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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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We would like to thank this reviewer 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).

Responses to Reviewer #1 comments

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

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Discussion Paper

Corrected, as indicated by the reviewer.

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

Done.

3. 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.

We added citations to Kaplan (2002) and Christensen et al. (1996).

4. 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.

We have added the latitude range for these studies (north of 50°N).

5. L14: This source is not confined to trees. Also there have 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.

We've added a citation to Ferretti et al. (2006); Nisbet et al. (2009) was already cited in the following sentence.

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

As indicated in the text, the doubling is dominated by increased tropical emissions.

7. P1739 L9: put year in front of 2100

Done.

8. 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.

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As suggested by the reviewer, we have added to our list the uncertainties associated with spatial heterogeneity and limitations of measurements for performing simulations.

9. 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.

No, the grid cell is treated as a combination of tiles with various plant functional types. The issues associated with not properly resolving the specific properties of wetlands are discussed in the Discussion section of the paper.

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

To address this comment, we added text to the Methods section indicating that we are only applying these factors as a sensitivity test, describe in more detail the redox potential factor applied, and indicate that the pH factor is described in detail in Meng et al. (2011).

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

As requested by the reviewer, this expression has been added to the text (new equation (4)).

12. L14: How was the 57% arrived at?

The parameters used in the ebullition model were taken from Kellner et al. (2006), Strack et al. (2006), Baird et al. (2004), Wania et al (2010). In response to the reviewer's comment we have more clearly identified the source of the ebullition parameters in the Method section.

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

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The impacts of aerenchyma in affecting the net surface CH4 flux are large and very uncertain. We added further discussion of this uncertainty, and indicate observations and experiments that could help improve the models, in the Discussion section.

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

Corrected.

15. P1749 L4: Capitalize Liyama

That letter is not an “L”, but an “l”; unfortunately with the font used in the paper it is unclear.

16. 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?

Equation (10) describes our approach for prognosing the fractional inundated area. We assumed that the parameters based on the inversion to historical inundation observations remain constant in the future. For clarity, we have indicated that equation in the second paragraph of this section.

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

They are different for each gridcell. In response to this comment we have more clearly described this feature in the text.

18. P1753 L16: Write out CLAMP fully.

Done.

19. P1754 L14: Write out RCP fully.

Done.

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

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1.9° x 2.5°, as described in Section 2.6.

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

Inclusion of rice agriculture in CLM4 is being undertaken in a related paper submitted to Biogeosciences (Meng et al.).

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

This observation is consistent with our conclusions.

23. 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.

We have changed the citation to 'submitted', and note to the editor that this paper has been submitted to Biogeosciences.

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

Corrected.

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

Corrected.

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

Done.

27. L13: Can you quantify the 'little change'

The change was on the order of 0.1 Tg CH4 yr-1; we have added a sentence to this effect.

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

As requested, we have added an equation describing the Arah and Vinten approach for micro-site CH4 production.

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29. P1759 L2: 7b has no ‘b’ label.

Done.

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

The relative change is the point of this modeling exercise; we therefore prefer to leave the result in terms of a fractional change.

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

This question is one of the critical questions that we identify in the paper, and emblematic of our conclusion that there are currently substantial barriers to accurate prediction of terrestrial CH4 emissions by any of the extant regional-scale models.

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

Yes. We have added a phrase to make this clearer, as suggested by the reviewer.

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

To address this comment, we describe the effects as fractional changes to the default scenario, and differences between the tropics and high latitudes.

34. 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.

We did compare our results to several global inversion models (Figure 5) and discussed the limitations of evaluating these types of global-scale surface emissions models against atmospheric inversion estimates. In the Discussion section we also indicate that a combined mechanistic land-surface model and atmospheric inversion approach

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could be a productive next step.

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

No, they were chosen because of our group's current focus on high-latitude systems. The point of this exercise was solely to illustrate the impact uncertainty in substrate production has on CH4 emission estimates.

36. 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.

In this sentence we include other models of the class of CLM4Me. Regarding the model uncertainty, we are arguing in this paper that all models of this class suffer from a similar set of problems. Further, having a realistic understanding of uncertainties will be a critical component of the coupling of models of this class with atmospheric inversions.

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

Yes, we have corrected this sentence.

38. 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.

The seasonal inundation factor effectively reduces the CH4 emissions for a system that is seasonally inundated (Equations 2 and 3). To address the reviewer's request for improved clarity, we refer to these equations in the introduction paragraph of Appendix B.

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

We have added text to the Methods section describing how these sensitivities are de-

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fined. For the pH factor, the model is run with no sensitivity to pH and the spatially distributed method described in the Methods section. For the fpE factor, we applied a 30-d lag that is now more clearly described in the Methods section.

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

We prefer the fractional year label.

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

No, Figure 8 refers to the fraction of CH4 produced that was oxidized in aerenchyma. There are very low predicted CH4 fluxes in these areas (Figure 6), so the fraction oxidized is also very small, and not very informative. To clarify this issue, we have masked out gridcells with net CH4 emissions less than 0.1 mg CH4/m2/d.

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

We apologize for the confusion: the colors refer to the different scenarios simulated for each parameter combination. We've added a description of the scenarios to the Figure 9 caption to clarify the scenarios.

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

We have added a reference to Equations (2) and (3) that define how the seasonal inundation fraction is applied to estimate CH4 production.

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