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Interactive comment on "Hysteretic temperature sensitivity of wetland CH_4 fluxes explained by substrate availability and microbial activity" *by* Kuang-Yu Chang et al.

Anonymous Referee #1

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The manuscript 'Hysteretic temperature sensitivity of wetland CH4 fluxes explained by substrate availability and microbial activity' by Chang and co-workers describes a modelling study in which the authors investigate the reasons for the differences in temperature sensitivity of methane emissions at the beginning and the end of the thawing season in two permafrost affected landscapes. They present observational data on this 'hysteretic temperature sensitivity' from one of the investigated sites (Stordalen Mire). However, to investigate the reasons for the observed temperature response they use data generated by their model. Based on the modeling results, the different temperature responses of methane emission during the thawing season is due to higher methanogen biomass and substrate production for methanogenesis in the later

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thaw season. This results in higher methane production and emissions at the same temperature in the later season compared to the early seasons.

The manuscript is concerned with a very important topic and there is no doubt that we need a better understanding of the factors regulating the different processes in the wetland CH4 cycle. This improved understanding has to inform models simulating methane emissions and their response to changes in environmental conditions. In this respect, the objective of the current study is highly relevant. On the other side, this study almost exclusively presents model-generated data on the regulation of methane emission in Stordalen Mire. The authors should make this clear and furthermore more critically evaluate the outcome of their model.

First of all, the model simulates a very low contribution of aerobic methane oxidation, which seems to be constant, irrespective of methane production (Fig. 3). The absence of methane oxidation makes the whole story much easier, since in this case, methane emission almost exclusively depend on methane production. However, several studies demonstrated the importance of methane oxidation in Stordalen myre (e.g. Perryman et al., (2020) or Singleton et al., (2018)) and numerous studies on other bogs and fens have shown the utmost importance of methane oxidation for methane emissions. The authors should comment on this, in particular since the unequal importance of methane production and methane oxidation during one thaw season may contribute to the 'hysteretic temperature sensitivity' of methane emissions observed. The bottom soil, where methane production takes place experiences in the early thaw season deeper temperatures than the surface soil, where aerobic processes like methane oxidation take place. In the late season, this pattern is reversed, since the soil starts freezing from the surface, which means that aerobic processes are earlier affected by freezing than anaerobic processes. Therefore, methane oxidizers and methane producers are exposed to different temperatures at the start and the end of the thaw season despite similar mean soil or air temperature. The very low contribution of methane oxidation in their model should be critically discussed on the background of the whole relevant literature and not only by considering the study supporting their findings. Furthermore, it would be interesting if the model is simulating a substantial contribution of methane oxidation at other sites, e.g. in Barrow.

A second critical point is the simulated extremely high concentration of substrates for methanogens. The simulated maximum acetate concentration is above the substrate concentration that is used to cultivate methanogens in the laboratory. Both simulated acetate and hydrogen concentrations are at least an order of magnitude above those concentrations measured in the presence of active methanogens and also much higher than concentrations that might enable fermenting organisms to gain energy by the production of these end-products. Previous investigations have shown an accumulation of substrates (but not to such high concentrations) if the consumers, in this case the methanogens, are inactive. In case of methanogenic activity, much lower concentrations are present to enable an energy gain for all organisms involved in the anaerobic food chain. Also in this case, the findings should be discussed on the background of available observations.

Furthermore, it is not clearly described in the manuscript, which observations are part of the manuscript. After reading the abstract, I expected observational and mechanistic modelling data from two sites (Strodalen and Barows) but the manuscript indeed presents and discusses almost exclusively model generated data on Stordalen. I suggest more clearly presenting, which kind of observational data are presented. As I understand, only Fig. 1 presents observational data to indicate that the 'hysteretic temperature sensitivity' is real and all the remaining data are generated by the model. I suggest either including more data and discussion on UtqiaÄąvik, or omitting this site. In the current manuscript latter site is only represented in three panels in Fig. 2.

To sum this up: The manuscript lacks in large parts of the discussion a critical evaluation of the model output, which should be discussed on the background of the available observational data.

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Specific comments:

L142 -144: The meaning of this sentence is unclear. Please clarify.

L 297: Hodgkins et al. (2014) gives no information on emissions, please revise

L305ff: The energy yield for methanogens indeed increases with rising substrate concentrations but the energy yield of fermenters decreases with rising end-product concentrations. Fermenters will most likely not be able to gain energy from fermentation at such high end-product concentrations. Please consider the whole anaerobic food web.

L329f: In L107 a fluctuating water table between the surface and -35 cm is given. Please clarify.

Cited references:

Hodgkins SB, Tfaily MM, McCalley CK et al. (2014) Changes in peat chemistry associated with permafrost thaw increase greenhouse gas production. Proceedings of the National Academy of Sciences of the United States of America, 111, 5819-5824.

Perryman CR, McCalley CK, Malhotra A et al. (2020) Thaw Transitions and Redox Conditions Drive Methane Oxidation in a Permafrost Peatland. 125, e2019JG005526.

Singleton CM, McCalley CK, Woodcroft BJ et al. (2018) Methanotrophy across a natural permafrost thaw environment. Isme Journal, 12, 2544-2558.

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