

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

Anonymous Referee #1

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Review of “Reviews and syntheses: Four Decades of Modeling Methane Cycling in Terrestrial Ecosystems” by Xiaofeng Xu et al. submitted to Biogeosciences Discussion.

General comments: This manuscript provides an overview and a synthesis of the evolution of models focusing on methane emissions from terrestrial ecosystems. The manuscript is based on a comparison among 39 methane models described in peer-reviewed articles, followed by a general synthesis that includes outlines of future challenges and directions in the field. I read the review with interest; it is a review which as far as I know has not been done before. Understanding the current state and potential future challenges of methane modelling will be of interest both to field researchers and for new modelling projects. The manuscript also has shortcomings that I think should be addressed before publication is considered. First, I find that the overall presentation can be improved for increased clarity, particularly with regards to sentence and

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paragraph structure. I have several examples in the specific comments below, but an overall assessment is recommended.

I also think the introduction could do a better job in outlining the scope of the manuscript, particularly I would favor some more specific information, e.g. “first we will give an overview of the range of processes that have been considered in methane models, based on this we will classify existing models as determined by the range of processes considered. The following sections will review and synthesize how models deal specifically with methane production, consumption and transport within soils. . . . etc.” I also recommend the authors to better define several key concepts in the manuscript. This would include your definition of a terrestrial ecosystem (see further comment below), and a definition of what constitutes a “primary process” with regards to methane dynamics (is this just your ranking of which processes that are more likely to have a stronger influence on the resulting emissions magnitude?).

The term “terrestrial ecosystems” is particularly important for this manuscript, since it defines the scope of the models that have been reviewed. How do you define terrestrial ecosystems? I.e. what is the distinction from aquatic ecosystems and why are not models of aquatic ecosystem methane emissions considered in this review? The review emphasizes the need to be able to estimate methane emissions at large regional to global scales, but aquatic ecosystems might (depending on your definition) have greater emissions than terrestrial ecosystems at the regional/global scale, so the omission of aquatic ecosystems is important. How do you define wetlands in terms of being terrestrial or aquatic ecosystems? The US and Canadian definitions of wetlands include open water wetlands with up to 2 m of standing water – are all these considered terrestrial in this review? Would it be considered a future challenge to extent the current models to include aquatic ecosystems, particularly streams, rivers, ponds and lakes?

Another topic that I do not think get sufficient attention in the manuscript relate to the diversity of goals for different models, and how that influences the choices made in the

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model development. In the introduction you bring up the fact that models can be developed for extrapolation to regional or global scales, or for process-level models that are developed to understand methane dynamics at the site level. The latter type of model requires information on many site-specific parameters (soil microbial community, iron and sulfur data etc etc), data which is not available for large regions. One recommendation in this manuscript is that more processes should be considered for methane model – however, for models aimed at regional to global scales this is likely to lead to highly unconstrained models since the data to run the models does not exist and is highly unlikely to be mapped. In short, I think there is a need to discuss how modelling goals will influence model development, particularly how this relates to available model data inputs.

The issue of spatial data availability, used as model input, is also not discussed in the manuscript. It is my belief that improved spatial data on wetland extents and wetland characteristics are likely to improve our accuracy of regional to global estimates of methane emissions (both magnitude and spatial patterns) much more than the incorporation of additional processes in the models. The use of different spatial products (wetland maps, inundation maps etc) for estimating global methane emissions is known to produce wildly different spatial patterns of regional methane emissions. I believe a discussion on how available data, and the use of available data, affect model development and modelling results deserve some attention in this review.

Specific comments:

P2 L37. I strongly discourage use the concept of global warming potential when discussing methane emissions from wetlands. GWP are only applicable when considering “new” sources, i.e. changes in emissions, but cannot be used when evaluating sustained emissions. Wetlands have been emitting methane for millennia, thus their methane emissions have a much lesser additional impact on climate forcing at this point than would be concluded based on GWP (unless they have increased as a result of climate change or by other means). See Frolking and Roulet et al 2007 Glob Change

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P3 L70-72. This is a weak sentence to finish an introduction.

P3 L74-76. Is it possible to reference the original sources?

P4 L83. What is meant by “primary CH₄ processes”? How do you distinguish primary processes from other processes? Do these primary processes include the 3 methanogenesis processes, 2 methanotrophy processes, and the 7 transport mechanisms? Several of these processes, which I assume are what you consider primary processes since they are listed in the sentence after your statement on primary processes, are not discussed in regards to how they are represented in models. E.g. methylotrophic methanogenesis is only mentioned once, and is not discussed with regards to how it is considered by models. Also, of the seven transport mechanisms you only discuss ebullition, diffusion and plant-mediated transport – what are the other four processes? Overall, I think you need a better framework for how you classify the different processes, including a motivation on why some of these processes are to be considered in the review and why other are not.

P4 L85. Clumsy sentence structure, omit “(depending on how one counts)”.

P4 L87. Importance in time and space – and you should probably highlight that it varies by wetland characteristics.

P4 L92. Perhaps a brief description is needed that explains the differences between acetoclastic and hydrogenotrophic processes, in terms of under what conditions they are more likely to dominate and why.

P4 L107. This is a awkward way of saying that upland soils are net sinks of atmospheric methane.

P4-5 L09-15. This sentence is very long and introduced several new concepts not previously described.

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P5 L16. This is the third time I have seen the same point being raised already - “process vary significantly depending on temporal and spatial scales”.

P5 L17. How do you define direct and indirect effects with regards to wetland methane emissions? It is not clear to me given the examples brought up. Is the classification of direct and indirect processes different from that of primary and other processes introduced earlier?

P5 L35. “Water sediments”, do you mean “Aquatic sediments”?

P5-P6 L36-57. I’m not sure this listing of the different methane models is effective. I would recommend merging this section with the section below (L181-199) on the different groups of model, i.e. to bring these models up as examples of each group.

P7 L66. What is your definition of regional simulation capability? This has not been presented.

P8 L09. Do you have any field data that can support your statement that substrate characterization is key for modelling methane production?

P10 L62. Unclear if you mean the third group of the three groups described in the “Model Classification” section or the third group described in the “Methanogenesis” section. I would recommend separating the models into groups once, rather than a new division of models in each section.

P10 L264. Seems appropriate to discuss substrate limitation and Michelis-Menten dynamics of methanogenesis in the section on methanogenesis rather than methanotrophy.

P17, L43. This sentence has poor structure, also, what is meant by “was not included in any of the three groups because that effort will likely be achieve over the long term”?

P19, L09. Can you give examples of less-studies ecosystems?

P20, L31. Sentence structure: “integration between model development and data col-

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lection is much stronger for advancing science”, do you mean that integration is important for advancing our scientific understanding of methane dynamics?

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