Dear Referee 2:

We greatly appreciate your time and effort to read, understand, and make comments on our manuscript. We have carefully studied your comments and revised the manuscript accordingly. Hope our responses have adequately addressed your concerns so that we can develop mutual understanding about your concerns and about what we present in the paper.

Please note the line numbers and pages numbers in this letter are all refereed in the revised manuscript.

Yiqi Luo
On behalf of all the authors

Below we list our point-to-point responses to your (i.e., referee 2 in this case) comments:

[Comment] In spite of words "mathematical foundation" in the title, the first mistake is contained directly in the first formula (1). Let's rewrite it in the component form:

$$\begin{pmatrix} X_1' \\ \dots \\ X_n' \end{pmatrix} = \begin{pmatrix} B_1 \\ \dots \\ B_n \end{pmatrix} u(t) - \begin{pmatrix} A_{11} & \dots & A_{1n} \\ \dots & \dots & \dots \\ A_{n1} & \dots & A_{nn} \end{pmatrix} \begin{pmatrix} \xi_1 & 0 & 0 \\ 0 & \dots & 0 \\ 0 & 0 & \xi_n \end{pmatrix} \begin{pmatrix} K_1 & 0 & 0 \\ 0 & \dots & 0 \\ 0 & 0 & K_n \end{pmatrix} \begin{pmatrix} X_1 \\ \dots \\ X_n \end{pmatrix} \qquad (1)$$

and see that in this notation all off-diagonal elements of matrix *A* are useless, and the system (1) is simply a set of trivial linear equations for disconnected variables. Do the authors know that matrix multiplication is non-commutative? My hypothesis is that the matrix A should be stated after other multipliers in the second member of the sum:

$$X'(t) = Bu(t) - \xi KAX(t)$$

Such a formula is at least mathematically correct and allows the following component view:

$$\begin{pmatrix} X_1' \\ \dots \\ X_n' \end{pmatrix} = u(t) \begin{pmatrix} B_1 \\ \dots \\ B_n \end{pmatrix} - \begin{pmatrix} \xi_1 & 0 & 0 \\ 0 & \dots & 0 \\ 0 & 0 & \xi_n \end{pmatrix} \begin{pmatrix} K_1 & 0 & 0 \\ 0 & \dots & 0 \\ 0 & 0 & K_n \end{pmatrix} \begin{pmatrix} A_{11} & \dots & A_{1n} \\ \dots & \dots & \dots \\ A_{n1} & \dots & A_{nn} \end{pmatrix} \begin{pmatrix} X_1 \\ \dots \\ X_n \end{pmatrix} \qquad (1\text{-a})$$

Consequently all next formulas should be corrected according to the new form of (1). It's completely unclear why "all off-diagonal values a_{ji} are negative" (page 8).

[Response] We are grateful to you for your time and effort to examine mathematical formulas. We agree with you that it is critical to make sure the mathematical expression of biological processes should be correct before we do any analysis.

Your comment prompted us to carefully re-examine the equation. After the multiplication of ξ,

K and A, Equation 1 becomes:

$$\begin{bmatrix} X_1' \\ X_2' \\ \cdots \\ X_n' \end{bmatrix} = \begin{bmatrix} B_1 \\ B_2 \\ \cdots \\ B_n \end{bmatrix} u(t) - \begin{bmatrix} A_{11}\xi_1K_1 & A_{12}\xi_2K_2 & \cdots & A_{1n}\xi_nK_n \\ A_{21}\xi_1K_1 & A_{22}\xi_2K_2 & \cdots & A_{2n}\xi_nK_n \\ \cdots & \cdots\cdots\cdots & \cdots & \cdots \\ A_{n1}\xi_1K_1 & A_{n2}\xi_2K_2 & \cdots & A_{nn}\xi_nK_n \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ \cdots \\ X_n \end{bmatrix}$$

Then the carbon dynamics in pool 1 will be described by:

$$X_1' = B_1u(t) - (A_{11}\xi_1K_1X_1 + A_{12}\xi_2K_2X_2 + \cdots + A_{1n}\xi_nK_nX_n)$$

The above equation states that change in carbon content in pool 1 equals carbon influx from a fraction of NPP (i.e., $u(t)$ times partitioning coefficient B_1) minus decomposition expressed by $(A_{11}\xi_1K_1X_1 + A_{12}\xi_2K_2X_2 + \cdots + A_{1n}\xi_nK_nX_n)$. Since K is decomposition coefficient, the term K_1X_1 describes that decomposition of carbon in pool 1 equals K_1 times X_1, so on for K_2X_2, and K_nX_n. Environmental scalar ξ_i modifies its corresponding K_i. Transfer coefficient A_{1j} in the above equation describes carbon transfer from pool j to pool 1. In the real world, no carbon is transferred from other plant, litter, and soil pools to leaf pool. Thus $A_{1j} = 0, j \neq 1$.

However, not all $A_{ij} = 0, j \neq i$. In TECO model with carbon transfer pathways as depicted in Figure 1a, $A_{41} \neq 0$ as it represents litterfall from leaf pool to metabolic litter pool. Xia et al. (2012) explicitly described the A matrix with all elements for CABLE model. There are many zero but several non-zero elements in matrix A to represent carbon transfers among pools. Many of those none-zero transfer coefficients as represented by A_{ij} are related to microbial carbon use efficiency.

Let us look at the equation you suggested (i.e., Equation 1-a). After the multiplication of ξ, K and A, Equation 1-a will become:

$$\begin{bmatrix} X_1' \\ X_2' \\ \cdots \\ X_n' \end{bmatrix} = u(t) \begin{bmatrix} B_1 \\ B_2 \\ \cdots \\ B_n \end{bmatrix} - \begin{bmatrix} A_{11}\xi_1K_1 & A_{12}\xi_1K_1 & \cdots & A_{1n}\xi_1K_1 \\ A_{21}\xi_2K_2 & A_{22}\xi_2K_2 & \cdots & A_{2n}\xi_2K_2 \\ \cdots & \cdots\cdots\cdots & \cdots & \cdots \\ A_{n1}\xi_nK_n & A_{n2}\xi_nK_n & \cdots & A_{nn}\xi_nK_n \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ \cdots \\ X_n \end{bmatrix}$$

and the carbon dynamics in pool 1 will be described by:

$$X_1' = B_1u(t) - (A_{11}\xi_1K_1X_1 + A_{12}\xi_1K_1X_2 + \cdots + A_{1n}\xi_1K_1X_n)$$

In the above equation, the term K_1X_2 or K_1X_n biologically does not make sense as it describes the decomposition of carbon in pool 2 by coefficient K_1. The latter describes the relative rate of decomposition of pool 1. Nor does K_1X_n biologically make sense.

Thus, we hope that you can see that our original equation still works.

The statement "all off-diagonal values a_{ji} are negative" (page 8) because carbon transfer from pool i to pool j to be positive by having negative coefficient multiplied with negative sign for this term. We have clarified this point by revising the sentences on line 177-179 as:

"In eq. 1, all the off-diagonal a_{ji} values are negative to neutralize the minus sign to indicate positive C influx to the receiving pools"

[Comment] But the more essential question is concerned to it's biological correctness and sense. According to (1, 1-a) matrix *A* consists of transfer coefficients and does not depend on system variables *X* making all the system non-autonomous and linear. There is no biological foundation for such strong universality of the form (1, 1-a) for all temporal and spatial scales and no mathematical proof in the paper. In particular, it's not clear how mass-balance relations are connected with that form.

[Response] Thanks for your question about the biological basis of the mathematical equation. The two paragraphs from line 144 to 162 describe the biological basis as below:

"Hundreds of models have been developed to simulate terrestrial C cycle (Manzoni and Porporato, 2009). All the models have to simulate processes of photosynthetic C input, C allocation and transformation, and respiratory C loss. It is well understood that photosynthesis is a primary pathway of C flow into land ecosystems. Photosynthetic C input is usually simulated according to carboxylation and electron transport rates (Farquhar et al., 1980). Ecosystem C influx varies with time and space mainly due to variations in leaf photosynthetic capacity, leaf area index of canopy, and a suite of environmental factors such as temperature, radiation, and relative humidity (or other water-related variables) (Potter et al., 1993; Sellers et al., 1996; Keenan et al., 2012; Walker et al., 2014).

Photosynthetically assimilated C is partly used for plant biomass growth and partly released back into the atmosphere through plant respiration. Plant biomass in leaves and fine roots usually lives for several months up to a few years before death, while woody tissues may persist for hundreds of years in forests. Dead plant materials are transferred to litter pools and decomposed by microorganisms to be partially released through heterotrophic respiration and partially stabilized to form soil organic matter (SOM). SOM can store C in the soil for hundreds or thousands of years before it is broken down to CO_2 through microbial respiration (Luo and Zhou, 2006). This series of C cycle processes has been represented in most ecosystem models with multiple pools linked by C transfers among them (Jenkinson et al., 1987; Parton et al., 1987; 1988; 1993), including those embedded in earth system models (Ciais et al., 2013). "

Moreover, we have conducted many synthesis studies to examine different aspects of the biological basis. The carbon input via canopy photosynthesis as described by $\begin{bmatrix} B_1 \\ B_2 \\ \cdots \\ B_n \end{bmatrix} u(t)$ has been well accepted. Scientists in the community have questioned carbon transformation through $A\xi KX$ in equation 1. We examine six assumptions of those carbon cycle models and the validity of our analysis in section 4.1 on pages 19-23. We would be happy to answer any specific questions you would have regarding those assumptions.

[Comment] Page 9 gives us an example of a risky statements made in the paper. Authors say that almost all world models of carbon cycle in terrestrial ecosystems have the form (1). They refer to the work (Manzoni, Porporato, 2009) and state that there is a review of 250 models of carbon cycling in it ! First, Table A2 in this work has 200 references to papers describing different versions of a smaller number of models. Second, I have a very strong doubt that all of them can be presented in the form (1) because they were made for various time scales, different set of compartments and different details of biogeochemical processes accounted for. Interesting is the fact that the model of Manzoni and Porporato (2009) themselves is nonlinear and does not look like the system (1) ! As well as another model of soil organic carbon and microbial dynamics made by Hararuk et al. (2015) also referred to by the authors !

[Response] Thanks for your comment. We agree with you that the nonlinear microbial models by Manzoni and Porporato (2009) or Hararuk et al. (2015) could not be represented by equation 1. This issue is pointed out in section 4.1 regarding those microbial models (i.e., assumption 1). We also pointed out that thousands of datasets we have reviewed do not seem to support those nonlinear microbial models as described on pages 19-20. Paper by Sierra and Müller (2015) also stated that most of the land carbon cycle models can be represented by equation 1.

Indeed, we have worked with many modeling groups and organized those carbon balance equations in their models into the matrix equations. It has been demonstraed that the matrix equation can represent those original models well as described in paragraph from lines 612-621. Please see another publication by Ahlström et al. (2015) with LPJ-GUESS for the application of eq. 1.

[Comment] In part 2.2 (pages 9-11) authors carry out comparison of the TECO terrestrial ecosystem model results and the system (1) calculations. Their statement on a 100% match of NEE calculations for TECO and (1) seem strange. If TECO is independent of the system (1) this is unbelievable result, in the opposite case the comparison has no sense.

[Response] This is the case. We organized those carbon balance equations of TECO into the matrix equation. We run the matrix equation to get the exact simulation outputs as from the original TECO model. We have done that with CABLE (Xia et al. 2012, 2013). CLM3.5 (Rafiquee et al. 2016), CLM4.5 (Shi et al. in prep.), BEPS (Chen et al. 2016), and LPJ-GUESS (Ahlström et al. 2015). In all the cases, the matrix equation can 100% reproduce simulations of those original models. It is unbeliveable. We understand it is surprising.

[Comment] Introducing two new definitions – the C storage capacity and C storage potential – could be a good idea of this paper if authors would explain their biological interpretation and mathematical correctness. First, we should make correspondence to (1-a) and note that $\tau_{ch} = (\xi KA)^{-1}$ instead of (3). Second, study of existence for this inverse matrix is needed to state mathematical correctness of these definitions because inverse matrix serves as a foundation for all math terms in the following text. There is no such study in the paper. Another question arises about chasing time τ_{ch}: why it's formula $\tau_{ch} = (\xi KA)^{-1}$ should have physical dimension of time ? There are no explanations in the text.

[Response] The biological interpretation of C storage capacity is given in Abstract (Line 42-45, 46-47), Results (lines 259-270), Discussion section 4.2, and Conclusions. For example, sentences on lines 632-635 in the Conclusion section state:

"The capacity, which is the product of C input and residence time, represents their instantaneous responses to a state of external forcing at a given time. Thus, the C storage capacity quantifies the maximum amount of C that an ecosystem can store at the given environmental condition at a point of time."

Similarly, C storage potential is also biologically explained in Abstract, Results, Discussion, and Conclusions sections. For example, the first paragraph in section 4.3 is:

"The C storage potential represents the internal capability to equilibrate the current C storage with the capacity. Bogeochemically, the C storage potential represents re-distribution of net C pool change, $X'(t)$, of individual pools through a network of pools with different residence times as connected by C transfers from one pool to the others through all the pathways. The potential is conceptually equivalent to the magnitude of disequilibrium as discussed by Luo and Weng (2011)."

Thanks for your comment. We have added the time dimension for chasing time on lines 258-259 as:

"In eq. 2, we name the term $(A\xi(t)K)^{-1}$ the chasing time, $\tau_{ch}(t)$, with a time unit used in exit rate K."

[Comment] All inputs in the model (1) are supposed constant or time-dependent. In particular on page 15 plant photosynthesis is declared only time-dependent. But for some temporal scales (a year, for example) it can essentially dependent on the plant carbon content and in that case the model (1) should have another form (Parolari, Porporato, 2016).

Reference

Parolari A., Porporato A., Forest soil carbon and nitrogen cycles under biomass harvest: stability, transient response, and feedback. // *Ecological Modelling*, v. 329, 2016, pp. 64-76.

[Response] We have carefully studied the paper by Parolari and Porporato (2016), particularly that paragraph on NPP on page 66. That study differentiated the productivity regime into C-limited and N-limited. The C-limit regime accounts for limitation of light, temperature and moisture whereas the N-limited regime accounts for nitrogen limitation. Both of the regimes have been discussed in relation with eq. 1. Please see sentences on lines 148-152 and lines 490-493 for more explanation. In the revised manuscript, we cited the paper and explained those environmental factors as represented by scalars on line 152.

[Comment] Therefore, since all other formulas and descriptions are based on the terms introduced above with mistakes as well as statements made without sufficient biological basis, the conclusion at page 25 (part 4.4, first sentence) about novel approach suggested by the authors to understand, evaluate, diagnose and improve carbon cycle models is represented as inadequate and seems early and premature.

[Response] We hope our responses to your comments above can help us communicate well with you and then gain mutual understanding on what we presented in this paper and what you were concerned.

We thank you for the valuable comments, some of which led us to improve the text and better communicate our points to the reader. We hope our responses above also demonstrate that our formulation did not include mistakes and that the terms we introduced were founded on sound biological principles. Thus, we stand by our conclusion that the presented approach enables one to understand, evaluate, diagnose and improve carbon cycles models.