Reply to Reviewer 2 (Guy Schugers)
Marius Moser, Lara Kaiser, Victor Brovkin, and Christian Beer

Marius Moser et al., "Process-based modeling of the CO_2:CH_4 production ratio is important for predicting future Arctic methane emissions"

The manuscript by Marius Moser and coauthors provides a review of model representations of methanogenesis (CH_4 production) in site-scale models and land surface schemes. It highlights the pathways of CH_4 production and their difference in production of CO_2 and CH_4, and it highlights the importance of capturing the ratio between these two compounds for accurate assessments of climate impacts, with a focus on the Arctic.

The manuscript provides a good overview of the literature on this subject and is well-written, and the overview provided and comparison of model implementations is of interest to the modelling community. After some modifications, I would like to recommend it for publication in Biogeosciences.

However, this does not necessarily mean that I agree with the proposed strategy of refining models with this information; I see major hurdles in the scaling of information that is primarily derived at laboratory scale to models that work at field scale or even grid cells of tens of kilometers. I would like to suggest the authors to discuss this challenge in greater depth (see my comments on section 5 below). I provide further suggestions and comments, hoping that these can help to strengthen the structuring and the impact of the paper even further.

We thank you for taking the time to carefully read and review our manuscript and for providing comments that will help to improve our paper. We will reply to each respective comment underneath. The reviewer's comments are written in black font while the author's response is in blue.

Major recommendations:

I would recommend to bring the introduction of the different pathways (which now starts at l. 117) forward in the text. You bring up hydrogenotrophic and acetoclastic methanogenesis already in the first paragraph of section 2. Because the pathways are so fundamental for understanding your argumentation, I would suggest to start with an explanation of those in section 2. The pathways could probably be illustrated in a simplified way, e.g. as done in Fig. 1.

Thank you for the good point, this indeed makes sense and we moved the introduction of the pathways to the first paragraph of section 2. A simplified illustration akin to Fig. 1 would probably be unnecessary though, since it would look very similar. We could refer to Fig. 1 at that point.

In section 4, the distinction between LSMs and process-based models in the paper seems somewhat arbitrary - e.g., I would group some of the models in this section also under LSMs (e.g. ISBA-LSM). Maybe it would be better to distinguish levels of complexity in different models, or application to site/point studies vs. application to regional (gridded) simulations.

It is true that the line gets blurred in a lot of cases and we think your suggestion of more clearly distinguishing levels of complexity (of the methanogenesis) and area of application is good. We will specify that the models presented in section 4 feature a process-based approach to methanogenesis in particular and that they were developed for small-scale application, in contrast to the global LSMs discussed in the previous section.

We will change the title of Section 4 to “State of process-based models of methanogenesis at local scale applications” and add this to the opening paragraph for clarification:
“The process-based methane models discussed in this section include both standalone models and methane-focused modules developed for larger models, such as LSMs. In contrast to the previously discussed LSMs, which are being used in global simulations, often as part of ESMs, the models in this section were developed for local applications, with an explicit focus on methane processes.”

In section 5, I think that the authors could do a better effort to bring the full complexity of the system into play. While the CO_2:CH_4 production ratios in methanogenesis are well represented by the two pathways that are discussed in detail in the study, the CO_2:CH_4 ratios measured in the field are a combination of emissions from methanogenesis as well as emissions from other processes (e.g. CO_2 fluxes from heterotrophic respiration under aerobic conditions), some of which may dominate over the methanogenesis fluxes. The cited papers from Galera et al. (2023) and Schuur et al. (2022) highlight this (and Galera et al. (2023) argues in fact that incubations provide limited information on in situ conditions). It would be worthwhile to enhance the discussion on how to obtain a useful parameterization of these processes at large scales, and on the availability of relevant data for parameterizing and evaluating models (or maybe the authors could provide suggestions for relevant measurements to be undertaken to constrain such models). The discussion of CO_2:CH_4 ratios from methanogenesis and of CO_2:CH_4 ratios as measured in the field (and hence originating from multiple sources) should be disentangled more in section 5.

This distinction is indeed important and we will make this clearer in the section 5 discussion. We will also add an opening paragraph to section 5 that puts the methanogenesis processes into context with the system at large, including a clearer distinction of CO_2:CH_4 emission ratios and CO_2:CH_4 production ratios. This paper intends to focus on the methanogenesis part, i.e., the CO_2:CH_4 production ratio, and how it is represented in models. Discussing the entire complexity of the system is beyond the scope of this paper. The authors of course agree that the emission ratios in the field are the result of a multitude of processes, only one of which is the CO_2:CH_4 production ratio, and other processes like methanotrophy might locally be more influential for the final emissions. By restricting methane production to a fixed ratio factor tied to overall anaerobic decomposition, we nevertheless fail to represent the observed dynamic of methane production (Knoblauch et al., 2018) and this directly translates to uncertainty – even in this initial step.
Following the reviewer's suggestion, we will elaborate more on relevant data for parameterization and evaluation, or the lack thereof, and measurements that are needed to constrain the models.

Some additional suggestions:

- l. 59: The text seems to mix two impacts of vegetation on CH_4 fluxes: (1) The presence and abundance of aerenchyma affecting the transport, and (2) the provision of substrate for methanogenesis. I would recommend to disentangle these two processes a bit further in the text, because the latter is not related to transport (which is what the paragraph deals with), but with production (which is discussed in the paragraph above)

Thank you for the good suggestion, we separated these two aspects in the text and moved the part related to production to the previous paragraph on the topic.

- l. 144: Regarding the representation of CH_4 production in ESMs, I think it is important to note that, in contrast to CO_2 (i.e. in the C4MIP simulations), the CH_4 feedback is not part of the CMIP6 simulations. But I fully support the statement that including CH_4 production and its feedback to the climate system in ESMs would be desirable.

That is a good clarification to add, thank you. We changed the sentence to:
line: 144: (…) such as the ones partaking in the CMIP6 (Coupled Model Intercomparison Project Phase 6), though simulating the CH_4 feedback was not part of this project (Eyring et al., 2016)."

- l. 152: The two methods presented here are not mutually exclusive: the TOPMODEL approach provides a representation of horizontal heterogeneity, whereas the layering provides a representation of vertical heterogeneity. It is great to have both introduced here, but I would recommend not to present them as contrasts.

Thanks for the great point, we changed this part accordingly. We changed the line to:
"The former case is frequently realized via a TOPMODEL approach (Beven et al., 1979), which determines the inundated areas in a grid cell (Kleinen et al., 2020), thus representing horizontal heterogeneity while the latter method represents vertical heterogeneity. Although many models settle for one of the two methods, they are not mutually exclusive."

It is nice to have the most commonly used models presented (l. 158 and further). It would be nice if you could refer here explicitly to the two methods introduced in l. 152, to highlight which models adopt which of the two approaches.

Thank you for the suggestion, we added this aspect to the description of the presented models.

- l. 200: The authors focus her very much on permafrost-affected landscapes, and while the different environments will certainly play a role for the parameterization of the processes, I hope that the focus on the underlying processes, which is argued for in this study, allows (in principle) an application across different environments.

Yes, that is completely true. The application across different environments should be the goal, but, as you said, if a process-based model was developed for and evaluated against data from a specific environment, like rice paddy soils, this plays a role for parameterization. The emphasis on permafrost-affected landscapes here is meant to express how these types of soils also need to be considered in these kinds of models.
It is a good idea to explicitly state this goal in the text though, we changed the text here:

line 203: “Although process-based models should ideally be applicable across different environments, permafrost-affected soils exhibit unique properties and microbial structures (Miner et al., 2022; Beer et al., 2022; Song et al., 2021) that are only comparable to the aforementioned ecosystems to a limited degree.”

Minor remarks:

- l. 40-46: The paragraph lists a number of incubation studies with different CO_2:CH_4 ratios. How comparable are these incubation studies in their setup - would we expect similar ratios from all studies, or are the differences explained by differences in the experimental setup?

The studies listed here certainly differ in their setup – Galera et al. conducted in situ measurements of the emission ratio while Heslop et al. (2019) and Knoblauch et al. (2018) conducted incubation studies of different length – and we wouldn’t expect similar ratios as a result. These studies are mentioned here with the intent to show the wide spread of ratios found in each of the respective studies, not to necessarily to compare them with each other.

- l. 50: You mention aerenchyma here without explanation - but you provide a good explanation later (l. 55). I would recommend to either remove the term here, or bring the explanation from l. 55 forward to the first time it is mentioned.

Good point, we removed the term in line 50.

- l. 82: The reference to Yvon-Durocher et al. (2014) is given twice in the sentence; one of the two could be removed

True, the second reference was removed.

- l. 179: "Naturally, the model has ..." It is not clear from the text why this is natural - I trust it is related to the study setup?

Yes indeed, it was related to the study setup and the name of the model version being ORCHIDEE-PEAT.
We dropped the “Naturally,” for clarity.

- l. 190: Unclear what "proper" relates to here - "properly incorporated"?

“Proper” here was meant as it being incorporated into the ELM as part of the E3SM climate model, so that it would enable global simulations. The “proper” in the sentence was dropped for clarity and the sentence changed to:
Line 190: “(…) has yet to be incorporated into the ELM for global simulations as part of E3SM (Ricciuto et al., 2021).

- l. 229: "compliment" should read "complement"

Was changed to “complement”, thank you.