Response to the Reviewer's Comments

We thank the reviewer for the constructive comments on our initially submitted manuscript. In the following, we provided clarifications to each comment and question from the reviewer. The comments by the reviewer are given in blue, our responses are in black.

Review of "Accelerating the spin-up of the coupled carbon and nitrogen cycle model in CLM4" for Geoscientific Model Development (GMD).

General Comments

The manuscript reviews challenges for spinning up global biogeochemical models, such as those coupled into Earth System Models (ESMs), in particular the Community Land Model 4 with Carbon-Nitrogen (CLM4CN). These models are typically initialized arbitrarily and then run to equilibrium as a surrogate for pre-industrial conditions. Because the equilibrium is dynamic (with repeated cycles of some representation of historical atmospheric forcing) and because of the nonlinear nature of the governing equations (in particular, the coupling between the carbon and nitrogen cycles in CLM4CN), an analytical solution to the spinup is difficult to achieve, and spinup simulations can be expensive, comprising the majority of the time required for generating results for a climate scenario with a particular model configuration. CLM4CN spinup has been addressed in previous manuscripts, as noted by the authors, but current methods leave room for improvement.

The authors investigate a Gradient Projection method for accelerating the spinup by extrapolating the change in slowly changing state variables over the course of one or more cycles of atmospheric forcing. They find that this approach can be used successfully to enhance the computational efficiency of spinup compared to previous approaches, especially when a strict criterion for "equilibrium" is used. As found by previous authors cited in the manuscript (e.g., Koven et al. 2013), the method works well, as CLM4 involves the coupling of processes ranging in timescale by many orders of magnitude: i.e., 30-min biogeophysical processes vs. accumulation of soil carbon pools with turnover times of hundreds of years. The authors also identify the non-convergence of CLM4CN under some conditions due to both representation of oscillatory physical processes (such as fire) and spurious numerical oscillation (due to the discretization and solution of the equations for soil moisture diffusion and interaction with groundwater). The authors are able to eliminate these oscillations when turning off the fire model or replacing the subsurface hydrology with a variably-saturated flow model with apparently better numerical properties.

As the manuscript addresses a challenge to climate modelers and presents clear and useful methods and results, I recommend it for publication in GMD. I would only recommend minor revisions in presentation to enhance readability and make the context clear to readers. The manuscript is currently well within the typical length of a GMD article, and some expanded explanation in some sections would improve the manuscript. Suggestions for doing so along with minor points of clarification are detailed below.

We thank the reviewer for the positive feedback.

Specific Comments

Introduction

1. Equations 1 & 2: as I understand it, I think these equations are missing factors for the fraction of carbon not respired to the atmosphere as they are transferred from faster to slower pools. Presumably this omission is only a problem for the presentation of these equations, as the actual numerical rate of change of carbon pools calculated from the model was apparently used later.

We thank the reviewer for the comment. Eqs (1) and (2) include the heterotrophic respiration fraction and the fraction of carbon that's not respired to the atmosphere. These two fractions add up to 1. When Figure 1 was presented, we added "Note that heterotrophic respiration fractions are not shown." We also added "The first term on the right hand side of Eqs. (1) and (2) includes heterotrophic respiration." after the equations.

2. The last paragraph of the introduction moves abruptly from the current problems with spinup to a brief mention of the new approach. I would here include some additional introduction about numerical methods for improving spinup, in particular the "Gradient Projection" approach used here: what problems is it applicable to, and are there similar applications in which it has been successfully applied previously?

We added the following in the revised manuscript:

In implicit time integration approaches, based on knowledge about the trajectory of the solution of the initial value problem, linear extrapolation from time integration was often used to find a good initial value for iterative multirate multidisciplinary processes [*Birken et al.*, 2014 and references therein]. A number of explicit Euler steps with small time steps followed by explicit Euler step with large time steps when the change in slow component due to fast processes become negligible has been shown to efficiently solve stiff ordinary differential equations [*Eriksson et al.*, 2003; *Gear and Kevrekidis*, 2003]. We made use of those concepts, referred to as the Gradient Projection approach in this study, to further improve the spinup.

Birken P., Gleim T., Kuhl D., and Meister A. (2014), Fast Solvers for Unsteady Thermal Fluid Structure Interaction, arXiv:1407.0893v1.

Eriksson, K., C. Johnson, and A. Logg (2003), Explicit time-stepping for stiff ODES, *Siam J Sci Comput*, *25*(4), 1142-1157, doi:Doi 10.1137/S1064827502409626.

Gear, C. W., and I. G. Kevrekidis (2003), Projective methods for stiff differential equations: Problems with gaps in their eigenvalue spectrum, *Siam J Sci Comput*, *24*(4), 1091-1106, doi:Pii S1064827501388157

Methods

3. p. 9113, l. 12: Expand "(carbon and nitrogen)" to include the bigeochemical processes, as provided for the list of biogeophysical processes. Some of this is included in the following text, but at least expand to "carbon and nitrogen cycling in vegetation and soils".

We replaced "(carbon and nitrogen)" with "(phenology, autotrophic respiration, heterotrophic respiration, carbon and nitrogen allocation, and nitrogen source/sink)".

4. p. 9114, l. 9-10: Please explain or cite the stability requirement noted. How was jp chosen?

We added the following in the revised manuscript:

Explicit or forward Euler method is used in CLM4-CN to solve the time-dependent ordinary differential equations with given arbitrary initial conditions. The explicit method can be numerically unstable (convergence of solution is not guaranteed) if the time step is too big [*LeVeque*, 2007]. For the first order kinetic type problem, i.e., $u'(t) = ku(t)$, the stability requirement is $|1 + kh| \leq 1$, in which k is the rate constant and h is the time step.

jp can be chosen as the time period needed to stabilize the components from fast processes after perturbation, or set as ~100 years.

LeVeque, R. J. (2007), *Finite Difference Methods for Ordinary and Partial Differential Equations: Steady-State and Time-Dependent Problems* Society for Industrial and Applied Mathematics, Philadelphia, PA.

5. p. 9116, l. 14-15: Please explain why mass conservation error occurs.

By moving water mass around after the Richards' equation is solved, Richards' equation at each node is no longer satisfied if its moisture deviates from its previous solution. We have confirmed the local mass conservation error of water in the original model of CLM4. We added the explanation in the revised manuscript.

6. Please add a sentence or two explaining and/or providing additional references for "using the integral finite difference approach and discretized temporally using first-order backward Euler differencing," for improved readability. ("Backward Euler differencing" is equivalent to an implicit-timestepping solution, right?) Please also mention or cite the theory for why this method is numerically stable and avoids the oscillations associated with the current CLM4 hydrology.

Yes, backward Euler differencing is equivalent to an implicit time stepping solution.

In STOMP, the water mass conservation equation equates the time rate of change of water mass within a control volume with the flux of water mass crossing the control volume surface. The mass conservation equation is discretized following the integrated finite difference approach of Patankar [1980], which is locally and globally mass conserving. Backward Euler differencing or implicit time stepping is suitable for the solution of the equation that is numerically unstable [*LeVeque*, 2007].

LeVeque, R. J. (2007), *Finite Difference Methods for Ordinary and Partial Differential Equations: Steady-State and Time-Dependent Problems* Society for Industrial and Applied Mathematics, Philadelphia, PA.

Patankar, S. V. (1980), *Numerical Heat Transfer and Fluid Flow* , Hemisphere Publishing Corporation, Washington, D.C.

The above sentences and reference have been added in the revised manuscript.

Results

7. p. 9117, l. 8: Do the authors have any explanation for why the results are wetter and cooler with the ostensibly improved numerical scheme?

We added the following explanation:
Using STOMP, mass conservation is improved, and the moisture content calculated is more accurate, resulting in a wetter and cooler soil.

8. p. 9117, l. 15: Please clarify that "the model" here means the Gradient Projection.

Yes, the model means the Gradient Projection. We made the clarification in the revised manuscript.

Conclusions

9. p. 9118, l. 3: The authors claim the results are "more accurate" with the new hydrology submodel, but this phrasing implies some improved comparison to observations. It seems the authors wish to argue that the better theoretical grounding or numerical properties of this model make it superior, and that the oscillations noted in the existing hydrology are not a correct behavior given the governing equations. If this is the case, this argument should be made in the Results or a Discussion section and referenced in the Conclusions.

We agree with the reviewer that the accuracy was improved because of a superior numerical scheme. It has been clarified in the revised manuscript. The argument is made in the Results section as suggested by the reviewer.

10. p. 9118, l. 4: Likewise, in noting that "more C [is] predicted," the tone implies that this is an advantage, but they do not reference observations to show that it is more realistic. In fact, CLM4CN does have too low soil carbon, but other factors besides the hydrology may also contribute to this bias, such as nominal turnover times shorter than observed (i.e., Koven et al 2013). Please rephrase so that it is clear that the prediction of more carbon is not suggested to be an advantage but merely a result, as there may be compensating errors in the model formulation.

The reviewer is right that the conclusion was based on comparison between two models, with no comparison to observations to suggest which solution is more accurate. Some discussion has been added in the revised manuscript.

11. The authors may have an opportunity here to comment in a Discussion section on the context of their procedure and future work. Is an equilibrium spinup approach appropriate? What properties should this model satisfy ideally for such a procedure to be used (e.g., convergence to a unique solution independent of starting conditions or acceleration procedure)? Are there other methods that could be applied to improve spinup further that might require more significant modification of the model structure?

We added the following in the Conclusion section:

No matter what modification is made to improve the speedup efficiency, a final spinup is always needed to reach a converged solution due to disequilibrium caused by the modification. This approach is especially useful when new model formulation is proposed and high quality solution (small convergence threshold) is needed for a fair comparison.