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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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Good paper addressing an important issue. There is a need for global climate – biogeochemical models to incorporate wetlands and their role in the global methane cycle. Or maybe more correctly, there is a need to be able to assess whether changes in wetland emissions over the next century will be an important feedback to consider. The model presented in this paper build of several previous models on global methane emissions from wetlands and the authors give due credit to the work that has been done before them. However, what is unique and original in the current manuscript is the model they

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develop is for inclusion in a global climate model so eventually a full coupled simulation with climate, permafrost, carbon and methane could be preformed. This model is still some way away from that stage but it the first serious step to this coupled step. All previous work has been very good, but uncoupled approaches.

The is a very long paper, but much of the detail is needed. It is asking a lot of the community to wade through the entire paper and I have my doubts that many will get all the way through. This will be unfortunate because the approach is interesting in many places. I am not sure how to reduce the paper – some of the detail in the model development could be allocated to the appendix and the sensitivity analysis section of the discussion is far too long – it could be easily condensed if it was more focussed.

Below I have numerous comments and points. None of these are what I would consider serious or potentially fatal but they are comments that arise from my confusion. It would have helped considerably if the authors had used line numbers for their manuscript. I apologize if my location indicates are mixed up.

Pg. Ln 6 This statement is a little presumptuous. Let the reader decide this after they have read and digested your paper.

Pg. 11 Where does the value of 0.2 for $f\mathrm{CH}_4$ 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.

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

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

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conclude from this - you do not think his criticisms were valid?

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

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.

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?

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?

Pg. 20 first 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?

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?

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

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? 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.

Figure 5 Why does the greater Q_{10} 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 Q_{10} lead to lesser emissions? Is it because a difference between the temperature sensitivity between production and oxidation.

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?

Figure 7 Makes sense - little or no oxidation with bubble flux but some with plant transport. If plants transport more then conc. decreases and the bubble flux cannot be sustained. Consistent.

Figure 8 What is the interaction between inundation and the fraction of annual CH_4 aerenchyma oxidation?

Pg. 33 section 3.7 There have been empirical studies by Morrissey that showed stomatal loss of methane was minimum but the same study showed considerable leakage from the pores in the plant streams of sedges.

Pg. 33 section 3.8 Is the decrease in high latitude wetlands reasonable. Over 90%

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of these wetlands are peatlands. Looking at peatlands in places like Alberta where P $\sim$ Et it is unlikely that the parameterization you have used for decrease in inundation applies in the peatlands.

Pg. 35 first paragraph Does this explain the difference in the Q_{10} response referred to earlier. I am still at a lose to explain why this is the case. What is interplay of variables and functions here?

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?

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?

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.

Many of the observations from northern wetlands are EC also. You report these in Table 3 so why isolate the tropics.

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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?

Figure 10 Good appears to be a scale appropriate comparison, except there is no way of confirming it is without appealing to Ockham's razor.

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 CO_2 to CH_4 ratio from the field. These re 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?

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.

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.

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?

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