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Interactive comment on "CLM4-BeTR, a generic biogeochemical transport and reaction module for CLM4: model development, evaluation, and application" *by* J. Tang et al.

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We appreciate the comprehensive and positive comments from the reviewer. We address the specific comments below.

Specific Comments: *The model now explicitly tracks the movement of tracers in soils. However, the representation of processes, such as temperature dependence and soil moisture dependency on decomposition stem from parameterization where the assumption was made that production = flux. Therefore the incorporation of tracer transport itself calls for re-evaluation of such parameterization. This is certainly beyond the scope of the paper, but I feel it won't hurt if the authors address this challenge in their*

paper.

Response: We recognized this pitfall in the current parameterization of soil biogeochemistry models. We are designing a study to specifically address this issue, for which the results will be presented in our follow up studies. We have added a sentence to the Discussion section addressing this point.

Further, the abstract states that CLM4-BeTR was able to simulate soil-surface CO_2 effluxes and soil CO_2 profiles accurately. However, in most cases (daily and larger scales)-as stated by the authors-the fluxes are almost indistinguishable from production rate of the model. It is acceptable for this paper that CO_2 fluxes disagree quite a bit from the data, since the authors state clearly that there was no intent to specifically parameterize for the site. Yet, the difference in production vs. flux is much much smaller than the difference in measured vs. observed fluxes. Secondly, the authors point out that the profile concentrations are sensitive to boundary conditions (but not efflux) so it is thus not clear whether such a conclusion can be drawn. Therefore I am not sure if the presented results really demonstrate this ability. The authors' second conclusion where the authors state that the surface fluxes and productions are generally not equal needs to be more nuanced. Based on most of the figures, these two quantities seem to be equal in most cases presented (on a daily time step). The exceptions are specific cases of winter (generally) and freeze thaw cycles (specifically) and on a sub-daily time scale. My suggestion is to more clearly point to the subdaily variations, present that in a figure with a temporal evolution of fluxes throughout an average summer and average winter day (as opposed to just a historgram). Such results are also (as pointed out by the authors in the main text) very critically for eddy flux tower inferences for partitioning the CO_2 fluxes. The third conclusion was hardly touched in the main text.

Response: We reported in the manuscript that the discrepancy between belowground production and surface efflux for CO_2 is a function of many factors, including site characteristics, time of the specific day or season, and the size of temporal window to compare these two fluxes. For the illustrative application at the site Harvard forest, the CLM4 model we use did not simulate strong changes in hydrological conditions, in particular for the ratio of ice vs. liquid water in the topsoil. Therefore, the discrepancy mostly manifested at sub-daily time scales. We followed the reviewer's suggestion and revised Figure 8 such that our conclusion "the production is not generally equal to the efflux" is better supported. In addition, for high latitude regions in the Arctic, where alternative freeze-thaw events are frequent in topsoil, we found many more large discrepancies occurred. In these regions, episodic effluxes were instigated by a period of frozen topsoil followed by a topsoil thaw, leading to a surface CO_2 flux that was much larger than the below ground production even at the daily time scale. Another strong support for the conclusion "the production is not generally equal to the efflux" is from carbon isotope simulations, which will be shown in follow-up studies. Finally, the statement about the eddy flux partitioning is a general comment rather than a conclusion; we modified this sentence to emphasize this point. A detailed analysis on how to make use of the new CLM4 model structure to inform eddy flux partitioning is planned for future studies.

Generally, the authors should really highlight what now becomes possible with the new model, namely to predict level of tracers in soil and to predict actual fluxes out of the soil, which can much better be compared against measurements (flux towers, collection chambers placed on top of soils), and inferences of biogeochemical processes from within soil-layer measurements of products of biogeochemical cycles in the soil.

Response: We realize that there are many potential new applications with our new model. However, we also realize more work should be done to make these applications possible and done in a rigorous and consistent way. For example, a consistent evaluation of the eddy flux measurement would require the model to be extended for dynamic trace gas transport in the canopy and even to the above canopy atmosphere. In the revision, we discussed possible new applications at appropriate places, as well as necessary further developments that we are diligently working on.

P2713L19: I am not familiar with the Strang splitting approach. This might be that I

have some difficulties to understand eq. 8. It seems to me that each expression as separated by parenthesis describes one update for Dif, Adv and R. The way I read equation 8 is update Dif for half the time step, update Adv for 1/2 time step, update R for 1/2 timestep, etc. By this, it seems to me that the source R is only integrated over half of the time step?

Reply: There is a typo for the integration of source R. It should be integrated with a fulltime step. We corrected it in the revision.

P2722L13: No results of N_2, O_2, Ar N_2O and NO are shown. Unless these tracers have influence on CO_2 (which might be true for N species), there is in principle no reason to model these.

Reply: These gases are modeled together to track the gas pressure in the soil column, so that the CO_2 gas concentration can be directly compared with gas samples from empirical studies. Also, including those major gases are important for a reasonable modeling of ebullition. We checked the volume fraction of the major gases, i.e. N_2, O_2, and Ar, all of which are of reasonable magnitude. The contribution of NO is relatively minor.

Subsection 3.2: As mentioned above, analysis of sub-daily variation might be worthwhile at the expense of the exhaustive comparison of daily fluxes. Subsection 4.1: The fact that the time step 30 min is sufficient to produce reasonable model result for the two analytical cases is an important conclusion and can be incorporated into the abstract.

Reply: We revised the analysis and put the conclusion of 30 min time step into the abstract.

P2724L27: H2, over longer time scales (several years as presented here), heterotrophic respiration is likely driven by the amount of carbon throughput rather than oxygen and organic matter availability.

Reply: The oxygen concentration will be important when the soil moisture is high enough such that the anoxic environment will occur. If the occurrence of anoxia becomes sufficiently frequent, the heterotrophic respiration will be a strong a function of O_2. For the site we used for illustration, the amount of labile carbon seemed to be the dominant control of heterotrophic respiration.

P2725L1-3: The CO_2 loss through runoff being < 1 % of CO_2 produced finds its way in the abstract. This finding reported here therefore it would warrant a little bit more explanation. What is DIC concentration in runoff, how sensitive is this to the water transport itself, etc.?

Reply: We found the DIC concentration is often higher than a few thousand ppmv and could be up to 1 mol CO_2 m^{-3} water and even higher in the leaching flux to belowground water. However, for the site we modeled, the water outflow is small which leads to small DIC export. We are currently synthesizing available measurements to do a large-scale analysis of DIC export with CLM4-BeTR.

P2725L16: This paragraph is for me a very central part of this paper. It is the sub-daily scale where differences between production and fluxes become most apparent. The authors also make the important case for GPP calculations from eddy covariance calculations. Perhaps figure 3S should be considered to be added to the main paper.

Reply: We revised this discussion and added sub-daily analysis into the main text to better support our argument (see revised Figure.8).

Fig1: Please check color code, it seems to me red is nitrogen. Fig1: It is not celar what the boxes on the right hand side represent. Fig.2: It is not clear what Δz_a and Δz_b mean (also from the main text). Fig 4: Is the unit on the x-axis mol m^{-3}? Fig8: Since sub-daily fluctuations are differ most critically in term of production vs. flux, I would suggest to have a figure that demonstrate the temporal evolution of each in a 24 hour period (e.g. as JJA average and DJF average), while relegating 8a and 8b to supplementary material.

Reply: We revised the figures and corrected relevant typos.

To other comments: We carefully integrated the remaining comments into our revised paper.

Interactive comment on Geosci. Model Dev. Discuss., 5, 2705, 2012.