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

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[We really appreciate the comments, which significantly improve the manuscript in terms of clarity and organization. All detailed point-by-point responses are listed below.]

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.

[We appreciated the positive comments.]

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.

[We have made a substantial revision to address the comments. Specifically, (1) we reorganized several sections to make them clearer, particularly the modeling section; (2) we clearly revised some statements to make them more consistent with the results; (3) scaling is not the key focus of this review, therefore, we did not expand writing on scaling and its reconciliation with atmospheric data analysis; yet we did emphasize that satellite data of atmospheric CH4 concentration could be used for model validation; (4)

we added a section for discussing presenting CH4 module in ESMs.]

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.

[We did remove some redundancies in different sections. This manuscript is not designed to cover model parameterization and external forcing specifically. Those two sections were discussed briefly model development perspective.]

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.

[For the citation to Figure 2, although those processes and their representation in models have not been reviewed, yet the processes themselves have been reviewed in

"primary CH4 processes" section. For the acetoclastic and hydrogenotrophic methanogenesis, we have included detailed definitions at their early occurrences. The advective transport has been defined as well.]

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.

[We really appreciated the suggestion of doing a cluster analysis. We did a cluster analysis based on model characteristics of representation of methanogenesis processes, methanotrophy processes, transport pathways, oxygen availability, multiple soil layers etc. All 40 models could be classified into three groups, which is consistent with our previous classification. See the updated Figure 3 and relevant text.]

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.

[We have revised the manuscript to address all comments. The recommendation of the third group of model in the early section of the paper has been removed as suggested. Then it is not in conflict with later section as reviewer suggested. We added description and summary of CH4 model representation in ESMs, the model classification have been done in a mathematical way – a cluster analysis. Other writing issues have been addressed as well. The original Figure 4 is a framework showing future model development as we envisioned, it is combination of summarized and visionary framework. Although it is little lack of evidence, we do believe it will be the key direction for CH4 model development and application.]

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.

[Since scaling and reconciliation are not the key focus of this manuscript, we did not plan to expand that section in this revision.]

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.

[We have added more text and wordings to make those statements strong and solid.]

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

[We still keep "contributes" because it emphasizes the contribution of individual process to the total oxidation or production.]

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.

[It emphasizes the oxidation of atmospheric CH4, taking up CH4 from atmosphere. This process is defined to distinguish from oxidation of CH4 produced from soils.]

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

[We separated it as a new paragraph.]

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

[We added one small paragraph to define different transport pathways.]

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.

[We revised the manuscript to have a bit more description of wind speed impacts on CH4 flux.]

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

[We revised the manuscript to define the direct and indirect 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.

[We have confirmed with Dr. Zhaosheng Fan, and treated the DOS-TEM as another independent CH4 model in the revision.]

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

[We have added definition for some key mechanisms in the manuscript.]

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.

[We expanded the Table 2 to include the model information on CH4 transport pathway.]

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?

[We have revised the manuscript to be clearer on this issue. The CH4 models were classified as groups, while methangoenesis was categorized as model algorithms.]

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.

[We have added definitions for acetoclastic and hydrogenotropic methanogenesis in the revised manuscript.]

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.

[Thanks for pointing out this inappropriate expression. We have removed citation of Zhuang (2004), and added another model as an example.]

Line 286: I think you should change “will likely” to “can”.âĂĺLine 287: I think you should

change “impossible” to “not straight forward”.

[Revised as suggested.]

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

[We do have ebullition in the section. In the revision, we have revised the section to have more specific information for ebullition.]

Line 292: Why is this the “final” bottleneck, or why is even referred to as a “bottleneck”.
Line 303: Define advective transport.

[We revised it to bottleneck, and added definition of 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.

[Revised as suggested.]

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

[We have revised the section to better describe variability in 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.

[Mistake corrected.]

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

[Mistake corrected.]

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

[We agree that pH variability is important and only few models consider dynamics of pH in soil over time and across space. Due to recent studies suggesting the importance of pH on CH4 flux, it would be noteworthy to point out its importance for future model development.]

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.

[We have revised to reflect this point.]

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

[We have revised to clarify it is temporal shifts.]

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”.

[We have revised it to a factor of 1-10.]

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