Biogeosciences Discuss.,
doi:10.5194/bg-2016-37-AC1, 2016


Biogeosciences Discussions
Open Access
EGU



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

**X. Xu et al.**

xxu@mail.sdsu.edu

Received and published: 19 May 2016

Reviewer #1

[We are deeply appreciated for the comments, which significantly improve the manuscript in terms of clarity and organization. Specifically, we 1) reorganized the introduction section; 2) defined the terrestrial ecosystems; 3) defined the primary CH4 processes; 4) revised the section for model purposes; and 5) addressed many other minor comments. All detailed point-by-point responses are listed below.]

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 chal-



Printer-friendly version

Discussion paper

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

[We have made a substantial revision to address the shortcomings as stated. See below for the specific responses to reviewers.]

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?).

[We have revised the manuscript accordingly. Specifically, we re-organized the last paragraph of the Introduction. The logic of the manuscript has been outlined at the end of the section. It highlights what we did for this review, which also addressed other minor comments in later section. The primary methane processes have been clearly described and listed.]

The term "terrestrial ecosystems" is particularly important for this manuscript, since it





Printer-friendly version

Discussion paper

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?

[We have added text to clearly define the terrestrial ecosystems covered in this review, indicating the differences from aquatic ecosystems. The definition of wetlands is used. We agreed that future expansion of review to cover aquatic ecosystems might be an interesting research effort, while it is not the focus for our current review.]

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 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 it 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





Printer-friendly version

Discussion paper

data inputs.

[We totally agree with the reviewer on the comment for the modeling purpose. Therefore, we added short paragraph to discuss model development and its association with the shift of models from mechanistic understanding to applicable model development. For the data requirements for parameterizing, and driving mechanistic models, we agree with the reviewer's comments, yet we believe more and more data will be generated and more insightful understandings are needed, which requires mechanistic models to fully understand the internal interaction and feedback between different processes.]

The issue of spatial data availability, used as model input, is also not discussed in the manuscript. It is my belief is 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.

[We agree that accurate data might result in more reliable model output, which would be important than model development. But model development remains a critical improvement we need to work on in order to reduce uncertainties in quantifying CH4 budget. In addition, model development will likely provide guidance for experimental design, which is the core of data-model integration.]

Specific comments: P2 L37. I strongly discourage use the concept of global warming potential when dis- cussing 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 mil-





Printer-friendly version

Discussion paper

lennia, 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 Biol.âĂĺP3 L70-72. This is a weak sentence to finish an introduction. P3 L74-76. Is it possible to reference the original sources?

[We have removed the term GWP and its usage for describing CH4 flux in the revision.]

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.

[We have rewritten this section; 1) we defined the primary CH4 processes; 2) we reorganized the detailed CH4 processes section. We organized them into two methanogenesis, two methanotropy and three transport processes. Each transport process is composed of one or more mechanisms. The CH4 transport is discussed at higher level of three transport mechanisms. See primary CH4 processes section.]

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

[We have removed the phrase as suggested.]

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

by wetland characteristics.

[Revised as suggested.]

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.

[We added explanations of acetoclastic and hydrogenotrophic processes.]

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

[We revised this sentence to emphasize that it is a range not exactly 100% for all upland. Yet we still kept ∼100% to make it consistent with other sentences. The percentage is used to help understand how much each single mechanism contribute to the total production, oxidation, or transport processes.]

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

[The sentence has been reorganized into two sentences. Meanwhile, we define the newly added terms diffusive and advective transports.]

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”.

[This sentence has been removed to reduce redundancies.]

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?

[In our manuscript, we have information that the direct and indirect impacts are based





Printer-friendly version

Discussion paper

on their associations with CH4 production, oxidation, or transport processes.]

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

[We changed it to freshwater sediment, to keep consistent with original publication.]

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.

[We have significantly shortened the paragraph by removing more than half of the listed models. And this section has been merged with the following section as suggested.]

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

[We added text to define the regional simulation capability. The models are defined with regional simulation capability if models directly read in and produce spatial maps.]

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

[We added one citation to support our statement of strong control of substrate on methanogenesis.]

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.

[We have revised the manuscript to clearly describe how we separated models based on model representation of CH4 processes. The section describing groups for methanogenesis has been reorganized as model algorithms.]

P10 L264. Seems appropriate to discuss substrate limitation and Michelis-Menten

dynamics of methanogenesis in the section on methanogenesis rather than methanotrophy.

[We have separated that section and moved the discussion of Michelis-Menten function into methanogenesis section, while keeping the discussion on methanotrophy in original section as appropriate.]

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”?

[We have revised this sentence for the purpose of improved clarity.]

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

[We added one sentence to show that the Arctic tundra ecosystem is an important contributor to global CH4 budget but long-term datasets of CH4 flux are lacking.]

âĂĺP20, L31. Sentence structure: “integration between model development and data collection is much stronger for advancing science”, do you mean that integration is important for advancing our scientific understanding of methane dynamics?

[We agree with reviewer. Yet we would like to keep sentence as it was because that sentence is used for general scientific studies. Meanwhile, we added a detailed description of data-model integration for CH4 cycling in the following sentences.]

---

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-37, 2016.





Printer-friendly version

Discussion paper