Reply to Reviewer 1 (Anonymous)
Marius Moser, Lara Kaiser, Victor Brovkin, and Christian Beer

The paper by Moser et al. is a review on the representation of methane production in process-based models. I think that in principle this is a decent overview that clearly addresses the overly simplistic way in which some models have traditionally modeled methanogenesis, while also proposing ways forward. Still, I have a number of comments for improvement and some thoughts to reflect upon.

We thank this reviewer for taking the time to carefully read our paper and to write this constructive review. The comments and thoughts provided helped to improve our manuscript. We will reply to your individual comments, with the author's response in blue underneath the respective comment.

First of all, there are many models discussed in this paper but what I'm missing is an overview table showing all LSMs and process-based methane models, and which processes are included in each. This would be really helpful to show which models are leading or still lacking. Similarly, when discussing equations, it would be good to show them. At the very least the basic Q_{10} and Arrhenius-type equations used by most models.

Thank you for the good suggestions, we will add an overview table, featuring all the discussed models, to the revised manuscript. The Q_{10} and Arrhenius equations will be explicitly shown in the text as well.

The table will look like this:

Models Overview			
Model	Methanogenesis	Temperature	Reference
JSBACH3.2	pre-set fraction, following (Riley et al., 2011)	Q_{10}	(Mauritsen et al., 2019; Kleinen et al., 2020)
JULES	scaling factor, pre-set fraction	Arrhenius	(Sellar et al., 2019; Chadburn et al., 2020; Clark et al., 2011)
JULES-microbe	methanogenic microbial biomass and activity, CO_2:CH_4 partition pre-set 1:1	Arrhenius	(Sellar et al., 2019; Chadburn et al., 2020; Clark et al., 2011)

Otherwise, given the paper's title and conclusion, I don't know why the paper decided to focus only on how the models represent methane production, because this is not the only uncertainty related to modeling Arctic methane emissions. Simply knowing where wetlands are located is perhaps one of the largest uncertainties in calculating Arctic methane emissions, as most recently shown by Ying et al. (2025). Correctly simulating snow cover is also very important to accurately simulate soil temperature and soil water content, which in turn affect soil biogeochemistry (Pongracz et al. 2021). Abrupt thaw processes are only mentioned once, even though they can completely transform landscapes and therefore strongly affect methanogenesis.

I am not suggesting that the authors include a complete overview of the uncertainties for spatial upscaling, but the title claims that the CO_2:CH_4 production ratio is important to predict future Arctic methane emissions. Is this true when compared to accurately simulating the environmental drivers that govern methane production and consumption? I agree that we need to include the right biogeochemical processes, but if the ecological and environmental boundary conditions are misrepresented in the model then the output will be wrong regardless. I hope the authors can reflect on this, because this paper mostly gives qualitative evidence to support the importance of the CO_2:CH_4 ratio. Perhaps add some text that delves deeper into how this relates to other types of uncertainty in modeling Arctic methane emissions.

The points raised in these two paragraphs are very important and the authors agree that for predicting methane emissions there are many other sources of uncertainty, most notably the ones discussed in the comment. In addition to improving the representation of methanogenesis processes, other processes like methanotrophy and transport processes need to be reliably represented to predict future Arctic methane emissions. We claim that how to represent methanogenesis processes in land surface models is among the most understudied parts. Obviously, boundary conditions are also very important as the reviewer states. However, even if we had e.g., a highly accurate wetland distribution, and a reliable snow module, the pre-set methane ratio factors, used in many models, would prevent us from capturing the observed dynamic of the CO_2:CH_4 production ratio and hence dynamics of methane production and emission.
We agree that the other uncertainty sources were understated in the initial version of the manuscript and, following the reviewer's suggestion, we will add more text in the discussion section to better portray this. A detailed discussion of all the processes and sources of uncertainty involved in the system is beyond the scope of this paper, however.

One more nit-pick about the title is that it should be specified that this paper focuses on terrestrial methane emissions, not other methane sources in the Arctic such as lakes, geological sources, wildfires, and rivers and streams (or the ocean, for that matter). Parmentier et al. (2024) showed that these other sources can contribute over 30% of the Arctic methane budget.

Thank you for pointing this out, we will adjust the title to reflect this by adding “terrestrial” and include the cited reference in the text.:
Line 67-69: “It is also worth noting that we focus on terrestrial emissions in this study. Other methane sources, e.g., wildfires, lakes, and marine and geological sources, also make up a significant part of the Arctic methane budget, potentially contributing over 30% to it (Parmentier et al., 2024).”

In addition, there are a couple of processes that are not discussed but might be important in the Arctic methane budget. In a region where the cold season lasts for most of the year, winter emissions can become quite significant (Zona et al. 2016; Treat et al. 2018), but this is not discussed in the paper. For example, burst-like emissions upon freeze-in are a physical process that locally can lead to very high emissions (Mastepanov et al. 2008). While not directly related to methanogenesis, this is not implemented in any model and also not discussed in the paper. It shows that transport is still highly uncertain, despite what’s claimed on line 66-67. See also Ito et al. (2023) for a nice overview of how the models currently represent cold season fluxes.

Thank you for discussing these processes. Cold season emissions and their representation in models are missing from this review and some text about them will be added to the revised manuscript together with the references cited.
As for the uncertainty of methane transport, we agree with this. However, the referred-to lines in the text were only meant to be seen as relative to the state of methanogenesis in LSMs which, in comparison, has arguably not seen as much development. In the introduction section of the revised version of the manuscript we will make more clear that this review is not about uncertainties in all processes governing the full methane budget but only focusing on methane production processes.

High-affinity methanogens are discussed briefly in the manuscript (line 338-341) but I think that these also warrant more discussion. The authors say that this is yet to be explored in most models but neglect to cite Oh et al. (2020) who simulated these methanotrophs and showed that the high latitude methane sink strongly increased as a result, thus reducing net emissions by ~5.5 Tg CH_4 yr^{-1}. These numbers are uncertain of course, but they stress the need to also focus on methanotrophy, not just methanogenesis. Btw, Oh et al. used a modified version of the Terrestrial Ecosystem Model (TEM), which I think is not mentioned in this paper despite a long history in modeling northern methane budgets (e.g. Zhuang et al. 2004; 2015), and one of the inspirations for the CLM(4Me) model mentioned in this paper. Might be useful to add.

As the focus of this review is on methanogenesis processes, we tried to keep the focus on the methanogenesis process in this paper so we kept this part about methanotrophs brief on purpose. However, we agree of course that methanotrophy is a very important process in its own right. We will add some further text to elaborate on this, including the reference to Oh et al. (2020), thanks.
The TEM is indeed an important model to mention and we will add this in the manuscript, thank you for pointing this out.

I know that the authors aim to focus on methanogenesis in particular, and I understand the wish for that focus, but the issues I mention above are also important to predict future Arctic methane emissions. Whether the CO_2:CH_4 production ratio is more important is unclear from this paper. For example, Sulman et al. (2022) did show an effect of Fe(III) reduction on these ratios, but the overall effect on emissions was relatively minor. Either a quantification of process importance or an uncertainty analysis would have been helpful to know whether including the extra complexity in the model, as suggested in Figure 2, really would lead to an important increase in model performance and reliably predict future methane emissions in the Arctic. If the authors are unable to better quantify this importance, then I suggest that the title and conclusions are adjusted accordingly.

Thank you for your comments. We indeed wanted this paper to be specifically focused on methanogenesis in order to highlight an often-overlooked aspect of methane emission modeling. Therefore, going into detail about all the other processes and sources of uncertainty in the system would be beyond the scope of this paper. But the reviewer raises an important point. As stated previously, we will include some further text in the discussion about the other processes/uncertainties that are discussed in the review to better contextualize the role of the CO_2:CH_4 production ratio.
As for quantifying this importance, we did not do this so far and also found little in the literature on this. However, even under controlled conditions, such as incubation experiments in the lab, the CO_2:CH_4 ratio varied between 0.2-0.8 (Knoblauch et al., 2018). This stands in stark contrast to the constant ratio factor, which is applied to the overall anaerobic decomposition and used in many LSMs, thus directly translating into great uncertainty of the methane production in the very first step, even before processes like transport and methanotrophy come into effect. To really estimate this uncertainty in relation to the uncertainties of other processes that govern the methane budget as a whole, we would need to apply a dynamic model that features the most important methanogenesis processes, which we discussed in the paper. We agree with the reviewer that such uncertainty analysis would be very important but for that we first need to represent the underlying processes. We will explain this need for a more process-based methane model able to predict the CO_2:CH_4 ratio in LSMs and the importance of quantifying the uncertainty in the revised manuscript and amend the conclusion accordingly. To reflect this adjustment, we will change the title of the paper to “The

role of process-based modeling of the CO_2:CH_4 production ratio in predicting future terrestrial Arctic methane emissions", as suggested by the reviewer.

Minor comments:

Line 42-44: are these both weight and molar ratios? Since C-CO_2:C-CH_4 and CO_2:CH_4 are both mentioned. I recommend converting these to the same unit for better comparison.

Galera et al. (2023) calculated their ratios on a molar basis while Heslop et al. (2019) calculated GHG production potentials from their incubations. Since the former measured in situ emission ratios and the latter production potential ratios, comparing these numbers is probably not that helpful and we only listed them here with the intention to show the wide spread of reported results in their respective studies.

Line 90: please show examples of a Q_{10} and Arrhenius-type equation in the text. Preferably with an example plot of how they differ.

See reply to previous comment, equations have been added to the text there based on the formulas shown in Xu et al. (2016).

$$f(T) = Q_{10}^{\frac{(T-T_{ref})}{10}}$$

$$f(T) = exp(\frac{\Delta E}{R}[\frac{1}{T_0} - \frac{1}{T}])$$

Line 144: very minor comment: maybe change "current" to "latest" (since it's been a couple of years)

Changed to "latest"

Line 161: which CMIP models use the scheme from CLM(4Me)?

MPI-ESM (JSBACH) in particular uses methane production based on CLM(4Me) while other CMIP models (CESM2, NorESM1-ME; see table in Zechlau et al. 2022) use versions of CLM for their land modules.
The wording of this line in the manuscript is inaccurate and the sentence has been reworked to:
Line 160-161: "(…) which prescribes the fraction of carbon released as methane from total anaerobic decomposition (Kleinen et al., 2021) – an approach based on the CLM(4Me) model by Riley et al. (2011).

Line 194-195: can you name these models here, and not just the references? Good to add to an overview table.

See reply to previous comment, an overview table will be added at this point in the manuscript, featuring the discussed models. The table will then be referenced here.

Line 229: which process-based model?

Tang et al. (2016) used the CLM-CN model by Thornton and Rosenbloom (2005) as a basis and extended it by incorporating processes from other models, such as biogeochemical reactions from Grant (1998) and Xu et al. (2015) and pH buffering from the WHAM model by Tipping (1994). See Tang et al. (2016) for further details.
They did not give a name to their new model version so we did not specify it in the text. We did change the line in the manuscript to:

Line 230-231: "Their model is an augmented version of the CLM-CN model (Thornton and Rosenbloom (2005) which has been expanded by incorporating additional biogeochemical process from, e.g., ecosys (Grant, 1998) and the model from Xu et al. (2015)."

Line 292-293: is this 40% of permafrost thaw emissions in the form of CO_2 or CH_4?

The number of 40% from Turetsky et al. (2020) is referring to general carbon loss (Pg C). Of these 40%, they found the share of CH_4 to be ~20%, which, however, translated into 50% of the added radiative forcing (Turetsky et al., 2020).

Line 320: typo: "Arcitc"

Typo corrected, thank you.

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