table. See also page 6127, when the Michaelis-Menten constants are discussed.

Our reply: We modified the table, and corrected inconsistent places.

Page, Line – Comment 6161-6162 - Figures 6a2, 6b2 6c2 6d3: Where is the “Total” output here?

Our reply: Because ebullitions in these simulations were too small to be identified from the figure, so the total flux is almost equal to plant transport plus diffusion, and thus hard to be seen.

6123, 2 - Contributes to . . .

6123, 7 - Inverse modelling reference needed, e.g. Bousquet et al.

6123, 14 - Process-based models, not “The proc. . .”

6123, 16 - In these models, . . .

6125, 1 - Is one reference sufficient here?

6126, 5 - Explain volatiles in this context

6129, lines 10-20 - A simple Figure illustrating the bubbling criterion would be really nice in the paper. Something showing a soil profile, the water table, bubbles etc.

6129, 15 - Explain the theta terms in eqn (15) here too

6130, 25 - I think that the choice of “Z” as the soil depth is confusing, and easy to confuse with the variable “z”. How about “Zsoil” or something similar?

6133, 14 - Explain “trial and error”.

6135, title - “nobservations” !

6136, 24 - Remove “sort of”

6137, 8-11 - Could show in an Appendix, perhaps, as this is interesting

6138, 12 - Reference needed!

6145, 16 - “in” that we here . . .

6146 - Soil “moisture”

6156 - In “the” S4 model . . .

6160 - “The” same time series . . .

6165 - Could put arrows on the Figure (between the applied WT change and the main emissions) to show clearly the delayed emission, perhaps.

Section 2.5: Explain the choice of the days for both water table and pressure increases and decreases.

Our reply: We addressed all the above minor comments in this revision.

Interactive comment on Biogeosciences Discuss., 7, 6121, 2010.