Manuscript: **Transient Dynamics of Terrestrial Carbon Storage: Mathematical Foundation and Numeric Examples**
Author(s): **Yiqi Luo et al.**

Evaluation: rejection

Remarks and comments

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:

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

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:

$$X^{'}(t) = Bu(t) - \xi K A X(t).$$

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

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

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).

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.

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 !

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.

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.

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). 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.

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.

Overall conclusion: the manuscript should be rejected.