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Interactive comment on "Accelerating the spin-up of the coupled carbon and nitrogen cycle model in CLM4" *by* Y. Fang et al.

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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.

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.

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.

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

GMDD
7, C3245–C3247, 2015

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.

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

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C3245/2015/gmdd-7-C3245-2015-supplement.pdf

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