Referee # 1

We thank the referee for the comprehensive and detailed comments, which have helped to improve the manuscript. Please find our responses to the referee's comments (indicated in bold) below.

1. **Page 4, line 6. First of all, please, fix the reference, it should be ZhUang, not Zhang.**

   Thank you for highlighting this mistake. The text has been changed accordingly.

2. **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.**

   Thank you for the observation. We agree that the main difference between Z13 and the Potter family of models (including MeMo) is how the reaction-transport equations are solved. MeMo solves the equation analytically while Zhuang solves it numerically using multiple soil layers. A numerical approach allows for resolving the vertical heterogeneity of, for instance, diffusion coefficients, soil moisture and, thus, yields better results for local simulations and/or when soil properties are well-characterized or when comprehensive ecosystem model output (e.g. TEM) is available. Yet, MeMo has been designed with the aim to investigate past and future dynamics of the global methane soil sink over large timescales (e.g. 10^2-10^5 years). These applications require a computational efficient solution, as well as the ability to constrain model parameters based on (paleo)climate model outputs. We therefore argue that the numerical approach does not necessarily represent an improvement over MeMo for such computationally expensive and data limited applications.

   We now include a sentence explaining this substantial difference between the models:

   P4L6 "The model of Zhuang et al. (2013) (hereafter referred to as model 'Z13') employs the same steady state reaction-diffusion equation for CH_4 as previous models; however, Z13 solves the steady state reaction-diffusion equation for CH_4 numerically using multiple soil layers"

3. **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.**

   While it is a technical issue, it is one that makes it prohibitive to easily implement the Z13 scheme. To decouple the Z13 model from TEM would require a significant modification of the code and possibly the model because there is no realistic way to include multiple soil layers without TEM. We feel our text correctly captures the technical challenge of adapting Z13 to other applications.

4. **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.**

We disagree with this comment and are puzzled as the reviewer mentions the comparison against flux data in the comment below. No previous global model of soil uptake has ever been validated against a global database of observations and we, for the first time, conduct a regional-scale validation (Figure 5 and Section 4.2 describe this regional validation against external observations).

A point-by-point comparison to data from Dutuar and Verchot (2007) is not appropriate for a global model. To do so would require comparison of site specific data with the closest location in a coarsely resolved model grid from MeMo. The comparison would not be meaningful because global models such as MeMo are designed to represent regional scale dynamics and not fine scale conditions. Consequently, we have validated our global model through comparison to regional ecosystem scale data, which is standard practice. Notably, our study is the first to date to perform such a global validation of a soil methanotrophy model. We show in Figure 5 how MeMo performs in a regional-scale validation compared to previous models, demonstrating that soil uptake of CH_4 in the tropics is greatly over-estimated in other models.

5.- **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.

The comparison between MeMo and field observations from Dutaur and Verchot (2007) was performed because the latter are the best data available on which to base a comparison. Dutaur and Verchot (2007) use local estimates to upscale to annual flux at each location and to total global uptake.

We acknowledge the limitations of performing a validation of a coarsely resolved global model against site observational data (as described in the previous comment); however, our approach represents the first step towards understanding model limitations. This simple validation demonstrates that previous models greatly overestimate soil uptake of atmospheric methane across the tropics because their parameterizations of soil moisture and k0 were not correct.

We now state in the text on Page 22 Line 14, "The latitudinal distribution of soil uptake rates of atmospheric CH_4 predicted using the R99 and C07 models, and MeMo are shown in Fig. 5 accompanied by direct measurements of CH_4 oxidation rates from Dutaur and Verchot (2007) and a 10° running average. We chose to validate MeMo and previous models against regionally averaged observations to conduct the comparison at scales resolved by global models such as MeMo. This model is not intended to represent fine-scale site-specific attributes of soil but rather broad regional soil characteristics and CH_4 uptake fluxes."

5. **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.**

Figures 1 and 2 are important to include to show the setup of the model. Figure 1 illustrates the computational solution of L, which is a fundamental difference between MeMo and previous models. Figure 2 is important because it shows the sensitivity of CH_4 uptake to k0 and thus the importance of the parameter. This detail is fundamental because k0 differs greatly from previous models.

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

Thank you for this suggestion; however, for clarity we prefer to include all equations in the text with appropriate citations rather than redirecting readers to other publications for that information.

Referee # 2

We thank Referee #2 for the positive assessment of this work and for providing comprehensive reviews. Please find our response to the referee's comment (indicated in bold) below.

Page 34 Line 32: for CRU data, now we can use TS 3.25 or TS 4.0.1 from web site. (This is not mandatory but optional).

Thank you for drawing our attention to the availability of these new data sets. For future applications, we will use updated forcing data, as they become available.

Referee # 3

We thank the referee for the detailed comments. Referee comments are shown in bold, followed by our response.

P1 L16 potent greenhouse gas

The text has been changed to 'potent greenhouse gas.'

P1 L22 at the global scale

The text has been changed to 'at the global scale.'

P1 L26 "We show that the improved representation of these key drivers of soil methanotrophy results in a better fit to observational data." Actually, it's hard to tell is the better model-data fit coming from process representation, driver representation, or just parameterization. But it's totally fine to conclude that the model improved structurally and parametrically.

Thank you for this observation. We have changed the text to indicate that the improved model-fit comes from both the structure and parameterization of MeMo.

P1 L26 "We show that the improved structural and parametric representation of key drivers of soil methonotrophy in MeMo results in a better fit to observational data."

P2 L5 preindustrial era

The text has been changed to 'since the pre-industrial era' on Page 2 Line 16

P3 L11 interannual variability and uncertainty

The text has been changed to 'large inter-annual variability and uncertainty.'

P5 table 1. Values for some critical constants are missing (e.g., kd, A, B)

Thank you for this observation; however, as far as we can tell all base variables and constants are included in the table (along with their values). The ones mentioned in the reviewer's comment are constructed from other variables and constants, and therefore were not included in the table. For example, kd is a variable that depends on soil moisture, soil temperature, k0 and nitrogen as explained in section 2.3.2. A and B are integration constants, whose values depend on L, kd, and D_{CH4} as defined by Eqs. 6 and 7. Integration constants A and B have now been removed from Table 1 to be consistent with our statement that only base variables and constants are presented in the table.

P13 Table 3. What's the uncertainty of MeMo k0 parameters?

This is an interesting point. Both C07 and R99 did not report the uncertainty of this parameter because of a lack of field measurements. In the case of MeMo, we face the same problem. We discuss the importance of k0 and the sensitivity of CH_4 uptake to this parameter in Figure 2 and discuss the scarcity of k0 measurements in the 'Model Limitations' section. We now include a sentence to explaining why k0 uncertainty could not be characterized:

P13 L22 "; however, the uncertainty of this value could not be characterized due to a dearth of available observational data."

P19 Figure 4. It's actually a little bit ambiguous that rN is parameterized with N input rate. With the same N input rate (gNm2y-1), one can fertilize the system with a monthly frequency verses a daily frequency. Then the actual N retained in the soil will be totally different across the year. Thus, the same N input rate could have different inhibition controls on CH4 consumption.

Thank you for this interesting comment. We agree that nitrogen fertilization will have temporal structure that is not resolved by MeMo. For this reason, we used long-term field observations to parameterize rN in addition to data from laboratory experiments. Both approaches yield a similar pattern. We have modified the figure legend to ensure it is clear that field measurements were obtained from long-term data.

P19 Figure 4 caption: "CH_4 uptake response as a function of nitrogen deposition and fertilizer application factor r_N. The linear fit (black line) is based on observations from field (long-term) and laboratory measurements (gray and blue dots; Supplementary 1, Table S3)."

P21 Table 7 global soil CH4 uptake has mean value and uncertainty. It's not clear in the manuscript, where the uncertainty is from? In particular, why the uncertainty is so large in observation, but the uncertainty is so small in MeMo.

The uncertainty in MeMo corresponds to temporal variability over the time period of the study (1990-2009). The large uncertainty in the observations results from: (i) the lack of representation of several ecosystems, and (ii) measurements being conducted during specific seasons rather than a full annual cycle. In the global observational dataset, Dutaur and Verchot (2007) standardized the data to annual values and across global ecosystems, thus providing global estimates. Nonetheless, these limitations result in a large uncertainty and we have added a paragraph explaining how the uncertainty was calculated in MeMo and in the observations.

P21L6 "MeMo predicts an average annual global flux of 33.5 ± 0.6 Tg CH_4 y^{-1} for the period 1990 to 2009. Uncertainty in this flux was calculated as the standard deviation of annual global CH_4 uptake."

P21L12 "Upscaling of field measurements of soil methanotrophy rates from 120 different studies spanning a wide range of ecosystems yielded an uptake flux of 36 ± 23 Tg CH_4 y^{-1} (Dutaur and Verchot, 2007). The large uncertainty associated with the mean flux results from differences in data representation for ecosystems and a tendency for sampling to be conducted seasonally rather than annually."