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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.

Anonymous Referee #1

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Referee report on:

Barriers to predicting changes in global terrestrial methane fluxes: analyses using CLM4Me, a methane biogeochemistry model integrated in CESM

Riley et al. Biogeosciences.

General Comments:

Riley et al. present a description of a new wetland methane model (CLM4Me) integrated into the CLM4 model. CLM4Me is a high complexity wetland methane model

based upon several previous models (including [Walter et al., 2001; Wania et al., 2010] [Zhuang et al., 2004]) and some interesting innovations. The model is evaluated over the modern period and is used for projections up to year 2100.

I have some concerns about the model being over parameterized with many components either difficult or even impossible to verify with real world measurements. I also feel that some of the complexity of the model was poorly proportioned (e.g. large effort expended on various transport mechanisms of methane from the site of formation to the atmosphere, each mechanism with a high uncertainty about how well these are performing; but no prognostic wetland finding scheme or wetland PFTs). However, with those stated concerns, I find the authors did a commendable job of stating the model weaknesses and exploring the model with sensitivity analyses. The authors are also very clear about the problems with such a high complexity model, i.e. over parameterization. While I am not fully convinced that their model is able to truly represent wetland methane dynamics (given the level of parameterization), it does present a worthwhile step forward and also advances our understanding of the strengths and weaknesses of high-complexity wetland CH4 modelling. Given the authors careful dissection of the model and openness to explain its weaknesses, I can find no major faults with the paper. I recommend publication with only technical revisions.

In general the writing and layout of the paper is very good. The paper is very long and ideally would be shorter. However, the paper does present a lot of material and is not repetitive.

Specific comments:

abstract L18: I think you don't intend the 'm-2'.

p1735 L6: Put the sources in the same order as they are of magnitude of flux.

L18: Anoxia? Anoxia is usually important for methane production...

p1736: You don't list any papers by Kaplan [Kaplan, 2002; Kaplan et al., 2006] or

Christensen [Christensen et al., 1996] using a simple wetland finding approach

p1737 L4: If this high latitude band was not defined the same as your band in the paper, please state it so the reader can compare.

L14: This source is not confined to trees. Also there has been numerous papers [Ferretti et al., 2006; Nisbet et al., 2009] of late that suggest this source is much less than the original paper [Keppler et al., 2006] suggested. Those should be referenced for an estimate instead.

p1738 L27: Is not the [Shindell et al., 2004] estimate that you describe on line 18 a tropical estimate?

P1739 L9: put year in from of 2100

P1741 L7: I would also add to this list 1. Limited knowledge of the effect of subgrid scale heterogeneity, and 2. Limitations of the input datasets. The limitations of the input datasets is a major problem in my view, yet it is not mentioned in the paper. I think this should be highlighted more.

P1742 L8: Am I right in understanding that while you have an inundated and non-inundated fraction, the grid cell is not given a tiled treatment? i.e. the grid cell PFT's are not necessarily growing in the conditions assigned to the different gridcell fractions? This would also have strong implications on the soil temperature and moisture regimes.

P1743 L8: What are your datasets for pH and redox potential?

P1745 L7: Can you include the Arah and Stephen expression?

L14: How was the 57% arrived at?

P1746: Does CLM4 have dynamic root distributions? If not, the aerenchyma transport seems highly over parameterized.

P1747: You change the Roxid,max label on P1748 L2

P1749 L4: Capitalize Liyama

P1751: I don't understand how your approach could be used for future projections, based as it is upon the Prigent dataset. How is this able to be prognostic?

P1752 L5: Do the values of p1,p2, and p3 vary between gridcells or are they assumed constant?

P1753 L16: Write out CLAMP fully.

P1754 L14: Write out RCP fully.

General: So what was the grid cell resolution of your simulations? The usual 0.5 x 0.5 degree CLM grid?

P1756 L12: You could mask out the rice areas. . .

L26: I think this is likely a result of the heavy parameterization of the model.

P1757 L5: Don't cite Meng et al. like a published paper. Your sentence should make it obvious that this is an 'in prep' paper.

P1757 L12: The [Beer et al., 2010] paper was GPP, not NPP

P1757 L24: Again I don't think you want the m-2 there.

P1758 L10: Perhaps change 'predicted' to 'simulated'

L13: Can you quantify the 'little change'

L14: I could not find how the anoxic microsites were defined, simulated or chosen?

P1759 L2: 7b has no 'b' label.

L3: Make life easy for the reader, give the default value again here.

L9-19: Given all this, how do you know what you produce is at all realistic?

P1760 L3: Tropics, mid and high latitudes. . . so was it also 20% higher globally?

P1761 L12: If you are not showing the data, then provide better quantification of the differences.

P1763 : Given the major problems with using site scale measurements to evaluate a large grid model, why did you not try air mass back trajectory [Worthy et al., 2000] or a more regional satellite and site measurement approach [Melack et al., 2004]? Basically, I think you should have tried for an in-between of the satellite/inverse-models approach and the site-level measurements.

L19: These sites are all high-latitude, were these chosen to minimize the problems CLM4 has with low latitude NPP?

P1765 L17: Given how uncertain your model results are, and the high level of parameterization, I am not sure this would be an improvement for the inversions.

P1779 L16: We seem to be missing part of this line? The sentence does not make sense.

P1780 L12: I do not understand this annual average seasonal inundation factor. What does a value of 0.95 mean? Please provide a better description of this factor.

Table 1:fpH and fpE if these are set to 1, how does the on and off sensitivity test work?

Fig 7: Since it is only 1 year, maybe put months instead of fraction of year to make it easier to read.

Fig 8: So the areas in the Sahara and the centre of Australia have some methane production? Or how should this be interpreted?

Fig 9: What do the different colours of the bars mean?

Fig B1: I don't understand the annual seasonal inundation factor so this figure is very confusing.

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