Geosci. Model Dev. Discuss., 5, C865–C869, 2012
www.geosci-model-dev-discuss.net/5/C865/2012/

Geoscientific
Model Development
Discussions

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

Anonymous Referee #1

Received and published: 5 November 2012

General Comments:

The paper "CLM4-BeTR a generic biogeochemical transport and reaction module for CLM4: model development, evaluation, and application" describes an implementation of a tracer framework into a land model. The motivation of this paper is that more and more constituents (CO2, nitrogen, methane, oxygen, etc.) are being incorporated in global model, and therefore there is a clear need to treat the reactions, phase changes, transports and exchanges of these tracers consistently within a land modeling framework. The framework is first tested against an analytical solution of tracer distribution in a soil column responding to a pulse and a periodic forcing. Secondly, the model then

demonstrates its capability of simulating CO2 effluxes in showing that, particularly on short time scales, storage and alternative pathways can lead to disparity between CO2 efflux and CO2 production belowground. The proposed framework is indeed an important step that guides land modelers away from single bucket soil models and present the right hooks to implement vegetation-soil tracer fluxes. The paper is nicely written and clearly addresses the need for such a framework and shows - with the application - also the advantages and opportunities of such a model. The technical description will be very useful for land modelers if they seek to add processes and tracers for biogeochemical application. Given that there are potential pitfalls and that the authors had to make assumptions for numerical implementation, I find it particularly laudable that the framework is (successfully) tested against analytical solutions. My minor objection is to some of the conclusions drawn from the application in the Harvard forest, which I will address in my 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.

Further, the abstract states that CLM4_BeTR was able to simulate soil-surface CO2 effluxes and soil CO2 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 CO2 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 CO2 fluxes. The third conclusion was hardly touched in the main text...

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.

Minor/technical remarks:

P2711L5: Do the authors mean non-steady state?

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 $\frac{1}{2}$ time step, update R for $\frac{1}{2}$ timestep, etc. By this, it seems to me that the source R is only integrated over half of the time step?

P2714L26: please explain z_a and z_b.

P2718L14: Is there a 1 sentence explanation that could describe the radiation boundary condition?

P2722L13: No results of N2, O2, Ar N2O and NO are shown. Unless these tracers have influence on CO2 (which might be true for N species), there is in principle no reason to model these.

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.

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.

P2725L1-3: The CO2 loss through runoff being < 1 % of CO2 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.?

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. 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 Delta z_a and Delta z_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.

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