

Point by point response	
Comment	Response
The paper suggested a 70% partitioning coefficient of quickly decaying fractions in the total litterfall will greatly improve the seasonality of the net CO ₂ exchange between the atmosphere and the terrestrial ecosystems. The 70% itself might be very high for many terrestrial ecosystems	This coefficient was changed to 20%, Figure 4 was replaced, justification of this setting was added.
Another question about the partitioning coefficients for quickly (Rh,q) and slowly (Rh,s) decaying litter pools is that whether the partitioning coefficients keep constant during the whole year.	This question seemingly resulted from the ambiguous caption to the Figure 4. I changed the caption.
Can the approach in this study capture the enhanced seasonality of CO ₂ exchange in the Northern Hemisphere(Graven et al. 2013)? It	I added the phrase explaining that this is not an attempt to make by a simple model the things that complex models failed to do: "Many aspects of complex model behaviors are beyond the scope of this study. Among them are the increasing amplitude of the seasonal changes in the globally averaged monthly concentrations of carbon dioxide (Graven et al., 2013) and the spatial distribution of soil carbon (Todd-Brown et al. , 2013). The version of the MONTHLYC model is used as a minimal model, that is, merely to explore the factors that affect the amplitude of seasonal changes in Na."
In the equation ($Na = -GPP + Ra + 0.3Rh,s + 0.7Rh,q$), the Rh is not only from litter decay but also soil organic matters (SOM).	I replaced this equation by an unambiguous version: $Na_{mod}(m) = -GPP(m) + Ra(m) + Rh,s(m) + Rh,q(m)$ (p 9, line 10)
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	I removed these words, added reference to Xia et al (2013), and add Apendix discussing residence times.

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defined as carbon residence time.	
it is not clear that the partitioning among the slow- and fastdecomposing pools is at all realistic and that it is the choice of (unrealistic) parameter values that enables the better comparison in CO2 seasonality	Appendix 1 was added to explain the partitioning among slow- and fast decomposing pools as aggregation of the pools proposed by Parton et al. (1987), the parameter values were also justified using the parameters from the work of Parton et al. (1987).
The author states that the motivation from this work was the "lack of research articles proving that the seasonal cycle of the globally averaged monthly atmospheric concentration of CO2 could be explained by the net exchange between the atmosphere and the terrestrial part of the biosphere"	I removed these words.
there is no spatially resolved information to determine whether limiting substrate availability moves individual sites in the right direction with respect to the seasonal cycle amplitude goes	I added Appendix 2 "Explaining the seasonal cycle of NEE at a Fluxnet site"
Previous work has suggested that most of the seasonality in atmospheric CO2 is driven by high northern latitude ecosystems (Randerson et al., 1997). Could the authors get the same result if their modification were made only in the northern hemisphere? If so, does this mean the seasonal behavior in temperate and tropical ecosystems is unconstrained without considering more spatially explicit data?	I added the phrase: "Numerical experiments also show that most seasonal changes in Na can be attributed to seasonal changes in NEE in the ecosystems located to the north from 25N. Hence, one need not increase WUE of raingreen forests to increase the amplitude of Na;mod: it seems that underestimated is the productivity of Northern Hemisphere deciduous broadleaf and mixed forests."
The author raises questions as to whether the mismatch between models and observations could arise from limitations in the observational network, but this analysis should be more fully developed or removed from the paper.	I removed this question from abstract and discussion, and keep only citation from (Chen, 2011): "According to Chen (2011), "The apparent discrepancy between modeling results and observations results from the "representation error" of observation stations" (Chen, 2011). This assumption is challenged here by demonstrating that the discrepancy can be reconciled through model tuning."