**Response to Dr. Luo and Manoj's Comments**

We thank Dr. Luo and Manoj for their detailed and constructive comments on our original manuscript. In the following, we provided clarifications to each of their comments and questions. In this response, Dr. Luo and Manoj's comments are given in blue, our responses are in black.

The manuscript describes a method to accelerate the spin-up of biogeochemical models based on gradient projection method, which was applied to the slow turnover soil carbon pools in CLM4 model. The authors claimed that their method "can reduce the computation time by 20–69% compared to the fastest approach in the literature". They also showed that their method did not work in three specific sites and the cyclic instability of carbon cycle in two of the three sites was resolved after replacing hydrology scheme in CLM4 with STOMP.

The manuscript is well-written, easy to read, and falls within the scope of the journal. The method is straightforward and should, in principle, work for this monotonic carbon accumulation system during spin-up.

However, there are some areas that need further explanation.

1. Their claim that their method "can reduce the computation time by 20–69% compared to the fastest approach in the literature" is not well grounded. They only compared their method to the AD method. The latter is not the fastest approach in the literature. The semi-analytic method is probably the fastest one published in the literature, which the authors did not at least compare with.

We agree that the reduction in computation time using our method was compared only to the modified AD method. We had previously restructured CLM4-CN and developed a steady-state solution directly using annually averaged rate parameters [*Fang et al.*, 2013; *Fang et al*. 2014]. Using our approach, we were able to implement the semi-analytical method in Xia et al. [2012]. As we mentioned in the manuscript, the semi-analytical method needs initial spin-up values of net primary productivity (NPP), which still requires long simulation time for stabilization because CN are tightly coupled in CLM4-CN. Besides, a final spinup is needed after the analytical solution. Our numerical experiment showed that the semi-analytical method is not necessarily the fastest. We added the above discussion in the revised manuscript and reworded the "fastest" to "one of the fastest" to include the possibility of other existing schemes that are faster.

Fang, Y., M. Huang, C. Liu, H. Li, and R. Leung (2013), A generic biogeochemical module for Earth system models: Next Generation BioGeoChemical Module (NGBGC), version 1.0, *Geoscientific Model Development*(6), 1977-1988.

Fang, Y., C. Liu, M. Huang, H. Li, and R. Leung (2014), Steady state estimation of soil organic carbon using satellite-derived canopy leaf area index, Journal of Advances in Modeling Earth Systems.
DOI: 10.1002/2014MS000331

2. The oscillation at US-IB1 and periodicity at US-SO2 are due to fast turnover (short residence time), with which total soil C dynamics are mainly determined by external forcing. The pool sizes (total amount of soil carbon content) is only at scales of 2-5 kgC m-2 at the two sites. NPP at those two sites is probably around 1 kgC m-2, leading to residence times of 2-5 years. When residence time is short, the soil C varies with environmental forcing (see the second paragraph on page 6 of Yiqi Luo, Trevor F. Keenan, Matthew Smith. 2014. Predictability of the terrestrial carbon cycle. Global Change Biology, doi: 10.1111/gcb.12766.) The oscillation and periodicity has nothing to do with the hydrological model of CLM but can be solved by having longer residence times (or reducing transfer coefficients). Thus the section from ling 21 of page 7 to line 18 of page 9 is unnecessary.

Thank you for the comment. We'd like to clarify that the issue of oscillation that we are trying to address refers to the oscillation of the annual average solution from one full length (multiple years) of forcing cycle to the next. For example, if the length of the forcing cycle is 3 years, the annual average solution of year one should be close to that at year four, which is driven by the same year one forcing. Within each forcing cycle, soil C varies with environmental forcing as shown in your reference. Note that Figures 2, 4 and 7 show the annual average total C, and the oscillations in Figure 4b and 4c correspond to fluctuations from one forcing cycle to the next rather than within the forcing cycle, which last 3 and 9 years for US-IB1 and US-SO2, respectively. The apparent fast turnover at US-SO2 was due to the long annual fire disturbance. Following your argument, soil C dynamics at US-SO2 should be determined mainly by the external forcing, which varies from year to year within the forcing cycle. However, this argument cannot be used to explain the oscillation of the annual average solution from one forcing cycle to the next. We don't rule out the possibility that the oscillation may be caused by factors other than the hydrological model. However, we demonstrated the non-conservation problem with the hydrological model used in CLM4 and were able to resolve the oscillation problem with a better hydrological model.

We made the clarification and added your reference to differentiate the oscillation from the daily, seasonal and interannual variability in the revised manuscript.

3. In section 2.2, page #5, it is better to write the equation of spin-up time as years, otherwise reader may miscalculate the spin-up time.

Because the number of years in a cycle of atmospheric forcing is different at each site, we are not able to give a specific number to replace the equation.

4. Since the main basis of the study is based on the extrapolation of the carbon at a future time tn, it is important that the value of the gradient of the carbon cycle between times t0, t1 and tn does not change considerably. Hence the value of mc chosen becomes critical for the gradient projection method to work. For example in Fig. 2a, consider that a user choses mc =12. Based on

the ks4 value in Fig. 1, the turnover year, t_27 years. According to the author, tn -t1 = t mc _324 years, but we can see that the gradient changes slightly when time > 300 years in Fig. 2a. The extrapolation may produce more extreme result depending upon the change in gradient in different cases. Hence it becomes crucial that the user choses appropriate value of mc but the author does not provide any information or suggestions on how to pick the value of mc.

As mentioned in the original manuscript, mc is the number of years of known atmospheric forcing. It is a given number. The future time step $\Delta t$ is chosen so that the solution of slow processes won't diverge or the solution is stable. In the case of soil4C, the future time step $\Delta t$ is chosen such that $\Delta t/\tau < 2$, where $\tau$ is turn over years of soil4C. We picked $\Delta t = \tau$ in the manuscript. A stop point of ~300 years for the modified AD approach was selected based on the results in *Koven et al.* (2013), but it is not required. The best approach is to stop when NPP reaches dynamic steady-state. After each execution of the Gradient Projection (GP) approach, we gave it about 100 years in a sort of prediction/correction for the system to stabilize due to perturbation of the components from fast processes. The explicit integration approach using a number of small time steps followed by a large time step when the change in slow components due to fast processes become negligible has been successfully used to solve stiff ordinary differential equations [*Eriksson et al.*, 2003; *Gear and Kevrekidis*, 2003]. From our experiment, as long as there is no oscillation in the trajectory of time integration between forcing cycles, the GP approach works fine.

We added a brief discussion of how we pick the initial spinup and how long a simulation is needed after each projection in the revised manuscript.

Koven, C. D., Riley, W. J., Subin, Z. M., Tang, J. Y., Torn, M. S., Collins, W. D., Bonan, G. B., Lawrence, D. M., and Swenson, S. C.: The effect of vertically resolved soil biogeochemistry and alternate soil C and N models on C dynamics of CLM4, Biogeosciences, 10, 7109–7131, doi:10.5194/bg-10-7109-2013, 2013.

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

Specific Comments Minor comments that have been marked in the pdf manuscript.
Note that Manoj is a post-doc in Yiqi Luo's group.

The following are responses to the specific comments made in the manuscript.

We corrected most of the editorial errors pointed out by Manoj in the revised manuscript, such as lower case letter for turnover rates, definition of first abbreviations etc.

p.5, l.24: why 300 here?

~300 years as a stop point for the modified AD approach was selected based on the results in *Koven et al.* (2013), but it is not required. The best approach is to stop when NPP reaches dynamic steady-state.

p.6, l.5: Since the method is based on the gradient or slope of two consecutive cycles of carbon, it seems that the method may fail when the gradient between the cycles changes. Like in fig 2a it can be that the gradient changes at least 2 or 3 times. How does the author suggest to deal with those changes?

This approach is analogous to using a large time step that satisfies stability requirement to integrate the slowest processes once the contributions from fast processes become negligible (e.g. after 100 years of small time step integration). After each execution of the Gradient Projection approach, we gave it about 100 years for the system to stabilize or damp the components from fast processes to offset the error caused at the projection step.

p.6, l.11: why 100?

With small time steps, we use 100 year simulation that is long enough to correct perturbation caused by the projection. It doesn't have to be 100 years. It can be the time period needed to stabilize the components from fast processes. We added the statement in the revised manuscript.

p.7, l.13 and l.14: reduction compared to what?

The reduction was compared to the modified AD approach. We made it clear in the revised manuscript.

p.7, l.18: (regarding oscillation) why? may occur with strong interannual variability in forcing and short residence times in carbon pools.

The oscillation we referred to is the fluctuation of the annual average total carbon between each full length of forcing cycle rather than interannual variability within the forcing cycle. We made the clarification in the revised manuscript.

p.7, l. 19: (regarding longer periodicity than atmospheric forcing for US-SO2) why, only occurs for pools with very short residence times?

Site US-SO2 has long annual fire disturbance (> half year).

p.7, l.23: does the model have fire simulations?

CLM4-CN can simulate fire effects based on a statistical fire model.

p.7,l.24-27: forcing will be reflected in oscillation only if the pool residence time is very short.

This comment is related to what we mean by oscillation in the manuscript. The oscillation we referred to is the fluctuation of the annual average total carbon between each full length of forcing cycle. Hence it is not a reflection of the forcing variability within each forcing cycle, and it cannot be explained by the short residence time of the C pool.

p.8, l.3: you need to obtain steady-state water table depth in order to get the spin-up results for carbon

Agree. That's why we investigated STOMP when we found that the water table oscillates from one forcing cycle to the next cycle in the CLM4 formulation.

p.8, l.13: (mass conservation) for water or carbon? should be water only.

Yes, it is water only. We added "water" in the statement in the revised manuscript.

p.8, l.14-16: "The water content formulation itself has been previously shown to cause solution instability for soils near saturation (Hills et al., 1989)." may not relevant to carbon cycle spin-up

We intended to point out the inherent issue of the water content based formulation regarding oscillations.

p.8,l.17: (comment on the new flow model investigation) its root is in carbon cycle model

We believe this comment is again related to the clarification of oscillation used in the manuscript. We were able to use a better hydrological model to resolve the issue.

p.8, l.20: (STOMP description) this paragraph may not be necessary

We think a brief introduction is necessary for the readers.

p.9, l.13: it should be separated from carbon cycle

It's possible that when the residence time of soil carbon is long enough, it can damp out the role of water table oscillation. However, the true residence time at certain location could be long or short, as governed by the underlying processes. For locations with short residence time, we still need to resolve the issue that may arise from other aspects such as the hydrological model.

p.10,l.3-5: (regarding more carbon predicted and less uncertainty if correct numerical scheme is used) Is it always true? you changed ware scalars so that you predict more soil carbon.

There are other uncertainties too. Yes, the conclusion was based on model observation. We made it clear in the revised manuscript.