Author responses to:

Interactive comment on revised "Modeling anaerobic soil organic carbon decomposition in Arctic polygon tundra: insights into soil geochemical influences on carbon mineralization" by Jianqiu Zheng et al.

Referee #1 (30 Oct 2018 report)

I would like to thank the authors for their rigorous work on addressing the comments on the early version. The manuscript is now significantly improved and easy to read. I have a minor editorial and two major considerations that will hopefully further enhance the quality of the manuscript.

We appreciate the reviewer's comments, which have improved this manuscript.

Major concerns:
- Authors have nicely responded to my comment on how hydrolysis process is implemented in the model. I understand that there is lack of data to implement hydrolysis step, independently. reason, authors have merged this step with fermentation process by assuming that hydrolysis is the rate limiting step, supported by observations from rice paddy soil. I was wondering how assumption could be for tundra and organic soil, the relevant systems, for this study. This assumption may contribute significant uncertainty for model predictions, since it regulates storage rate of Carbon. For instance, having microbial uptake as the rate limiting step may lead to substantial discharge and accumulation of oligomers depending on hydrological processes. Overall, carbon turnover rate and GHG emission rates over time.

As the reviewer notes, data are limited comparing rates of anaerobic hydrolysis versus substrate uptake in tundra soils. If microbial uptake were limiting during incubations, we would expect the accumulation of substrates in soil pore water. In our previous paper (Z. Yang et al. / Soil Biology & Biochemistry 95 (2016) 202) we measured a rapid decrease in reducing sugar and ethanol concentrations in pore water, correlated with the production of CO_2, CH_4 and organic acid fermentation products. When we added glucose to the depleted samples, gas production rates increased quickly. We interpret this result as a limitation in carbohydrate hydrolysis. We elaborated on this point in the revised manuscript on pages 3-4.

- The absolute values for many parameters are still not provided. I suggest presenting a table that includes all the parameters with values used or fitted from the simulation.

We added a new supplementary Table S1 "Model parameter values for reactions A1-A5." Also, the full model code is now available online:

Zheng, J., Thornton, P., Painter, S., Gu, B., Wullschleger, S., and Graham, D. E.: Modeling Anaerobic Soil Organic Carbon Decomposition in Arctic Polygon Tundra: Insights into Soil Geochemical Influences on Carbon Mineralization: Modeling Archive, Accessed at https://doi.org/10.5440/1430703, 2018.

This citation has been added to the manuscript.

- Minor: There are still numerous typo and grammar errors. I suggest a careful reading of the English.

We carefully proofread the revised manuscript, incorporating numerous small corrections – particularly in the references.

Referee #2 (6 Nov. 2018 report)
This study examines an integrated model simulating CO2 and CH4 production in permafrost soils and combines a first order linear decay model with a reaction kinetic simulation to look at effects of pH and fermentation on CO2 and CH4 flux. Overall this rewrite is easier to follow then the original version and model development easier to follow.

We thank the reviewer for productive comments, which we used to clarify and enhance this manuscript.

The authors still have not addressed how the model was tuned and I feel the model documentation could still be improved. This is, essentially, a model development paper since there is no hypothesis that is address with competing models or simulations scenarios. As such I would strongly suggest a table of equations summarizing the various pools which are tracked in the model associated parameters with how their ranges were derived. Relatedly there are vague statements about model fitting (page 5 line 40 specifically) but no actual algorithms given to reproduce parameterization.

We clarified that model optimization was performed using the least squares method by fitting with the observed CO2 production on p. 5, line 34. A description of various carbon pools that were adopted from a previous version of the CLM model with the decomposition cascade structure and parameters is described in Supplementary material (p.5). Furthermore, the complete model code with implementation instructions, input and output files is now available online for perusal (see DOI above).

Ideally I would also like to see a comparison with previous simpler models to motivate the added model complexity. The authors try to get around this with the correlation analysis but there demonstration of model improvement to give a sense of what the gap in the model-data fit was closed by this new formulation.

To characterize the improvements added to this model we introduced new simulations shown in supplemental Figure S10. Starting with a reference model that lacks new modules for iron reduction or dynamic pH calculation, we computed baseline values for CO_2, CH_4, TOAC and WEOC production, as well as pH and f_{pH} values. The added complexity of iron reduction interacted closely with dynamic pH calculations to produce simulations that better represented observed gas production and pH changes in the modeled incubations. These results shown in Figure S10 are briefly discussed in the main text (page 11, lines 30-38).

In the end the idea of combining a first order linear decay model with a reaction kinetic approach is intriguing and I'm left wanting more out of this paper.