

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 #2 (C. Koven)

Overall comments: This is a useful approach that ought to be considered more widely as a way of calculating equilibrium conditions for C and C-N models. I have concerns about the generality of the approach, in particular the role of feedbacks that some models may include in making the initial NPP estimate and allocation terms differ between what is calculated in the short spinup period 1 and the final equilibration period 3 from figure 2; this may make the approach better estimated as an iterative procedure to work in more complex models that may include more nonlinear interactions between C stock and NPP/allocation. In addition, more clarity on the comparison that is being made between the "traditional" and revised approaches would benefit the paper.

Thanks for the reviewer's positive comments. According to the reviewer's suggestions, we have added a paragraph to discuss the assumptions that are required by the SAS method (Line 458-478). We also have point out for those models with complex vegetation models and high non-linearity, an iterative approach of the SAS is necessary. 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.

Specific comments:

Abstract, line 14: Need to define what the traditional method that is being used as a point of comparison here. Is it the full model?

Thanks the reviewer's suggestion. We have define the traditional method when it first appears in the abstract (Line 29-30). The reviewer is right that the full model is used in this study.

page 805, Line 1; "Modeling ecosystem biogeochemical cycles is an initial value problem." I disagree, modeling ecosystem biogeochemical cycles is much more than an initial value problem! Perhaps better to just say that "Modeling ecosystem biogeochemical cycles is highly dependent on initial values because of long-term persistence of ecosystem state properties".

Thanks a lot for the reviewer's suggestion. We have revised the sentence as suggested by the reviewer (Line 44-45).

page 807, line 3: Not necessarily, in principle spinup could be achieved using a long-term model run that is non-repeating.

We have replaced 'all' with 'most' in the revised version (Line 102).

p. 807, line 9-14. This isn't clear. An with coupled C-N models is that the productivity of the ecosystem is tied to the slowest pool N stock by mineralization of N through decomposition of soil organic matter. So how do you estimate the NPP via a short initial spinup if you don't know what the N mineralization is?

The reviewer's is right that NPP is tied to the slow organic matter pools which supply N to plant growth via N mineralization. This process contributes to the great variation of NPP in the initial spin-up. NPP is tightly tied to soil available N, which is determined by N input (N fixation, deposition and mineralization) and release (plant N uptake, and N losses) processes. When NPP and plant pools approach to stabilization in the initial spin-up, plant N uptake also approaches to steady state. We agree with the reviewer that the decomposition of slow organic matter will change N mineralization even after NPP meets the steady-state criterion. However, because the turnover times of slow organic matters are too long, so the impacts of slow SOM decomposition on NPP through N mineralization is very small after NPP reaches steady state. Thus, we don't need to know the N mineralization in the initial spin-up. We have revised the sentence to make the logic flow more clear in this version (Line 110-112).

p. 809; lines 14-21: this assumes that the allocation parameters are independent of productivity. What if this is not the case; e.g. autotrophic respiration or allocation depend nonlinearly on the C stock in a given pool?

The reviewer is right that in our equations allocation parameters (B vector) are independent of productivity. However, in some models NPP allocation may depends nonlinearly on the C stock in a given pool, e.g., stop allocate to leaf if there is enough leaf C stock. In that case, the approximation error of using \bar{B} to represent NPP allocations will increase, and lead to an larger approximation error to estimate steady-state carbon and nitrogen pools with the analytical solution. This problem can be solved by the final spin-up which follows the analytical solution (Fig. 1). In this version, we have add several sentences to address this problem and made it clear (Line 180-187).

p. 810, line 9: It is not clear how this method avoids the issue that NPP is a function of N and therefore the NPP will be different between the initial step and the final step. Is this the reason why the final step is needed? Why not iterate through the loop until the model converges?

Thanks the reviewer's comments. For those models that NPP is a function of N, NPP will be different between the initial and final spin-up steps. It may lead to a longer time for the final

spin-up to adjust all variables to steady states. In this case, as suggested by the reviewer, an iterative approach is very useful. We have discussed this issue in Line 466-482.

There are mainly two reasons we need the final step. 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 180-187).

p. 814, lines 13 and 19. Can you replace loop units with years in this discussion? Also, what is the "traditional" spinup procedure used here? I think replacing traditional with a more informative description of what is being compared against (e.g. "full model"), throughout the paper would make the comparison more clear.

Thanks the reviewer's suggestions. We have used 'years' as unit to describe the spin-up time in this version (Line 318, 321). We also have defined 'traditional spinup' in the Abstract (Line 29-30) and the beginning of Introduction (Line 49-50).

p. 818-819. It would be useful to lay out exactly what assumptions are required for this method to work; i.e. can there be any nonlinearities within equation 1 and still have this work?

Thanks the reviewer's suggestions. We have point out the two important assumptions that are required for SAS in the discussion (Line 462-482).

p. 820, last paragraph. Analyses of equilibrium solutions to biogeochemical analyses using similar types of methods have been done, and it may be good to flesh out some of this discussion with references to the relevant papers, e.g. Bolker et al. (1998), "Linear analysis of soil decomposition: Insights from the century model".

The reviewer is right. We have cited Bolker's work in Line 86, 88, and 437. In this revised version, we have flesh out it in the discussion (Line 535-537).