Biogeosciences Discuss., 10, C1807–C1810, 2013
www.biogeosciences-discuss.net/10/C1807/2013/

Biogeosciences
Discussions
Open Access

Interactive comment on "The effect of vertically-resolved soil biogeochemistry and alternate soil C and N models on C dynamics of CLM4" *by* C. D. Koven et al.

C.D. Jones (Referee)

chris.d.jones@metoffice.gov.uk

Received and published: 12 May 2013

Review of Koven et al., "The effect of vertically-resolved soil biogeochemistry and alternate soil C and N models on C dynamics of CLM4"

This paper sets out to document the development and testing of improved soil C and N biogeochemistry in the CLM land surface model. It is a very clearly written manuscript and is an excellent example of developing, implementing, testing and evaluating different options for a model formulation. If only all -CN soil model development was performed so well!

As well as a very clear documentation of the developments, and reasons for them,

in CLM4, the manuscript examines some important scientific questions. In particular the study finds a need to introduce a parametrisation of reduced SOM turnover with depth, and discusses why this might be required. It is clear that such a parametrisation improves the fit of model simulations to data (with use of isotopic data being particularly important) but it is not yet known why. Far from being a failing, this is a good example of how detailed model testing and evaluation can highlight potentially important missing processes - future development may now address this issue and attempt to solve the reasons behind it.

I recommend this paper be published with minor revisions. A few specific comments and suggestions follow.

Chris Jones

- One motivation for trying the CENTURY scheme for C-cascade is to address the total amount of carbon stored, and you find that it improves (increases) this. But could you not simply adjust the turnover times in the original CLM scheme? If a soil-C model gives a factor 2 wrong storage for about right NPP inputs, then it can't just be the cascade scheme that's wrong - there must be a problem with the mean lifetime. There must be lots of tunable scope in such parameters as tau, especially for global application - is it really necessary to adjust the cascading of carbon rather than the lifetime of each pool? If you simply tune the turnover times in the CLM scheme you could easily get the right global amount - so is there a reason not to? e.g. does this break the site level evaluation?

- Having shown some initial maps of carbon content globally you don't discuss global quantities or use these to evaluate the schemes behaviour in its global, 20th century simulations. If you, say, adjust turnover times by +-20% you would immediately adjust your stores by the same fraction and probably the magnitude of your response to changes. The site level evaluation of amount and profile are extremely important, but getting a good match to global amounts and distribution is also important - I felt you

could have made more quantitiative use of your observational (or at least data-based) spatial fields.

- can you describe your experiment set-up in more detail? e.g. presumably it is land-surface only driven by obs meteorology and CO2. What about N deposition? what about land-use change? any other land management? I think you also need to discuss the inputs from the land-surface model INTO the soil components. At no stage do you show how well your litter inputs to the soil compare with obs - which are probably pretty sparse, but you can assume they are close to NPP on annual timescales and we do have some global fields/estimate of NPP. Likewise are you simulting or prescribing the vegetation PFT cover? or what about soil physical properties? does CLM do a realistic job of soil T and moisture? if the hydrology or soil physics is wrong then your rate modifiers will be and the soil BGC won't match the obs, even for a"perfect" -CN model. I think you should acknowlegde that the evaluation is complicated by not always being able to constrain where in the chain of processes errors creep in - it may not always be the soil BGC

- p.7207, line 16. You say all the rate modifiers are between 0-1. Is this true for temperature? how do you keep a Q10 between 0-1?

- eqn. 8. can you define "As"? Presumably related (but is it eactly the same as) atm C14 from figure 3?

- is it worth adding an Appendix, or supplementary info, with a more complete set of technical details of the model? such as how many vertical levels you use, do you have a list of new prognostic variables? It is not clear always in the process discussion whether some of the N-related variables in particular are state variables, or diagnostic. Any new ancillary data that are required - such as soil texture, depth etc? Plus maybe a list of data used in the evaluation - other modelling groups might like to be able to access these site level data for example

- can you check a few of the entries in the tables? e.g.: - p.7216, line 25 (should refer

to table 1?) - says the "A" factors collapse the turnover times onto that of the fastest pool. But "soil 3" has A=5, but there is more than a factor of 5 between soil 3 and soil 1 tau? - some of the table 2 numbers don't match figure 2 - e.g. rj for L2-S2 - Tji for CWD sums to 2?

- p.7224, lines 10-20. Can you describe how you spin-up your permafrost? in reality much of the carbon here accumulated in warmer climates which subsequently froze, so it's not possible to spin-up this pool under constant climate. Did you initialise from obs?

- p. 7225, line 12. Now your soil-CN model has the capability to affect vegetation productivity, which is not the case in -C only models, can you show then how vegetation carbon/storage/productivity etc are affected by the addition and developemnt of soil-N processes? Should this not also form part of the evaluation? The paper is about the role of this on carbon dynamics, but you only really show soil-C results.

Interactive comment on Biogeosciences Discuss., 10, 7201, 2013.