**Biogeosciences
Discussions**

Interactive comment on "Barriers to predicting changes in global terrestrial methane fluxes: analyses using CLM4Me, a methane biogeochemistry model integrated in CESM" *by* W. J. Riley et al.

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We would like to thank Prof. Roulet for the positive and constructive comments on our paper. Since there were no substantial criticisms in the introductory remarks, we respond in detail to specific comments below (the reviewer's comments are numbered and our responses are given immediately below each comment). Response to Reviewer #2 (Nigel Roulet)

1. Pg. Ln 6 This statement is a little presumptuous. Let the reader decide this after

they have read and digested your paper.

Unfortunately, we could not tell exactly which statement the reviewer is referring to.

2. Pg. 11 Where does the value of 0.2 for fCH4 come from? Why is it not 0.3, or even 0.5 as it should be in a perfectly anaerobic system. No one would argue that in wetland there is perfect anaerobic conditions, but the parameter you have used few have measured it. Many back calculate it, but arguably it is one of a few critical parameters in all methane models that simulate production. More details on how you came to this value is warranted.

This value was derived from the values used in LPJ-WhyMe (Wania et al., 2010). We acknowledge in the Methods and Discussion sections that this parameter is very uncertain. For this first version of the model we used a single value globally, but future work should focus on using observations to better constrain this parameter.

3. Pg. 11 Should there be a mass term in equation 2? Does there not need to be some constraints on production that is linked to the ability on appropriate substrate?

We assumed that CH4 production is directly related to heterotrophic respiration, that there are no time delays between fermentation and CH4 production, and that soil organic matter can be treated uniformly with respect to its decomposition under aerobic and anaerobic conditions. We have added these caveats to the Methods section to address the reviewer's concern.

4. Pg. 14 ebullition - there was a commentary on Wania's description of bubble nucleation written by Timo Vesala in Biogeoscience. You have chosen to ignore this. What are to conclude from this - you do not think his criticisms were valid?

No, as far as we can tell, Vesala's criticisms were valid. However, they seemed to relate to Wania et al.'s Discussion paper description of gas versus aqueous phase CH4, which Wania removed in the final version of that paper. The method we chose to estimate ebullition fluxes in CLM4Me was based on the observation that volumetric CH4 gas

contents above 12-15% are consistent with ebullition events (see Methods section). We argue in our paper that the representation of ebullition incorporated in most CH4 biogeochemistry models is crude and would benefit from further research.

5. Pg. 14 I am correct that this assumes that there is no physical constraints to bubbles traveling through the soil matrix?

Yes, that is correct. However, we recognize that physical constraints exist which hinder bubble transport, and that this mechanism can result in bubble degradation and re-equilibration with the aqueous phase. We have added a sentence to this effect in the Methods section.

6. Pg. 16 first paragraph-This role is one of the gas transport alone. There is emerging evidence that roots provide substrates that support methane production. Do you deliberately ignore this, or do you assume it embedded implicitly in this parameterization? If it is important then one would expect methane to be related to GPP for those plants where this is important and not so much related to SOM.

We agree with the reviewer that root exudation is likely an important component of methane substrate production. However, CLM4CN does not explicitly predict root exudation, so we were not able to easily incorporate this mechanism. We address this omission in the Discussion section and intend to remedy this problem in future model versions.

7. Pg. 18 line 8 If you are using a hybrid organic soil matrix by mixing organics into mineral soils how does this influence the porosity used in the calculation of gas diffusion coefficients? What is the maximum porosity that you can ascribe to your soil matrix?

The soil porosity in CLM4 is based on parabolic scaling between mineral and organic soils, as described in Lawrence and Slater (2008), and adjusted for a fibric-to-sapric vertical transition (see Section 2.3). In response to the reviewer's comment we have added text to more explicitly describe this representation in the model.

8. Pg. 18 second paragraph. Has this been confirmed by anyone else? Is it a regular occurrence? I believe the Swedish team Mastepanov works with have seen this in some years and not in others. Until we have the physical model of how and why this occurs is it premature to incorporate this at this stage?

We only performed this analysis as a sensitivity study, since it doesn't have a large effect across the region and the hypothesized mechanism is plausible (i.e., excluding methane from remaining dissolved in the ice fraction of freezing soil pore space). In response to this comment, we clarified in the paper that this analysis is a sensitivity study and that other mechanisms may be relevant.

9. Pg. 20 equation 10 This means that any problems with the inundation approach will be embedded in these three parameters which means they are only transferable if others use the same database - I.e. They are dependent on the inundation database?

Yes, as applied here they are specific to the Prigent et al (2008) satellite-inferred inundation product. Our approach is intended to provide a first estimate of how inundation may change. As we discuss in the paper, however, inundation is a first-order controller on regional CH4 emissions and models need to be able to predict the magnitude and seasonality of inundation accurately. Much more analysis and work needs to be performed to predict how inundation will change in high-latitude systems, as we discuss in the Discussion section.

10. Pg. 24 section 2.7. Why not examine the sensitivity to S?

We showed that the impact on annual emissions was $\sim$5%, which is much smaller than the other factors we analyzed in our sensitivity study. We therefore concluded that a sensitivity analysis about this parameter would not be especially meaningful.

11. Figure 4 This indicates the model does very poorly at the site level. Are the results significantly different from random? None of these a very close at all. The only fit that looks reasonable is the Boreas NSA and the Mississippi, and I guess the

Amazon from Devol though this is a limited comparison. What do these graphs tell us? The model is more often than not an order of magnitude off and only sometimes gets the seasonal pattern right. Can you do some simple statistics on these comparisons to show they can be considered part of the same population? These results beg the question about the appropriateness of the approach for testing the model. What should a grid scale model with very lumped parameters and parameterization be expected to do in a comparison with site specific observations?

In this paper we did not focus on performing well-constrained site comparisons, but rather compared net emissions estimates from the global model against observations. We discuss the problems with this approach in some detail in the Discussion section, as well as the problems associated with models that are tuned to individual site observations. Our approach does, however, test the ability of an un-tuned biogeochemical CH4 model built for global simulations to represent net fluxes at individual sites.

12. Finally, all your examples are for areas that emit methane. There are wetlands that emit little methane - does the model also emit little methane at these sites - e.g. you should estimate effectively emit no methane from site like Mer Bleue.

In Figure 4, we showed comparisons between predictions in the assumed saturated zone and observations (which likely had a variety of saturation conditions). For a system like Mer Bleue, where the water is often below the surface, these assumptions would lead to an over-estimate of the net flux. However, if the model were forced with local hydrological conditions, we might expect better agreement with observations.

13. Figure 5 Why does the greater Q10 give higher emissions in the tropics, as would be expected, but lower emissions from high latitudes - not what I would expect? Since everything else should be equal in the simulations how can a higher Q10 lead to lesser emissions? Is it because a difference between the temperature sensitivity between production and oxidation.

It sounds like the reviewer is assuming that the Q10 is being applied relative to a local

mean annual, or optimal, temperature, whereas the model currently assumes a base temperature globally of 295 K. We discuss in the Discussion section the limitations to this approach, which is common across global CH4 models.

14. Pg. 29 global section – there is something wrong with this sentence - 270 is globally and 160, 50 & 70 are the regional tropics, temperate & north of 45?

As requested by the reviewer, we have clarified the regions each of these values corresponds to.

15. Figure 8 What is the interaction between inundation and the fraction of annual CH4 aerenchyma oxidation?

The results shown in Figure 8 are flux-weighted, so most of the signal is from the inundated fraction. Emissions from the non-inundated fractions are small globally.

16. Pg. 33 section 3.8 Is the decrease in high latitude wetlands reasonable? Over 90% of these wetlands are peatlands. Looking at peatlands in places like Alberta where P is ∼ Et it is unlikely that the parameterization you have used for decrease in inundation applies in the peatlands.

We are not explicitly modeling peatlands in the current version of the model. However, peatlands that lose underlying permafrost may experience some drying, and there may be regions where precipitation increases less rapidly than potential ET. To address the reviewer's concern, we note that we discuss in the Discussion section the uncertainties in simulation of current and future inundation, and mention the unique impacts of peatlands on local water budgets.

17. Pg. 35 first paragraph Does this explain the difference in the Q10 response referred to earlier. I am still at a loss to explain why this is the case. What is interplay of variables and functions here?

See answer to the previous question on Q10 responses.

18. Pg. 35 third paragraph. As asked before, why did you not test the sensitivity to changes in S? See your first section of the discussion. How important is it improve the S parameterization versus some of the other variables and parameters you examined, or another way of asking this is can you demonstrate given your current method for estimating S that it is worth attempting to improve other parts of the model or does the uncertainty is S and how project changes in S the log jam issue. How important is solving the hydrology issue to obtaining useful results?

Please see our answer to the previous question regarding seasonal inundation sensitivity. Also, in the text we contend that predicting high-latitude hydrological changes over the coming century is critically important to obtaining useful results.

19. Pg 37 paragraph that continues on from the pervious page. Exactly my question above - is this the critical issue to advancing on this problem. Is it a show stopping high hanging piece of fruit and can you demonstrate it is or is not?

Accurately predicting the inundated fraction (magnitude and dynamics) globally is a critical direction for future research, and represents a topic that many groups are currently attempting to address.

20. Pg. 37 section 4.2 The problem with chambers is they provide observations close to the scale of the variability of emissions. But without some scaling methods they are not likely to provide reasonable estimates for the ecosystem, let alone grid, scale fluxes. EC measurements provide a larger spatial scale of integration but many of the functions you have in your model are not from this integrated scale but from finer scale process work. The satellite data is at appropriate scale but they do not, at present, give surface fluxes or at least this has not been demonstrated.

Thank you for these comments and observations, which we agree with. We have added a sentence to this section to highlight these issues.

21. Many of the observations from northern wetlands are EC also. You report these in

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Table 3 so why isolate the tropics.

Most of the observations from northern systems are from flux chambers (Table 3).

22. Pg. 38 No e on Degero

Thanks for the comment; we have corrected the spelling throughout the text.

23. Pg. 38 second paragraph Is this assumption valid? Whiting & Chanton demonstrated a relationship but it really is based on two, apparently separate populations. There are many wetlands in there relationship that show little or no relationship between NPP & CH4 and then wetlands that do - these are the graminoid dominated wetlands. But is it valid to extend this relationship to all wetlands as you do here?

We did not intend to extend this relationship across sites, but rather to indicate that errors in NPP may be consistent with errors in CH4 emissions. We note that several current large-scale CH4 models explicitly relate CH4 production to NPP.

24. Pg. 40 first paragraph You can get some of these from the sites that you use - e.g. Temperature, possibly pH, but there are no reasonable measures of in situ substrate production (incubations probably give reasonable comparisons of relative difference in potential). There are no good measures of the CO2 to CH4 ratio from the field. These are laboratory estimates and there is no evidence they apply in situ. Why would we assume, given the temporal variability, that this ratio should be constant?

We agree that there is substantial variability and uncertainty in this parameter, and highlight these problems in the Discussion.

25. Pg. 41 Section 4.4 Given the uncertainties is 20% even considered differences. Why not conclude the models produce the same results given their relative confidence intervals.

This result represents a difference between two realizations (resulting from different climate forcing and holding all the uncertain parameters and processes constant) of

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the same model. In the Discussion we highlight all the reasons this difference is very uncertain, and discuss why we believe all global models suffer from the same problems.

26. Pg. 41 introduction to sensitivity analysis - It does beg the question that without realistic vegetation is the effort on the aernchyma a good investment. It is to the community as it's inclusion is probably one of the more important factors in getting the methane emissions right, but it does need to be present or absent -I.e. You have to know the vegetation to know when it should be applied. This is going to be a significant challenge.

We agree that characterizing vegetation properties and presence accurately is critical to making accurate predictions of CH4 emissions. In response to this comment, we added some discussion of these issues to the Discussion section.

27. Pg. 41 - 47. Far too long. The audience will know is much of the literature you review here. What they really want to know whether your model captures the expected sensitivity to these variables? When it does good, when it does not - why not? Is it because there is something missing in the model? Is it because the empirical studies do not take into account the numerous variables involved I.e. The emergence of complexity?

We felt it important to discuss the literature in some detail because we wanted to emphasize that most models applied at large scales do not account for many of the important processes affecting CH4 emissions. Our goal here was not to analyze why our model performed well or poorly, but rather to discuss a broad outline of what needs to be accomplished to improve the next generation of CH4 models. We discuss both the needed empirical studies and advances in modeling that will be required to create credible large-scale CH4 emission estimates from bottom-up models.

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