Review of "Soil Methanotrophy Model (MeMo v1.0): a process-based model to quantify global uptake of atmospheric methane by soil"

Summary

The manuscript describes a new model of methane consumption by upland soils. The topic of the modelling is hot and deserved a lot of attention in literature over recent years. Paper consists of detailed model description, data sources description, global and regional methane consumption estimates, their seasonal variability and discussion of the listed results.

Major comments

1. My first big concern is about correspondence between your solution of eq. (2) (using eqs. (3) and (6-7)) and boundary conditions. I will use the same symbols and introduce following substitution

$$\alpha = \sqrt{\frac{k_d}{D_{CH_4}}}$$

Upper boundary condition for your model is $CH_4(0) = C_{CH_4}$, Dirichlet type, fixed atmospheric level concentration. If we substitute eqs. (6) and (7) into eq. (3) and write it for the upper bound ($z = 0$) we get following equation:

$$CH_4(0) = \frac{C_{CH_4} \cdot \exp(\alpha \cdot L) \cdot \exp(-\alpha \cdot 0)}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))} + \frac{C_{CH_4} \cdot \exp(-\alpha \cdot L) \cdot \exp(\alpha \cdot 0)}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))} \qquad (r1)$$

Let's rewrite (r1) in the following form

$$CH_4(0) = C_{CH_4} \cdot \frac{\exp(\alpha \cdot L) + \exp(-\alpha \cdot L)}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))} \qquad (r2)$$

To correspond your upper boundary condition fraction term $\frac{\exp(\alpha \cdot L) + \exp(-\alpha \cdot L)}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))}$ from eq. (r2) should be equal to 1 (numerator and denominator should be the same and non-zero), but as we see, it is not.

Lower boundary condition for your model is $CH_4(L) = 0$, also Dirichlet type. By the way in several places you speak not about complete consumption of methane but 99.9% of C_{CH_4} consumed. It creates ambiguity and should be fixed. If we substitute eqs. (6) and (7) into eq. (3) and write it for the lower bound ($z = L$) we get following equation

$$CH_4(L) = \frac{C_{CH_4} \cdot \exp(\alpha \cdot L) \cdot \exp(-\alpha \cdot L)}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))} + \frac{C_{CH_4} \cdot \exp(-\alpha \cdot L) \cdot \exp(\alpha \cdot L)}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))} \qquad (r3)$$

Let's rewrite (r3) in the following form

$$CH_4(L) = C_{CH_4} \cdot \frac{2}{(-\exp(-\alpha \cdot L) + \exp(\alpha \cdot L))} \qquad (r4)$$

It is obvious from this equation that $CH_4(L) \neq 0$. For example given in your paper parameter values $k_d = 5 \cdot 10^{-5}\, s^{-1}$; $D_{CH_4} = 0.196\, cm^2 \cdot s^{-1}$; $L = 687\, cm$ eq. (r4) leads to $CH_4(L) = 5.79 \cdot C_{CH_4}$. Not $0.001 \cdot C_{CH_4}$ and not zero as should be according to the paper.

So suggested general solution does not satisfy both of used boundary conditions.

2. My second big concern is rationality of building this model in its current state. I suppose that each new model should provide substantial improvement of available models. But in your paper only one class of available models is described and improved (models of Curry, Ridgwell, Potter; further CRP models). To my knowledge there are much better models of methane consumption (as example, Saggar et al., 2007; Zhuang et al., 2013). The main their advantage is description of methane consumption and soil methane diffusion not as constant along the soil profile (like in your model and CRP models) but as dependent on soil depth. These models also take into account all environmental controls considered in your paper. So it is not correct to ignore them.

That's why it is necessary:

- to tell that these models exist and to give their brief description
- to explain why it is imporntant to build a new model and why your model is better than others.

This explanation is necessary to give comparing your model with CRP models too. You consider the same factors as CRP models, so what are the reasons to improve these models? Are CRV models or models from (Saggar et al., 2007; Zhuang et al., 2013) predict measured methane fluxes worse than your model or not good enough?

3. Using L, the depth of total methane consumption, is good idea, but total methane consumption (or consumption up to 0.1% of atmospheric methane level) does not occur in natural upland soils. There is a certain threshold of methane consumption by microorganisms. Methane consumption stops if this threshold is reached because microorganisms cannot get enough energy by methane oxidation for cell maintenance (Stackhouse et al, 2017). According to literature methane concentration is never close smaller than 0.1 ppm in deep soil horizons and consumption declines to zero in deep soil layers – about 50-70 cm (Bender and Conrad (1992), Whalen et al (1992), Czepiel et al (1995), Priemé and Christensen (1997), Jensen and Olsen (1998)). To my knowledge biological consumption of methane was not ever investigated on a depth more than 1 m in upland soils. Threshold of consumption varies depend on ecosystem type, climate and is defined by oxidation efficiency of methanotrophs (see references above and Stackhouse et al, 2017).

That's why I think that this approach of using total methane consumption depth is not correct.

I think, paper can be published only if all these three problems will be solved.

Minor comments

1. Page 5, rows 15-20. It would be better to give here any estimates, why only diffusive transport and biological consumption should be considered. What about convective transport? Is it always can be omitted? Why term on a right side of eq. (2) is not important? Are conditions really always steady state?

2. Page 10. What is the reason of using old Moldrup paper (same as in Curry paper) while there is much more recent and better soil gas diffusion model in (Moldrup et al., 2013)?

3. Page 30, row 11. Please fix, Sabrekov (like in list of references), not Savrekov.

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