Biogeosciences

Discussions

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Interactive comment

Interactive comment on "**Biogeochemical model of CO$_2$ and CH$_4$ production in anoxic Arctic soil microcosms**" *by* **Guoping Tang et al.**

Anonymous Referee #2

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The manuscript proposes a new module to the CLM-CN. It attempts to address a key limitation of existing land surface models in that the anaerobic decomposition is poorly represented. This work is novel and quite thorough. Most of my comments are technical except that I am not 100% comfortable with the testing dataset. It appears that the incubation experiment did not flush the headspace of microcosms. (This manuscript and the original publication both neglected to discuss flushing). It is well known that the accumulation of CO2 and other trace gases in headspace distorts gas diffusion and greatly impacts decomposition. The suitability of the testing dataset is worth a thorough discussion.

I am very intrigued by this model, especially its addition of geochemical components. From an empirical perspective, I agree that redox dynamics and the turnover of pH and single substrates are all key in understanding anoxic decomposition. However, it is still

unclear whether adding these processes make sense in modeling decomposition and trace gases production. Personally, I would like to see a comparison between the new model and CLM4ME/CLM-CN and evaluate whether the new model offers meaningful improvement. Other data sources could also be used to calibrate the model. These and other future directions should be discussed in details in the conclusion.

Specific or technical comments Title, would it be better to say biogeochemical _modeling_ or _A_ biogeochemical model of CO2 and CH4 production?

When citing references, please add a space after each semicolon.

P3L2, there is a disconnect between the topic sentence and the following text. The topic sentence introduces 'simple substrates' and their importance in modeling CH4, while the following sentences switched the focus away from simple substrates. This paragraph can be reorganized to improve the flow of thought.

L8, lignin should not be classified as polysaccharides.

L14, acetate and H2 have been added _to models_?

When introducing soil pH and its role in LSMs, it is worth noting that soil pH changes rapidly as a result of redox reactions. For instance, iron(III) reduction consumes protons and usually increases soil pH.

L25, use pH buffering capacity instead of pH buffer capacity. It would also be useful to define 'pH response functions'.

L26, logarithmic

P4L4, the last sentence is rather weak and does not connect with the next paragraph. A better transition is needed to highlight why it is necessary to compare temperature response functions.

In 2.2.2, would it be possible to discuss how iron reduction and methanogenesis interact in the model? I can see that both of them reply on mD and kD, but what determines

the partitioning of electron donors between these two processes? These details would be valuable to interpret the results later on (P9L28-35).

P6L26, Riley et al. (2011) in fact cited Meng et al. (2012) Biogeosciences, 9, 2793–2819 for this specific pH function.

P7L8, the speciation of what?

P8L7, please justify why f(mega) and f(ferb) differ so much during initiation. A coma is missing between 0.5 and f(ferb). Also, I would keep the acronyms consistent, including their letter cases, throughout the manuscript.

L10, 'bioavailable ferric oxides' is a vague and potentially controversial term. Please provide a definition. Also justify why HCl-Fe(III) is used to represent bioavailable ferric oxides. These papers may be useful: Hyacinthe et al. 2006 Geochimica et Cosmochimica Acta 70: 4166-4180, Poulton and Canfield 2005 Chemical Geology 214: 209-221.

L26-27, I cannot follow this sentence.

L28, it appears that soil microcosms were not flushed after gas sampling. Without flushing headspace, CO_2 concentration builds up and consequently distorts gas diffusion. Without regular flushing, results from these incubations would misrepresent the decomposition processes in the field. Please comment.

P9L8, again, the inhibitory effect of high headspace CO_2 on microbial activities likely explained why the CO_2 level off in the microcosms. Are these results appropriate for calibrating models? Please comment.

L10, except _in_ the center organic soils.

L15, _so does_ microbial activity every year

L26, pH _increased_ with Fe(III) reduction or the _increase in pH_

Qualitatively, there is evidence to support that Fe(III) reduction competes with CH4 production. The center organic soils had the highest CH4 production, while their Fe(III) reduction was the lowest among all treatments. The authors claimed that "the impact appears less significant than expected", but I don't understand what they were expecting. Please elaborate.

P10L24-25, if microcosms in Roy Chowdhury et al. (2015) were not flushed regularly, then their results underestimated CO2 production. Thus, it is not surprising that the models appeared to overestimate CO2 production. Please comment.

P10L26, notice that Knoblauch et al. (2013) flushed microcosm headspace whenever CO2 concentration reached 3%.

P12L6, Figure 3S, adsorption of CO2 on iron oxides played a bigger role in the mineral soils than in the organic soils. Why?

L19, Table 2 should be referenced here instead of Table 1.

L20-23, I agree that higher f(labileDOC) increased model performance. But such improvement occurred mostly in samples kept at 8 degrees. For samples in -2 degrees, models with the lowest f(labileDOC) were actually the best. Please comment on the interactive effects of temperature and f(labileDOC) on CO2 production.

P14L10-11, soil redox condition can also explain why mineral soils have lower pH than organic soils. Soils in reducing environments usually have high pH because reduction reactions consume protons. With a much higher water content (Table 2), organic soils are in more reducing conditions and likely have higher pH than mineral soils.

Set a hanging indent for references.

Tables should be reformatted.