Biogeosciences Discuss.,
doi:10.5194/bg-2016-207-RC1, 2016


Biogeosciences
Discussions

# *Interactive comment on* "Biogeochemical model of $CO_2$ and $CH_4$ production in anoxic Arctic soil microcosms" *by* Guoping Tang et al.

**Anonymous Referee #1**

Received and published: 17 June 2016

Soil carbon models are a critical source of uncertainty in Earth system models both due to limitation in model-data and process representation. This manuscript addresses modeling hurdles in a key process (anaerobic decomposition) missing in most soil carbon models. This work is a timely, novel, and carefully conducted. However I am uncomfortable with the current introduction, justification, and implication presented in the manuscript. The actual results section is very strong but would suggest extensive revisions to the introduction and conclusion.

In general, I would suggest better connections between and within the introductory paragraphs. The main paragraphs jump around and paragraphs them selves lack coherent structure.

I would also like to see a discussion/acknowledgement of other mechanisms that could



influence anaerobic decomposition which were not captured by the proposed model. While not ALL processes can be included in a model, some acknowledgement of those missing processes and either how then can be incorporated into future work or how they influence current simulations are appropriate and highlight the limitations of the proposed model.

Relatedly the authors need to address how these detailed pool based kinetic models scale to well-structured heterogeneous soils. Great detail is gone into on the chemical processes governing methanogensis in the introduction but there is no discussion of how the physical structure of the soil plays into substrate and oxygen availability and the inherent limitations to applying mechanistic pool models to highly structured soil columns. While this is a common shortcoming of soil carbon models I feel that, given the level of processes detailed covered in the model, this is critical to address and justify utilization of a pool model with such explicit process representation. Minimum the authors need to acknowledge that the scaling of known kinetics from well-mixed experiments to highly structure soil cores is a relevant open question in the field.

The great strength of this manuscript is the highly detailed and careful analysis of the proposed model, grounded against a data set. This was very well done and I feel should provide the backbone of a new discussion section which could be extended to suggest potential follow up experiments based on the model results. However there is no formal model-data integration nor a comparison with an adequate number of data sets to justify this as a mature component of a new CLM module, as is implied by the current introduction and discussion. If this is the manuscript that the authors want to write then I would suggest more data sets, a formal data-model integration, and demonstration of improvement to previous ratio-based models. But the current analysis would be completely appropriate in a different context which focused more on implications to future experimental designs and long term model projections. I strongly encourage the authors to carefully consider an alternative framing of this very interesting study.

——Line by line response——

P2L2 Actually many of the IPCC models suggest that SOC will increase in the northern latitudes due to increases in inputs (Todd-Brown et al 2014), I would suggest softening this statement to reflect the huge uncertainty in the current state of the science. Less controversial would be a statement referring to a general ramping up of the entire carbon cycle in response to climate change, increases are expected in both primary production and decomposition. Whether the net effect will be to convert SOC to CO2/CH4 is highly debated.

P2L2-14 I like the content of this paragraph but it needs to be re-organized. There are three distinct topics in this paragraph which would be better served breaking them up/integrating with later parts of the manuscript: a review of expected high latitude SOC vulnerability to climate change, summary of CLM-CN representation of anaerobic conditions (coupling this with a general review of aerobic decomposition would not go amiss here but that is a soft suggestion), and the comparison to lab incubations.

P2L15 CH4 is critical not just because of it's high global warming potential but also because of it's emissions rate and residence time in the atmosphere. Please add some citations to reflect this.

P2L19 Why is this lag critical?

P2L25 Why is the CH4:CO2 ratio important? Maybe lead with this being a critical parameter for current models and then show how this is a dynamic response to the competing Redox ladder. I think this is where the authors are trying to go with this but it is lost in the paragraph. Could the proposed model be compared with the standard ratio model?

P2L33 Why is an aqueous phase essential for these calculations? Soil decomposition models are implicit descriptions of carbon dynamics anyway, why do we need an explicit representation of this process? Can a micro scale process like an explicit terminal electron acceptor model be simulated on the macro scale? I would suggest placing this study in the context of the increasing number of 'explicit' soil carbon dynamics models



(ex: Wieder et al 2015). These models may or may not increase the overall accuracy of predictive dynamics over a well-tuned traditional model but they can provide critical scientific insights into the process of soil decomposition. This introduction lacks this critical nuance and oversells the capabilities of the proposed model.

Sect2.2.1(and elsewhere) Please refer to model pools and other variables by name (variable) consistently throughout the manuscript, ex: organic acid pool (Ac). This reduces the need to refer back to tables/sections. Manuscripts are rarely read linearly and having to search for abbreviation definitions slows down reading.

P8L20 I applaud the authors for making their scripts available in the supplemental. Thank you.

P12 Nice job walking through potential drivers of model-data mismatch. These provide a rich pool of candidates for future investigations. I feel that this should be the main focus of the conclusions. Given that a single data set was used and no formal model-data integration done this model is not quite ready for a full land carbon model integration as is implied by the authors in the introduction and conclusions. What IS done quite elegantly is an analysis of several representations of potential mechanisms and how they influence overall carbon mineralization in the context of a common model structure.

P12 WEOC, TOTC are an unusual acronym in the field. Consider writing out the full name instead, I found myself forgetting what it stood for around here and having to go look it up. See previous comment about variable/pool references.

P13L19 This is a highly controversial statement that does not belong in the results section. While it is appropriate to highlight the relatively low amount of mineralized carbon there are several possible mechanisms for this that are unrelated to the chemical structure as suggested by this statement. Just because that is the explanation that fits into the model that is presented, it is not the only explanation (I'm thinking of various physical mechanisms like co-location and aggregate formation, as well as the substrate rarity argument ala Allson 2006). Please move to the conclusion and soften this statement considerably.

P14L17 Cite the equation reference for f(pH)

P14L26 Given the noise generally inherent in these measurements I would hesitate to call this a 'substantial' difference. Could you can provide error bars for the data or some kind of significance testing.

Figures: In general, would it be possible to add error/uncertainty bars to the data points in the figures? This would place the modeled sensitivity in the context of the measurement error.

P16L5-7 WHAM is an aqueous pool model, claiming that there is no needed modifications when applying it to a well structured soil column seems a bit of a stretch.

Table 2 Formatting needs to be fixed for the table entries and I would suggest replacing TOTC with Total Organic Carbon and WEOC with Water Extractable Organic Carbon. OC is a common enough abbr. that it could be used here without explanation but TOTC and WEOC are not.

Figure and Table captions: Figure and table captions need to be able to stand alone in the manuscript, people will often scan the figures to get a sense of the results of the manuscript. Please expand the figure captions to more fully reflect the conclusions being illustrated here, this is particularly needed for the supplemental figures.

====================

Todd-Brown, K. E. O., Randerson, J. T., Hopkins, F., Arora, V., Hajima, T., Jones, C., Shevliakova, E., Tjiputra, J., Volodin, E., Wu, T., Zhang, Q. and Allison, S. D.: Changes in soil organic carbon storage predicted by Earth system models during the 21st century, Biogeosciences, 11(8), 2341–2356, doi:10.5194/bg-11-2341-2014, 2014.

Wieder, W. R., Allison, S. D., Davidson, E. A., Georgiou, K., Hararuk, O., He, Y., Hop-



kins, F., Luo, Y., Smith, M. J., Sulman, B., Todd-Brown, K., Wang, Y.-P., Xia, J. and Xu, X.: Explicitly representing soil microbial processes in Earth system models, Global Biogeochem. Cycles, n/a–n/a, doi:10.1002/2015GB005188, 2015.

Steven D. Allison and Allison, S. D.: Brown Ground: A soil carbon analogue for the green world hypothesis?, Am. Nat., 167(5), 619–627, doi:10.1086/503443, 2006.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-207, 2016.