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Interactive comment on "Reviews and syntheses: Four Decades of Modeling Methane Cycling in Terrestrial Ecosystems" *by* X. Xu et al.

Anonymous Referee #3

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Comments on the manuscript by Xu et al "Reviews and syntheses: Four decades of modeling methane cycling in terrestrial ecosystems" submitted to Biogeosciences Discussions.

Overall Evaluation

This manuscript presents a review of approaches used to model methane dynamics in terrestrial ecosystems in the last four decades. The review largely focuses on describing the variability in structure and mathematical descriptions of processes among 39 terrestrial methane models. Parameterization issues are touched upon in the section on environmental controls, mostly with respect to variability in Q10 (which affects temperature sensitivity of processes). The discussion makes suggestions for adding more complexity to methane models, primarily along the lines of more explicitly considering

microbial processes and dynamics. The discussion finishes with identifying knowledge gaps, modeling challenges, data needs, and the need for data-model integration.

This manuscript tries to cover a lot of ground. The primary strength of the manuscript, in my opinion, is largely in the description of variability in mathematical descriptions of processes. The other aspects of the review didn't provide a lot insight in my opinion, as the issues discussed were in many cases just touched upon and were not well developed. My main concern about this manuscript is that in trying to cover a lot of ground, it covers some of that ground poorly. I think there are several issues to address to improve the review. First, I think there are some general organization issues that could be addressed to improve the manuscript. Second, there are a number of cases in the presentation of putting the "cart" before the "horse". Third, I didn't find that the description in the variability in structure (as depicted in Figure 3) was based on an objective evaluation of the 39 terrestrial models. Fourth, there a number of assertions in the manuscript that should be presented as more open issues. Fifth, the challenge of scaling is only touched upon in the manuscript and needs to be better developed, and there is a need for some discussion of reconciliation with atmospheric data analyses. Sixth, beside the scaling/reconciliation issue, I also found several issues that need to be better developed/discussed including the modeling of ebullition, vertical representation of processes, model benchmarking, and data-model integration. Below I go into more depth on each of these issues, and finish my review with a listing of specific comments.

Issue 1: Organizational issues in the manuscript. The manuscript starts out well, but then gradually gets more and more disorganized. There is a lot of overlap of material between some of the later sections of the manuscript that could be eliminated with a more effective organization. Perhaps consider the organization of Luo et al. (2016, Global Biogeochemical Cycles), which review soil carbon models. The organization of that paper is (1) model structure, (2) model parameterization, and (3) external forcing. I think additional in this manuscript concerns scaling and reconciliation with atmospheric data. The strength of this manuscript is that it generally does a good job of reviewing model structure, but a rather inadequate job of reviewing model parameterization, external forcing, scaling, and reconciliation issues.

Issue 2: "Cart" before the "Horse" issues. There are a number of places in the manuscript where the "cart" comes before the "horse", from the perspective of this being a review paper. For example, the citation to Figure 2 on line 162 talks about the timeline for inclusion of "key mechanisms", but these mechanisms haven't been described in a general sense yet. Table 2, which contains the list of "key mechanisms" isn't cited until line 175. Even when Table 2 is cited, the general reader gets no background on these mechanisms/features of models, as it is not used beyond a simple citation at the end of a sentence. Other rough spots in the manuscript involve adequately describing terms used in the manuscript. For example, acetoclastic and hydrogenotrophic methanogenesis suddently appears on lines 238-240 without any prior description. "Advective transport" (line 203) is also not described.

Issue 3: Analysis of the variability in structure. What is the basis for defining three different types of models? It seems to me that this could be done in a much more objective fashion by doing some sort of cluster analysis among the 39 models reviewed in this study. Information from Tables 1, 2, and 3 could be put into an objective cluster analysis so that we better understood what factors seem to cause models to be distinct (or not distinct) from each other.

Issue 4: There are a number of assertions in the manuscript that have not been justified by any sort of rational analysis/argument. For example, why make a recommendation in the last sentence of section 4 (lines 196-198) on the third types of models as the means of moving forward with respect to improving reduced form models for application in Earth System Model applications? First of all, this is too early in the manuscript. Second, doesn't making this recommendation conflict with the sentence on lines 211-212 that the optimum complexity remains to be determined? At the end of section 6 there are four recommendations for models "based on the above-mentioned needs" and a citation to Figure 4. I didn't find the previous text in section 6 as being very

helpful for establishing these as the top needs. This all comes before the section 7, which talks about knowledge gaps and data needs. The arrows for benchmarking and data assimilation in Figure 4 have not been developed, and the issues of vertical transport/diffusion have only been touched upon. Also, the top recommendation that "the models (features?) should be embedded in an Earth System Model" seems strange to make here. The point here is that arguments have not been well enough organized and crafted to effectively make these recommendations. This sort of all gets back to issues 1 and 2 above. Finally, I can't say that I'm very fond of Figure 4 as being the synthetic figure for this manuscript – we've seen a lot of these sort of figures over the years. I suggest thinking about something that is truly synthetic based on this manuscript.

Issue 5: The issues of scaling and reconciliation with atmospheric data. Scaling is an important issue. It does pop up several places in the manuscript as a sort of "between the lines" issue, but it really needs its own section. I also think that the issue of reconciling model applications at particular scales with data from atmospheric analyses needs to be part of the discussion.

Issue 6: Other issues. I also found several issues that need to be better developed/discussed including the modeling of ebullition, vertical representation of processes, model benchmarking, and data-model integration. For example, transport mechanisms don't even show up as key features in Table 2, although they do appear somewhat in Table 1. These issues are touched upon in several places in the manuscript, but are not really effectively dealt with in a meaningful way.

Specific comments

Line 104-105: "contributes" is not really the right verb to use here. Just says "varies from 1 to 90%", for example.

Line 106-107: I really don't know what you mean by "oxidation of atmospheric CH4 contributes". Aren't all of the previous mechanisms in this paragraph ultimately oxidation of atmospheric CH4, albeit in the open pore space of the soil.

Line 109: Perhaps start a new paragraph after "methanotrophy.".

Lines 109-116: There is no information for the uninitiated reader to understand how these pathways differ from each other.

Line 120: I think this might be the only occurrence of "wind speed" in the manuscript. What do you mean by "wind speed" as an environmental factor.

Line 121: Define what you mean by "indirect" vs. "direct" environmental factors.

Line 147: I don't think Fan et al. (2013, Peatland DOS-TEM) has anything to do with the Zhuang et al. (2014) model in that it has a number of different features and to my understanding the two models do not share any code base.

Line 162: As mentioned earlier, the reader needs to know more about the key mechanisms before you present/interpret Figure 2.

Line 175: Need to make better use of Table 2 in the manuscript. As I indicated earlier, transport mechanisms need to be included in Table 2.

Line 213: Does use of "first group of models" refer to model types in Figure 3, or to the first set of empirical models referred to in the first paragraph of section 4.1?

Line 238-240: Where does the information on acetoclastic and hydrogenotropic methanogenesis appear in Table 3? Note that these production processes have not been defined for the reader.

Line 280: Why is Zhuang (2004) cited here in the context of immediately transporting CH4? This model is primarily a monthly model with a pseudo-daily time step. This transport issue is an important temporal scaling issue, and one which should appear in a separate section on temporal scaling.

Line 286: I think you should change "will likely" to "can".

Line 287: I think you should change "impossible" to "not straight forward".

Line 291: I note that ebullition is not adequately treated in this section (section 4.4).

Line 292: Why is this the "final" bottleneck, or why is even referred to as a "bottleneck".

Line 303: Define advective transport.

Line 313: I think you should change "most" to "some". Note that ebullition seems to be ignored in these three "transport" challenges. It is a dominant pathway in some systems.

Line 319: I note that the simulation of variability in some environmental controls is not adequately treated in section 4.5 on environmental controls.

Lines 331-332: I think that this sentence needs to refer to Eq 9, 10, and 11 instead of 10, 11, and 12. Note that the third function in Eq 9 is essentially equivalent to Eq 10 in that the Q10 can be derived from the exponent.

Line 347: I think you mean Eqs. 13-16 instead of 12-15.

Lines 356-367: Do any models represent pH variability in time? It would be useful to know how models represent pH variability in space.

Lines 393-394: Why is the comparison of high frequency observational data needed for future model-model inter-comparison? I think it would be most important to high quality seasonal and interannual estimates derived from observations to effectively test and compare models.

Line 405: With respect to shifts, are you referring to shifts in time or in space?

Line 479: What do you mean by "order 1-10". Do you mean by a "factor of 1-10"? The language could be confused for "orders of magnitude".

Luo, Y., A. Ahlstrom, S.D. Allison, N.H. Batjes, V. Brovkin, N. Carvalhais, A. Chappell, P. Ciais, E.A. Davidson, A. Finzi, K. Georgiou, B. Guenet, O. Hararuk, J.W. Harden, Y. He, F. Hopkins, L. Jiang, C. Koven, R.B. Jackson, C.D. Jones, M.J. Lara, J. Liang, A.D. McGuire, W. Parton, C. Peng, J.T. Randerson, A. Salazar, C.A. Sierra, M.J. Smith, H. Tian, K.E.O. Todd-Brown, M. Torn, K.J. van Groenigen, Y.P. Wang, T.O. West, Y. Wei, W.R. Wieder, J. Xia, X. Xu, X. Xu, and T. Zhou. 2016. Toward more realistic projections of soil carbon dynamics by Earth system models. Global Biogeochemical Cycles 30:40-56, doi:10.1002/2015GB005239.

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