Responses to Comments from Reviewers

(Manuscript number: gmdd-5-C276-2012)

Dear Dr. David Lawrence:

Thanks very much for your giving us the opportunity to revise this manuscript by addressing all the reviewers' concerns. We also appreciate the two reviewers' insightful comments and suggestions. Below, we address the reviewers' comments and questions point-by-point. We have made changes in the text accordingly. The original reviewers' comments are italicized and our response to the reviewers' comments follow. The major points are listed as follows:

Generality of the method. Both reviewers have some concerns about the generality of applying the SAS method to all kinds of models, especially those have a complex vegetation submodel and nonlinear processes. As suggested by the reviewer #2 (C. Koven), an iterative approach could be useful for these models. We have added a paragraph to discuss the assumptions of the SAS method, and point out the necessary of using an iterative approach for those models with complex vegetation submodels and nonlinear processes (Line 458-478).

Definition of 'traditional spin-up'. Reviewer #2 (C. Koven) suggested us to better define the 'traditional' spin-up method. We have defined the 'traditional spin-up' in the Abstract (Line 29-30) and at the beginning of Introduction (Line 49-50) to make the comparison more clearly.

Reasons for the final spin-up. Reviewer #2 (C. Koven) have some concerns about the reason we use the final spin-up. There are mainly two reasons, one is our analytical solution is based on a linear mathematic system, but some processes are simulated nonlinearly in the model; the other reason is the temporal averages used for approximating those time-varying variables will yield errors to estimate the steady-state carbon and nitrogen pools. We have addressed them clearly in the method section (Line 176-189).

Thank you very much again for giving us the chance to revise this paper. If you have any questions, please let us know.

Sincerely,

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Reviewer #1 (R. Lardy)

General comments: This paper is a good contribution to enhance the accuracy of C allocation to soil pools and obtain a faster convergence thanks to a semi-analytical solution. Spin-up runs are by themselves an issue and the number of alternative method is very low, so it worth a publication, after some editing work.

Thank the reviewer for the positive comments.

However authors should consider in their conclusion that the method would not "work" on a model were NPP stabilization is only reached when soil organic matter is also stabilized. So for some model like Century (which contains a "very simple vegetation model") it would work, whereas for some model with more interactions like PaSim, it would not be very efficient.

The reviewer is right that the efficiency of our method may be low for some models with very complex vegetation submodels. It's because those models need a long time for NPP to reach steady state, which acts as the initial spin-up in the SAS method. For these models, an iteration of the analytical solution at the end of each recycle of meteorological forcing would be needed. In fact, such an iterative procedure has been successfully applied to the Pasim model (Lardy *et al*. 2011). We have added a paragraph in the Discussion (Line 462-482) and a sentence in the Conclusion (Line 561-562) to address this question.

Specific comments: P808 , line 7 : I guess it is more aboveground biomass than just woody biomass or leaves biomass is ignored?

Here we divided plant biomass, including both above- and below-ground biomass, into leaf, root and woody biomass. Hence, leaves biomass is not ignored.

P809, line 5: maybe it could be useful to precise that A represent C fluxes from respiration and transfers to another pool.

The reviewer is right that it is more precise if we use one matrix to represent carbon fluxes from respiration and transfers to another pool. In fact, in the first version of our matrix equation (see Equation 1 in Luo *et al*. 2003), *A* matrix represents carbon fluxes from respiration and transfers to another pool. In this study, *C* matrix represents carbon fluxes from decomposition of each pool, and *A* matrix represents carbon transfers from one pool to another. Separate these processes into *A* and *C* matrices make us very easy to find them out in the model.

P810, line 5: Why using C/N ratio of the end of the spin-up run for Uss, and not annual average values, as for Ä ˇA. Isn't there dynamics within a year?

The reviewer is right. We used the temporal average values of N/C ratios of the last loop of the initial spin-up. We made it clear in this revised version (Line 179-180, 299-300).

P812, line 13: it is not clear to me what is the needed time step of the forcing? If meteorological forcing is generated a 3 hour time step, then it is sub-daily and not daily data.

The reviewer is right. We have deleted the 'with a time step of 3 hours' in this version (Line 255).

P813, line 11. I guess stabilization is more accurate than steady-state. Cf. general comments.

We have replaced 'steady state' with 'stabilization' to describe NPP in the manuscript in this version.

P813, line 21. If I understood well, it might be useful to precise than mean changes are smaller than 0.01% per year compared to previous cycle.

We have revised the sentence according to the reviewer's suggestion (Line 292).

P814, line 6: The comparison between each year and not each cycle may be problematic for extreme year (e.g. 2003 for Europe). Indeed theses year will naturally induce high change in C pools. Is the passive pools stable enough in your model to support that?

Thanks the reviewer to point out the difference between year and cycle. We have replaced 'each simulation year' with 'each simulation cycle' in this version (Line 304).

P814, line 7-10: This is right that slower pool needs longer time to reach equilibrium, but due to initialization forced values, passive pool may be forced at steady state values whereas other pools are not. I would suggest to extend criteria to other pools. P815, line 18, same remark are previous one.

The reviewer is right that it is better to extend criteria to other pools. In this study, other pools reach steady state much earlier than the passive SOM pool. In the revised version, we have extended the steady-state criteria to any of the soil carbon pool (Line 303-304).

P817, line 16-17 Right, but comparison of line numbers is only relevant if it is the language (i.e. Fortran), which I am not sure?

We have revised the sentence and deleted the comparison of line numbers in this version (Line 404-405).

P820, line 20: This is still costly, I think SAS alone is not enough to reach such a goal.

Yes, SAS alone cannot reach that goal. We have changed the sentence and suggested that SAS could help reduce computational cost for ensemble analysis (Line 520-521).

Technical corrections : P810, line 6 and 12: it is not equation 3 but 4.

P810, line 18: the reference is just Kowalczyk (2006).

P814, line 1: It is equation 4 and not 3.

P818, line 18: it is the RothC model (not RathC)

P820, line 25 and 28; P821, line 4, equation 4 and not 3

P832, P833. In the legend, the unit is kg instead of kg/m^2

We have made the above corrections in the revised version.

Reference

Luo, Y. Q., White L. W., Canadell J. G., DeLucia E. H., Ellsworth D. S., Finzi A. C., Lichter J., and Schlesinger W. H.: Sustainability of terrestrial carbon sequestration: A case study in Duke Forest with inversion approach, Global Biogeochem. Cy., *17*(1), 1021, doi:1010.1029/2002GB001923, 2003.