

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

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General author's 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: Several models of methane emission from wetlands have been constructed in the last 15 years. Most of these models are used to simulate methane fluxes on a single site, reproducing fluxes which have been measured by series of flux chamber measurements. The models vary in complexity; generally the more complex models require more parameters and input data. Although these models generally simulate the seasonal variability of fluxes reasonably well, small-scale temporal variability

C3643

remains a problem. In particular daily variations are often poorly reproduced. Ebullition is one of the processes behind small-scale temporal variation. The paper by Tang et al. addresses this problem, by introducing an ebullition model that takes account of air pressure and does not rely on a single concentration threshold for the generation of bubbles. Furthermore it includes all relevant gases, simulating also the methane content of the bubbles rather than assuming a fixed value. The paper also shows the effects of using models of different complexity, in particular with respect to the gases and reactions included in the model. The improvements of model fit resulting from the higher complexity models are clear but not spectacular. Despite these results, this paper is worthwhile publishing, albeit with minor modifications. Overall, the paper is well written, the model equations are explained thoroughly, and the figures are clear and informative. The sensitivity tests of the model are quite useful and illustrate very well the effects of changing the model structure. However, the authors should have added a somewhat more critical evaluation of their results. I would have expected a discussion of how large the benefits are from the more complicated models - and their disadvantages in terms of higher parameter and data requirements. Such a discussion would make the article rise above the level of 'just-another-methane-model'. See the remarks below.

Specific comments 1. The discussion and conclusions remain rather technical and comment only on the sensitivity tests of the model. I would appreciate reflection of the authors on the practical use of their model improvements. It is necessary to know the benefits of increasing model complexity in relation to the increasing parameter and input data needs. I suggest the authors to pay attention to the following questions: - Discuss the intended use of the model. This will generally not be just the reproduction of local field data, but temporal or spatial extrapolation. For instance gap filling, spatial upscaling of fluxes across a larger area. - To what extent is the model fit improved by using the higher complexity versions? To what extent are the improvements significant or are they marginal only, and maybe not exceeding the uncertainty in the data? How large are the cumulative differences between the model results with respect to the total

C3644

emission in the three simulated years? Consider statistical testing of the differences in model fit for the lower and higher complexity models. - What are the 'costs' in terms of parameter and data requirements of the models relative to the benefits of increased model fit? For instance the better fit of S4 requires the availability of air pressure data; also list other extra parameter and data requirements.

Response to the comment 1: To address these comments, first, we added a new section - Section 3.5 to discuss the issues that will be encountered in applying the model for regional extrapolation. We believe the current manuscript is rather long already and plan to present the regional and global analysis in a follow-up study using the developed models with different complexities. Second, on the practical use of the new models with respect to number of parameters and input data requirement: Although the models are of different complexities, they have almost the same input data requirements and the same number of parameters. The more complicated S2 and S4 models have fewer parameters than does the S1 model. For instance, the fraction of CH₄ oxidized during plant transport is computed explicitly in S2 and S4 models rather than parameterized as in S1 model. The extra input data for S4 model are surface pressure, which is readily available (e.g. datasets from NCEP Reanalysis 1 and ECMWF Interim Reanalysis). Third, based on the site level study, we found the uncertainty due to using different model structures is as much as 5% in the 3-year model and data comparison at our study sites. We also found the modeling of water table dynamics is more critical for the S4 model, and its ebullition algorithm is intimately related with the status of standing water and soil moisture in comparison with other models. In the sensitivity test to transient water table change, we found, when the plant was removed, that the S4 model predicted orders greater CH₄ emissions through ebullition than S1 and S2 models did. Therefore, using S1 and S2 models to quantify the regional wetland CH₄ emissions, the emitted fluxes could be greatly underestimated in places where ebullition is the dominant pathway of CH₄ efflux because the S1 and S2 models did not capture the responses to water table dynamics. To conclude how regional emissions respond to water table dynamics, we are currently testing different algorithms of

C3645

modeling water table depth.

The Section 3.5 that conveys some of these discussions in the revised manuscript, particularly on regional extrapolation, is quoted below:

"3.5 Issues for regional application of the different CH₄ 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 CH₄ production potential (\hat{P}_{CH_4}) and maximum CH₄ 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 CH₄ effluxes. In particular, when the S4 model is used in regional simulations, there are grid cells, where vegetation is sparse, emitting CH₄ mainly via the pathway of ebullition. In contrast, the S1 and S2 models greatly underestimate the CH₄ emissions (Fig. 10) in such case. In these simulations, water table depths play a significant role in affecting CH₄ production, oxidation, soil pressure profile, and diffusion process. To more accurately simulate water table dynamics, we are currently doing vigorous testing of dif-

C3646

ferent 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. ”

2. *The approach to modeling of ebullition is not entirely novel; also Granberg et al. (Global Biogeochemical Cycles, 15, p 977-991, 2001) use pressure-dependent ebullition in their model.*

Response to the comment 2: We explained the difference between our and their approaches, and cited their work in this study.

3. *Page 6131 line 14-15: 'It is likely that our algorithm will not always give superior results to that obtained using the volume threshold based method in other studies' . This is a highly relevant remark. To my knowledge model inter-comparison has never been done for methane emission models, and will be very useful.*

Our reply to the comment 3: To conduct comparisons among a number of models is beyond this work. We will follow the suggestion to conduct a comparison study with modelers who are developing those existing models as a next step.

4. *P 6137 line 10. Water table sensitivity tests: this is a nice result of the sensitivity tests, why is it not shown?*

Our reply to the comment 4: We showed the results in this revised version. We found, when no plant is present to support gas transport, that only the S4 model respond significantly to the transient dynamics of water table (Fig. 10).

Detailed remarks:

P 6318, line 12: 'When the cumulative differences were analyzed, we found for a three year period at the Buck Hollow site, the S4 predicted around 2000 mgCH₄ m⁻² y⁻¹ using the transient atmospheric pressure data than using the standard 1 atm pressure. ' This sentence is confusing - predicts S4 more or less CH₄ with the transient

C3647

pressures? Please try to quantify the differences also relative to the total flux.

Our reply: We reorganized this sentence and redrew the figure to make the statement clearer.

Page 6144, line 14: in the Walter-Heimann model the percentage of oxidation during plant transport can be varied; in their model they set this factor to 50% but they stress the high variability of observed values. So selecting a value of 50% is rather arbitrary, and might be obtained from calibration.

Out reply: We agreed. We chosen 50% as a median estimate based on previous studies. It is possible to make this number as an extra parameter. To keep a minimum number of parameters, we here still treat this as a constant.

Technical comments

Page 6135, heading 3.1: 'nobervations' – observations

Page 6140, line 20-24 Have mercy with your readers. Consider splitting up or reordering this very long sentence.

Page 6145, line 9: "And Rveg is a scaling parameter needs calibration to account for differences in conducting capabilities for different plants", better: Rveg is a scaling parameter which needs calibration to account for differences in conducting capabilities for different plants.'

Our reply: In this revision, we corrected typos and grammar errors, and reorganized sentences which are not clear.

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