

Interactive comment on “The quasi-equilibrium framework re-visited: analyzing long-term CO₂ enrichment responses in plant-soil models” by Mingkai Jiang et al.

Anonymous Referee #2

Received and published: 24 January 2019

This study tries to establish a quasi-equilibrium (QE) analytical framework, introduced by Comins & McMurtrie (1993), for evaluating model assumptions on carbon-nitrogen interaction in influencing ecosystem responses to elevated CO₂. Overall, this paper is extremely valuable for understanding a variety of assumptions in influencing model outputs of carbon and nitrogen coupling.

I particularly like your examples on page 23 to make a point that “the QE framework can highlight where additional complexity is not valuable.”

Here are a few suggestions to improve your manuscript:

First, the authors may consider improve the readability of your paper so that your mes-

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sage can go more miles. It is quite competent of the authors to work out all those equations in section 3. But those equations will hinder delivering your message as not all the ecologists or even modelers will go over those equations when they read your paper. In addition, would it be possible to convert Table 1 to a graph so that readers can quickly get your message? To me, Table 1 is probably the most important part of your manuscript. Even though I am familiar with the subject, it still takes me a while to go over the table. Converting it to a figure may help deliver your message faster. Moreover, the abstract I don't think deliver the message well, especially the second half.

Second, the work by Comins & McMurtrie (1993) is great. But, during the same period in 1990s, Dr. Edward Rastetter has developed the Multiple Element Limitation (MEL) model of carbon-nitrogen interactions. He published a few papers to illustrate similar principles on carbon-nitrogen interactions as revealed by G'DAY. In fact, Ed Rastetter also lumped all those assumptions (or processes) into three categories as in the first three items of your Table 1. MEL further shows the time scales at which each of the three categories of processes plays. In other words, MEL not only gives information about the equilibrium responses but also offers information about C/N interaction to influence transient dynamics. I think the authors at least should acknowledge Ed's work in your manuscript.

Third, it is fine that the G'DAY model offers an analytical framework to evaluate model assumptions on carbon and nitrogen interactions. However, the impacts (or sensitivity) of those assumptions evaluated by the framework depend on the ranges of the variables you changed. For example, your analysis shows that wood N:C flexibility is very important for modeling carbon and nitrogen interactions. What ranges of wood N:C did those studies change? Do those ranges realistically match observations? Lots of data are available to evaluate those ranges. In fact, several studies have evaluated the ranges of changes of those variables (e.g., Liang et al. 2016). Bringing observations into your study may require the authors to do additional work but will improve quality

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of your study. At least the authors should add discussion on observed vs. modeled ranges of changes.

Forth, if the authors want to popularize the QE framework to be used by the broad community, they may develop a simpler scheme for others to use. The extensive list of those equations may make it very difficult for others to use.

Reference: Junyi Liang, Xuan Qi, Lara Souza, and Yiqi Luo. 2016. Processes regulating progressive nitrogen limitation under elevated carbon dioxide: a meta-analysis. *Biogeosciences*, 13, 2689-2699.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-291>, 2018.

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