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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 reviewer for the comments, which significantly improve the manuscript in terms of clarity and organization. Specifically, we 1) removed redundancies; 2) emphasized the importance of spatial maps of wetland data; and 3) addressed many other minor comments. All detailed point-by-point responses are listed below.]

General comments In this manuscript, the authors reviewed 39 terrestrial methane models and discussed their limitations and future opportunities. This kind of model review has been partly conducted in introduction of model intercomparison project (e.g., WETCHIMP; Melton et al., 2013, Wania et al., 2013), but I agree that this manuscript gives a more thorough overview. The 39 models were classified into several categories (or generations) from the points of processes and complexity. Also, the authors gave

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good overview of underlying mechanisms of methane production, consumption, and transportation. In the light of its importance as the second important anthropogenic greenhouse gas, this manuscript is timely and within the scope of the journal.

[We appreciate the positive comments.]

The manuscript is fairly prepared, but I have several recommendations. First, I felt redundancies in the manuscript. For example, influential factors of methane processes are similarly listed in Page 5 Line 118 and Page 12 Line 322. I recommend refining the manuscript by reducing redundancies. Second, I recommend giving a broader picture of terrestrial models that include methane processes. The authors mentioned that methane schemes would be implemented into Earth system models (ESMs). Similarly, integrated terrestrial models (other than ESMs) should include methane processes to evaluate e.g. the effect of mitigation practices. Overall, I recommend that the manuscript be worth publication after moderate to major revision.

[We have carefully revised the manuscript and removed redundancies. We have also added a paragraph to discuss the implementations of CH4 module in ESMs.]

Specific comments Page 3 Line 65 This manuscript does not cover several quantitatively important processes such as methane emissions from biomass burning, termites, and ruminants. Please justify here for ignorance of these processes.

[We totally agree that CH4 emissions from biomass burning, termites, and ruminants are important. While important, these processes have not been included in this manuscript because they are not the focus of this paper.]

Page 5 Line 133 In the 1980s, E. Mattews and I. Fung (1987) achieved a pioneering work in which not only terrestrial but also atmospheric methane dynamics were simulated at the global scale. I think that their work should be mentioned in text.

[We acknowledge this pioneering work, although we did not include it because the approach in their paper is simply multiplying wetland area with measured CH4 fluxes.

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It is not a modeling approach as we described. In this revision, we did cite this important work but did not treat as an independent ecosystem CH4 model.]

Page 6 Line 159 In Figure 6 of Wania et al. (2013), estimations of methane production area in the contemporary models are well summarized.

[In the revised manuscript, we added text to emphasize the importance of spatial maps of wetland distribution, and acknowledge the review of CH4 production area has been done for a group of models in Wania et al. (2013).]

Page 7 Line 190 âĂĺCan you give several examples for the second group model?

[We have added few model examples as suggested.]

Page 8 Line 193âĂĺ Can you give several examples for the third group model?

[We have added model examples as suggested.]

Page 9 Line 233âĂĺCan you show the 31 models by adding a column in Table 1? Page 9 Line 244âĂĺ“address” should be “addressed”.âĂĺ

[We appreciate the comment, yet we did not add it as a new column because the information has been shown in the Table 2 in a different format.]

Page 10 Line 246 and Table 1. In addition to Ridgwell et al. (1999), several methane oxidation models have been presented and could be mentioned here: e.g., Del Grosso et al. (2000) and Curry (2007).

[We do have DAYCNET, CLASS models reviewed and summarized in the Table 1.]

Page 10 Line 251 Can you indicate a typical value of the contribution of anaerobic methane oxidation in total oxidation?

[We did have this rough estimates in the primary CH4 processes section.]

Page 11 Line 275 In terms of the modeling of vertical profile, parameterization of methane diffusion coefficient within soil is critically important. Do you agree?

[We totally agree that diffusion parameter is very important in terms of simulating vertical profile of the biogeochemical processes and CH4 flux. Yet it is not focus of current review as current paper emphasizes model structure and mechanisms. We did discuss this important parameter in our revision.]

Page 13 Line 35 Yvon-Durocher et al. (2014) implied that the temperature response of methane emission would be evaluated using a single consistent model. If correct, the divergence in present models would be largely reduced. Do you agree?

[We would agree that Yvon-Durocher et al's approach is applicable for single CH4 process. Since the observed CH4 flux is a combination of many different processes. Using a single consistent model might not be the best way to represent CH4 flux. Yvon-Durocher's approach provides a theoretical understanding of some consistencies between observed CH4 fluxes across space.]

Page 14 Line 356 As long as I know, only a few global dataset of soil pH is available. Also, in situ measurement and model prediction of soil pH are rather difficult. I think these difficulties in using soil pH should be noted.

[We agree that global dataset of soil pH is lacking, yet a number of field experiments and modeling studies do confirm the importance of soil pH to CH4 flux. We did note the difficulties for modeling soil pH in the revision.]

Page 15 Line 380 It looks wired to give a summary at this place, because it is usually given at the end of the manuscript. Actually, the statements around Page 16 Line 411 are as if your conclusion.

[This summary section is a short paragraph for CH4 modeling section only, while the last conclusion section is for high-level summary and key findings for the whole manuscript. We would still keep this section but make it as a sub-section of modeling section.]

Page 18 Line 460 A few more processes not mentioned here have been presented:

e.g., emission from tank bromeliads (Martinson et al., 2010) and emission from small ponds (Holgerson and Raymond, 2016).

[We have included these new findings in the manuscript and identified them as a knowledge gap and future direction for modeling community.]

Page 19 Line 504 I recommend adding one more (6th?) challenge. Modeling of human-natural processes such as emission from managed ponds and estuaries is important in terms of mitigation. Namely, we should consider both natural biogeochemical processes and human management effects.

[We have added it in the revised manuscript as suggested. We appreciated the reviewer for pointing this out.]

Page 21 Line 540âĂĺDo you mean “Markov Chain Monte Carlo (MCMC)”?

[Mistake corrected.]

Page 25 Line 623âĂĺPlease correct information for Bohn et al. (2015):

[Mistake corrected, thanks.]

Figure 4 Can you include the microbial community factor into the figure?

[We have revised the figure to show several different functional groups of microbes that control the CH4 processes.]

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