Comments to Alexandrov GA, Explaining the seasonal cycle of the globally averaged CO_2 with a carbon cycle model

The attempt in this study to solve the discrepancy between simulated and observed globally seasonality of atmospheric CO2 concentration is important for a better understanding of future climate-carbon cycle feedbacks in the earth system. The author used a very simple approach to study this issue and attributed the major reason to the representation of quickly decaying fractions of litter in current models. Although the theoretical approach is novel for the global carbon cycle study, some aspects about terrestrial carbon cycle were not clear or fully considered in the current study:

(1) The paper suggested a 70% partitioning coefficient of quickly decaying fractions in the total litterfall will greatly improve the seasonality of the net CO2 exchange between the atmosphere and the terrestrial ecosystems. The 70% itself might be very high for many terrestrial ecosystems, especially those with high production of woody biomass. Although the author mentioned (P71 Line 28) that the value is not impossible, it's better to have some empirical evidence to show the reasonable range of the partitioning between fast and slow litter fractions in the real world.

(2) Another question about the partitioning coefficients for quickly ($R_{h,q}$) and slowly ($R_{h,s}$) decaying litter pools is that whether the partitioning coefficients keep constant during the whole year. The 70% share of quickly decaying litter might be ok during the summer time when leaf and fine root growth reach their peaks. However, more litter would stay as slowly decaying fraction during the winter seasons with less or no leaf/fine root activities. As shown in Fig 4, the approach work well during Jun-Aug, but not good enough during winter months.

(3) Can the approach in this study capture the enhanced seasonality of CO2 exchange in the Northern Hemisphere(Graven et al. 2013)? It would be great if the author can show the advantage of his approach in capturing the temporal trends in the seasonality of global CO2 exchange.

(4) In the equation ($N_a = -GPP + R_a + 0.3R_{h,s} + 0.7R_{h,q}$), the Rh is not only from litter decay but also soil organic matters (SOM). If the Rh includes SOM dynamics, the results in this study also indicate that model could better capture the seasonality of global CO2 exchange if more carbon losses are simulated from the slow/passive SOM.

(5) I cannot fully agree with the discussion in P71 Line 1-10. It's right there should be something disrupts the balance between the organic matter decay and production and leads to a pronounced seasonality of NEP. But, the reason could

be the transfer of carbon among pools in terrestrial ecosystems, which is usually defined as carbon residence time. There is always mismatch between carbon influx and efflux in different seasons wince the photosynthetic carbon influx cannot return to the atmosphere immediately. As shown by Xia et al. 2013, the carbon residence time played an important role in regulating terrestrial carbon stocks. I agree with the author that the shift between the phase of NPP seasonal cycle and the seasonal cycle of litterfall production may has some effects, but I'm not sure it is a key cause in all land regions.

References cited in this comments:

Graven HD, et al. Enhanced Seasonal Exchange of CO2 by Northern Ecosystems Since 1960. Science, 341: 1085-1089.

Xia J, et al. Traceable components of terrestrial carbon storage capacity in biogeochemical models. Global Change Biology, 19: 2104-2116.