Review of "Soil Methanotrophy Model (MeMo v1.0): a process-based model to quantify global uptake of atmospheric methane by soil", Report №2.

Major comments

1. Page 4, line 6. First of all, please, fix the reference, it should be ZhUang, not Zhang. Second, I think, sentence "The model of Zhang et al. (2013) (hereafter referred to as model 'Z13') employs the same steady state analytical solution as model C07" is incorrect. Z13 uses steady state reaction-diffusion equation for methane (as almost all discussed models), but this equation was solved *numerically* for entire soil depth from 0 m to 1 m (theoretically it is not possible to solve this equation analytically in this case). Thus Z13 takes into account vertical heterogeneity of methane consumption controls, which is not the case for models of Potter family and your model also. It is principle advantage of Z13 and other recent models in comparison with your model. It definitely should be mentioned in a paper text.

I also do not understand why you write in the same paragraph "However, such a stand-alone application (i.e., decoupled from TEM) would require a new implementation or presumably significant modifications to the code." It is not a disadvantage of the Z13 model, it is a technical issue.

Zhuang, Q., Chen, M., Xu, K., Tang, J., Saikawa, E., Lu, Y., ... & McGuire, A. D. (2013). Response of global soil consumption of atmospheric methane to changes in atmospheric climate and nitrogen deposition. Global Biogeochemical Cycles, 27(3), 650-663.

2. This is not good that you do not check presentation of your model against MEASURED methane fluxes. I see that you use for field data for parametrization how temperature, moisture and nitrogen influence on methane consumption. And you illustrate it with nice figures. But you do not show model presentation against independent flux data set. It is important because influence of methane consumption controls is often not independent from each other and multicollinearity does exist in this case. So it can be dangerous to use model to the global flux calculations and predictions without a validation using independent methane flux data.

It is not always the problem. For example, for Z13 authors also did not do this validation. But they use numerous flux data sets to obtain values of different parameters in optimization procedure. In this way Z13 takes into account interaction between controls, although this algorithm is very often mathematically incorrect (it is so called ill-posed inverse problem).

We can see MeMo presentation for different latitudes against combined methane flux data in comparison with other models of Potter family (Fig. 5), and MeMo seems to give substantial improvement. But in data set from (Dutaur and Verchot, 2007) almost all flux measurements are not seasonal average but measurements made in several days or weeks during season. You also do not use data obtained in sites where fluxes from (Dutaur and Verchot, 2007) data set were measured. In summary, I think that there is a lack of validation in your model because in Fig. 5:

- you compare modeled seasonal fluxes and sporadically measured fluxes
- you did not validate your model for set of sites with their own ecological parameters; instead you compare fluxes measured in multiple sites of 10° regions with modeled latitude average seasonal flux.

That is why instead of pointwise convergence necessary for predictions you showed only convergence in general, when model accuracy for certain geographical points and sites is hidden.

I suggest to mention that you did not validate your model directly and to explain why you did not do it. At least for sites used for validation in Z13 it should be possible.

Minor comments

1. I recommend to remove Figures 1 and 2. They do not give any deep insights in the subject. I think ideas presented in these Figures are obvious without visualization for almost all readers of GMD. You can write several sentences instead.

2. If you use the same model for soil gas diffusivity as previous models I recommend to shorten section about it (2.3.1).

Summary

In my opinion, the manuscript was significantly improved after first round of corrections. Introduction now give much better representation of the state of the art in methane consumption modeling and the scope of current paper. Mistakes in math were fixed, some results of the model became much more realistic (for example – estimates of L). Authors made a big efforts to upgrade paper text, I really appreciate this.

But I think, some aspects still need to be fixed. Moreover, I think that there is a lack of model validation.