Tomomichi Kato

University of Tsukuba, Tsukuba, Ibaraki, Japan

Handling Topical Editor, Geoscientific Model Development (GMD)

Re: GMD-2018-41

Dear Dr. Kato,

Thanks so much for sending us two referees' comments and suggestions on our manuscript "Carbon-nitrogen coupling under three schemes of model representation: Traceability analysis" (GMD-2018-41). We greatly appreciate the two reviewers for their valuable comments and suggested amendments. Their inputs have helped improve the paper tremendously. We have carefully studied the comments from the reviews and made revisions based on them in this version of manuscript.

In the revised manuscript, we have addressed all the comments from the two referees. Specifically, we added the Figure 5 for annual averaged size and C:N ratio of each C pool and the Figure 6b for the CUE in the C-only version and the three C-N schemes of TECO model. We also have added a new figure (Figure S2) in the supplemental information to show the annual averaged N content for each C pool among the three C-N schemes. In the Materials and Methods and Results sections, we also have shown the different effects of C-N coupling hypotheses among three simulations as suggested by both referees. We clarified our discussions based on more referenced results as suggested by the referee #1 and discussed the differences with the original models (CLM4.5bgc and O-CN) as suggested by referee #2. We greatly appreciate the suggestions from the reviewers, as addressing them has strengthened the manuscript.

We confirm that all authors have met the authorship criteria.

We also declare that the submitted work is our own and that copyright has not been breached in seeking its publication.

Here are our detailed responses to the reviews. Please note that the comments from the referees are in ***italics*** followed by our responses in **regular** text.

We hope you will find our revision satisfactory for publication in ***Geoscientific Model Development***.

Yours Sincerely,

Xuhui & Jianyang

Xuhui Zhou, Jianyang Xia
School of Ecological and Environmental Sciences, East China Normal University
500 Dongchuan Road, Shanghai 200062, China
Email: xhzhou@des.ecnu.edu.cn, jyxia@des.ecnu.edu.cn

Response letter to comments (gmd-2018-41)

Will Wieder's comment (Referee #1)

General comments

Du and co-authors present a very interesting study using a matrix approach to compare the implementation of three distinct representations of C-N biogeochemistry in the TECO land model. The mathematical approach seems very powerful and the results are compelling.

[**Response**] Thanks so much for your positive comment.

I'd encourage the authors to unpack their results more to make findings more accessible to readers not familiar with any of the N schemes presented here. Refocusing the text around big differences in assumptions being made between each modeling approach and how that translates to the different C stocks and fluxes would be very helpful.

[**Response**] Thanks very much for your suggestions. In this revised version, we added more results (e.g., C pool sizes and C:N ratio in Figure 5, CUE in Figure 6b, the sensitivity of N processes to ecosystem C storage capacity in Figure 10c, and N pool sizes in Figure S2) to support our findings. Based on our results, we displayed the different N and C fluxes under different C-N schemes (Figures 3 and 4) and the different C and N status among plant tissues, litter and soil pools (Figures 5 and 6) as well as the ecosystem C storage capacity (Figure 7). To evaluate the alternative representations of C-N processes dominating the ecosystem C storage capacity, we applied the traceability analysis framework to trace the key factors in different schemes. We found that different process assumptions caused divergent C residence time and plant production among different C-N schemes in this study (Figures 8-10). We added the detailed information and discussion in both Result and Discussion sections in Lines 372-381, 386-389, 425-428 and 498-501.

The discussion only sparing refers to the display items presented in the results, making me wonder if the ideas being discussed are just the authors' opinions or if they can clearly be demonstrated by results presented here. On revision, please reference display items to support claims being made in the discussion.

[**Response**] Sorry for the confusion. We carefully revised the whole manuscript and also referenced more necessary results in the Discussion section accordingly. As a consequence, our manuscript has been considerably improved. We hope you will find our revision satisfactory.

Finally, there are enough grammatical errors to be distracting in the text. Some of these a highlighted in technical corrections, below, but revisions to the manuscript should be made for language fluency.

[**Response**] We carefully revised the manuscript according to the comments, paid attention to the grammar, and made necessary changes. We also asked a native English speaker (Mrs. Megan C. Foster) to revise the whole manuscript. Please see below for the detailed responses point by point. As a consequence, our manuscript has been considerably improved. We hope you will find our revision satisfactory.

Specific comments

Line 60: For a paper that's more generally about the implementation and assumptions of C-N coupling in land models it strikes me as odd to lead off the introduction with an immediate nod to nitrogen fixation. Fixation is important, but leading off with a brief discussion sets up unrealistic expectations for the reader for what's ultimately being discussed in the paper.

[**Response**] Thanks for your comments and suggestions. We deleted the description of nitrogen fixation and have rephrased this paragraph carefully, especially emphasizing the processes of carbon-nitrogen coupling in affecting the terrestrial ecosystem C storage.

Line 84: References are needed to support these claims, as it seems to conflate C cycle uncertainty (e.g. Arora et al. 2013) with C-N representation in models, which is not accurate.

Line 86: Similarly, references are needed as the 'contradictory results' from implementation of C-N models have not been clearly established in the literature.

[**Response**] We revised the descriptions of the related references (Arora et al., 2013; Zaehle et al., 2015; Sokolov et al., 2008; Wania et al., 2012; Walker et al., 2015) and added them in our revised manuscript in Lines 85 and 89.

Line 97: I may be forgetting something, but don't recall the Xia et al (2013) paper accomplishing all that it's being credited for here. Maybe other references are needed where the authors demonstrate how the matrix approach has been used for 'benchmark analyses, model intercomparisons, and data model fusion, and improved model predictive power'? Otherwise revise this sentence to avoid implying a single paper did all this work.

[**Response**] Thanks for your comments and suggestions. We added some references and modified the sentence as "The traceability analysis has been developed to diagnose the simulation results within (Xia et al. 2013; Ahlström et al., 2015) and among (Rafique et al., 2016; Zhou et al., 20) models.".

Figs 1 & 2. How is mineral N retranslocated from the litter pool? After a leaf has fallen do plants still have access to this N? Doesn't retranslocation occur before senescence?

[**Response**] Sorry for the mistake. The mineral N was retranslocated to other tissues before the live tissues (i.e., leaves, fine roots and live stems) senescence in TECO model. We simply added an arrow to plant growth module to represent the retranslocation of the mineral N to other tissues in the Figs. 1 and 2. We described it in Lines 158.

Fig 2. I really appreciate the effort to clearly spell out different assumptions between different C-N coupling schemes and map onto the structure of TECO's C and N pools. I fear this figure is too jumbled with small, tilted text to be useful, and would encourage authors to spend some time cleaning up this display item so it's more clear & useful.

[Response] Thanks so much for your suggestions. We deleted all the numbers and rearranged the text in the figure to clarify the display.

From the description in the methods, it seems like the entire coupling of C-N biogeochemistry occurs through the different implementation of the N scalar from each scheme (Eq. 30). Is this true? If so, documenting how the aspects summarized in Table 1 are actually being implemented seems important (either in the main text, SI, or an appendix). If this is where the magic happens it should be clearly spelled out using language from the N related (red) text in Fig 1.

[**Response**] Sorry for the confusion. The N scalar is set as the respiration and decomposition rate modifier, which considers the changes of N content to compare with the initial condition (Eq. 33). Depending on both the N supply and loss for each C pool, the N scalar mainly affects the C residence time directly (Fig 7). The different aspects among three C-N coupling schemes introduce different effects on N supply and losses directly and/or indirectly, and thus affect the C residence time via N scalar. Beside the N scalar, the different representations summarized in Table 1 also introduce other aspects to affect the C storage. For example, the different implementations of the N down regulation have differently constrained power on GPP (although those powers were not significant in this study); the different assumptions on tissue C:N ratio led to different C allocation ratio (eq.1-6) and further affect the baseline residence time (eq.30); the different representations of plant N uptake and biological N fixation result in different C investment, and thus the different autotrophic respiration. In this revised version, we added those description and discussion in both Method and Discussion sections in Lines 319, 406-409, and 529-536.

In previous work this author group has demonstrated that the matrix approach gives identical results to the conventional system of differential equations. Can a similar plot be made with a CN version of TECO? That is, can lumping a coupled C:N model into a "N scalar" (eq. 33) account for everything that's going on in the model? I'm assuming it can, but this is never clearly demonstrated in the results.

[**Response**] That version of TECO-CN had incorporated the "N scalar" into the respiration and decomposition rate modifier (Du et al. 2017), which had been used in the previous work (e.g., Zaehle et al., 2014). In this study, N scalar is a key factor, and we separated it from the environmental scalar (ξ_E) and baseline carbon residence time ($\tau_E^{'}$) in the traceability analysis framework to trace the different effects that were introduced by the three C-N schemes. We also compared our TECO-CN version with the version used in Zaehle et al., 2014. We found that the results matched well (See Figure R1 below).

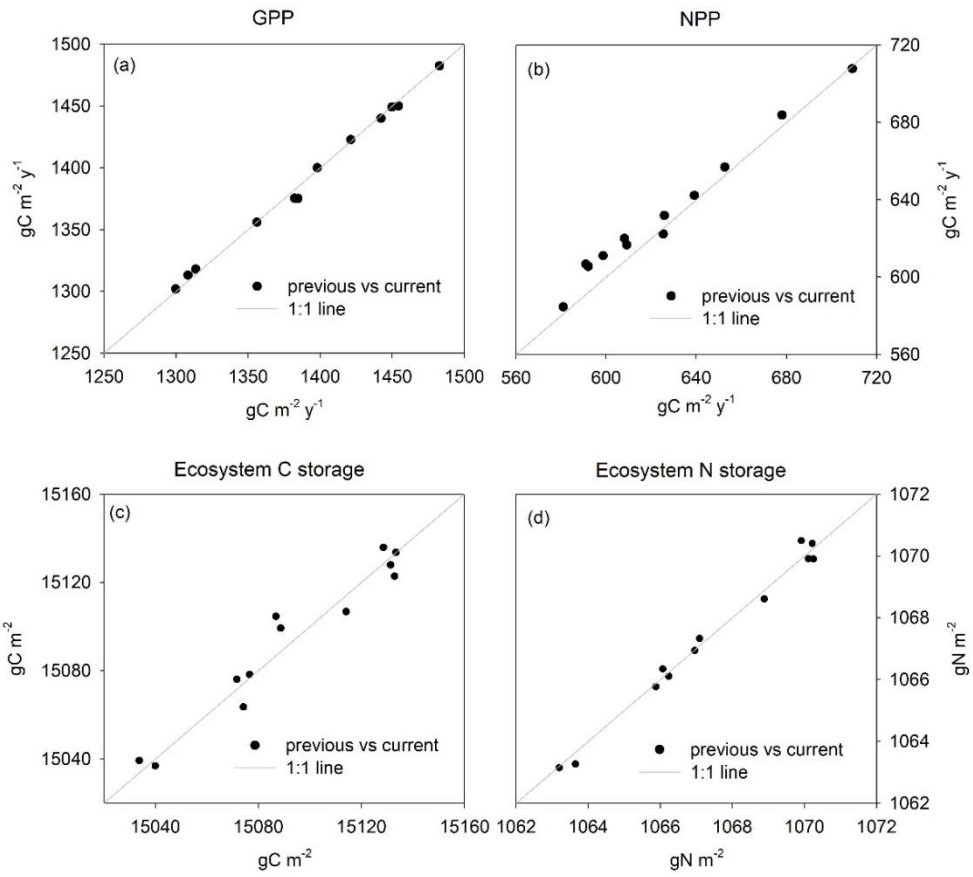

Figure R1. Comparisons of GPP, NPP, ecosystem C storage and ecosystem N storage at the steady state from this study vs. the TECO-CN version used previous work.

Besides difference in NUE (Fig. 5) I'm struck by the differences in carbon use efficiency (CUE, the ratio of NPP:GPP) among N models that's attributable to large difference in autotrophic respiration among models. Is this worth displaying or discussing further?

[**Response**] Thanks for your suggestions. Yes, we found that carbon use efficiency (CUE) varied among three N schemes. The SM2 has the highest CUE while SM3 has the lowest CUE among three C-N schemes. We added this result in Fig 5b. The direct factors of those differences mainly attribute to difference in autotrophic respiration and N limitation on production (i.e., down-regulation effect). For the SM2, plant uptake N does not need to cost C, which lead to the highest CUE. In the SM3, however, the lowest CUE is due to both the C cost of plant actively uptake N and the assumption that increases respiration to remove the excess C. In this revised version, we added those Results and Discussion sections in Lines 386-389 and 498-501.

Why did SM1 increase the mean residence time of C relative to the control model (Figs 6 inset & 7). I'm assuming it's because of N 'limitation' of passive C turnover? Does this seem realistic? It must be caused by relatively quick turnover of this pool and an low C:N ratio of SOM in SM1, or low respiration coefficient in fluxes between slow and passive pools that are driving a high immobilization flux in SM1 (Fig. 3)? Alternatively, does the stoichiometry of litter quality drive these results? More details on these mechanisms seem worth discussing?

[**Response**] Thanks so much for your comments and suggestions. Yes, the slower turnover rate of passive SOM pool dominated a longer mean ecosystem residence time in SM1 compared with those in C-only version. Our results showed that lower heterotrophic respiration rate (Figure 4) and C:N ratio of passive SOM (Figure 5b) as well as higher immobilization flux (Figure 3) jointly 'limited' the turnover rate of passive SOM pool. For the SM1, the microbe immobilization dominates a low C:N ratio and then affects the decomposition cascade for passive SOM (Fig 8). The reason is that the representation of N immobilization in TECO-CN has the potential to accumulate N:

$$Imm_N = \begin{cases} \sum_{i=4}^{8} min\left(\left(\frac{C_i}{CN0_i} - \frac{C_i}{CN_i}\right), 0.1 * SN_{min}\right) \; for \; CN_i \geq CN0_i \\ \sum_{i=4}^{8} min\left(\left(\frac{C_i}{CN_i} - \frac{C_i}{CN0_i}\right), 0.1 * SN_{min}\right) \; for \; CN_i < CN0_i \end{cases}$$

We added this equation and more information in Method and Discussion sections.

Figures 7 and 9 seem like really interesting, powerful strengths of the tractability analysis presented here. In my estimation there's not nearly enough text in the results or discussion to walk readers through what's being shown here. Unpacking the information communicated in these figures would help readers access what's being shown and how the tractability analysis helps us understand differences among model formulations. (Note, some of this could even fall into the introduction and methods by foreshadowing key differences among model formulations that are important to the results presented here from the start).

[**Response**] Thanks for your comments and suggestions. In the revised version, we reorganized the information communicated in these figures carefully, mainly tracing how the different hypotheses among C-N coupling schemes modulate the ecosystem C storage based on traceability analysis. We hope that you satisfy our revision.

Line 508: If this is the most striking difference, is there a take home figure that clearly communicated this message? As presented, I'm not sure this conclusion is well supported by the results or discussion.

[**Response**] Sorry for the confusion. Originally, we used the sensitivity of N processes to NPP and ecosystem residence time (τ_E) among three C-N coupling schemes to display this difference, which was shown in the previous Fig. 9. In the revised version, we extended this sensitivity to ecosystem C storage (NPP $\times$ τ_E) in Fig. 10 based on the different representations among three C-N schemes. We emphasized the difference and added more discussion in Lines 425-428 and 501-505.

Technical corrections

Line 37: For clarity, replace 'them' with 'the three C-N coupling schemes'

Line 43: Consider replacing 'divergent' with 'differences in'?

Line 58 & 64: Avoid starting a sentence with an abbreviation, that is write out 'Nitrogen'.

Line 59: 'Requires' should be plural

Line 66, I'd add Hungate et al. (2003) to this list of references

[**Response**] Done as suggested.

Line 71: It seems odd to talk about progressive N limitation as occurring with "growth enhancement when N mineralization increases". Is Dr. Luo comfortable with this definition?

[**Response**] Sorry for the confusion. We revised the sentence as "Early C-N coupled models demonstrated that the N availability limits ecosystem C storage capacity with associated effects on plant photosynthesis and growth in many terrestrial ecosystems…"

Line 72: Awkward. Please revise for fluency & clarity.

[**Response**] Sorry for the confusion. We revised this sentence as "Recent studies have largely confirmed these results by improving C-N coupling models with multiple hypotheses."

Line 80: These are from Cleveland et al (1999), not my work, and their implementation in models is summarized nicely by Meyerholt et al. (2016).

[**Response**] Thanks for pointing out our mistake. We added these two references and replaced the "Wieder et al., 2015" to "Wieder et al., 2015a".

Line 129: Should this be 'data', not 'date'?

[**Response**] Sorry for the mistake. We replaced "date" by "data".

Also from what plots, the meteorological *paragraph starts off discussing the AmeriFlux tower, but are the biomass data from the control FACE plots?*

[**Response**] Sorry for the confusion. The forcing data used in this study were taken from the AmeriFlux database, while the biomass data were taken from the reference study. To clarify this point, we revised the first sentence of this paragraph as "The forcing data used in this study were taken from the Duke free-air CO_2 enrichment (FACE) experiment….".

Line 138, 180: I'm a little confused. Is this the first publication of TECO-CN2.0, if so they should be referenced? If not, are there other versions of TECO-CN and how does the implementation of C-N biogeochemistry differ in the present model?

[**Response**] Sorry for the confusion. There are two versions of TECO-CN model. The first version was used in Zaehel et al., 2014 and this study, and the second version is a simplified version used for data assimilation (e.g., models in Shi et al., 2015 and Du et al., 2017). Both versions are the variant of the TECO-C version published in Weng and Luo, 2008. To make it clear in this study, we replaced "TECO-CN" with "TECO-CN2.0" accordingly.

Table 1: References to Thorton et al are actually for CLM4cn (not CLM4.5bgc, as implied in the table). The implementation of C-N biogeochemistry is similar in each model, but the structure and stoichiometry of SOM pools are different in each? Please clarify in the text and references which version of the model is used for SM2.

[**Response**] The version of CLM4.5bgc is used for SM2 in this study. We changed the references as "Koven et al., 2013" and "Oleson et al., 2013".

Fig. 1. It seems odd to have N fixation going directly to soil mineral N pools. I realize that CLM (and likely other models) do this, but the simplification should at least be noted in the text?

[**Response**] Thanks for your comments. We added a new dotted arrows from N fixation to plant part in Figure1 and the description "*set N fixation as an option when the plant N uptake is enough for growth in terms of C investment" in the legend of Figure1.

Fig.1 Why doesn't the soil C-N module need to take up mineral N? This seems to contradict Fig. 2, and could be corrected with two-sided arrows?

[**Response**] Thanks for pointing out our mistake. As suggested, we replaced those one-sided arrows with two-sided arrows in Figure 1.

Throughout section 2.2.2 should units for fluxes be communicated?

[**Response**] Thanks for pointing out what we have neglected. The units were added in the revised version.

Eq. 19. This would give a fixation flux in gN/m2/s, but TECO doesn't work on that time step?

[**Response**] Yes, the unit of biological N fixation flux is g N m^{-2} s^{-1}. We added it in the revised version.

Line 321. What are all these abbreviations? Regardless, there's too many here to be coherent, and I'd encourage these to be written out fully throughout the text.

[**Response**] Thanks for your comments and suggestions. We deleted "i.e., DRP, PS, PUN, PMC, BNF, RtrN and SS" in this section.

Line 349. These differences are relative to the C only control? If so restating this here may help clarify?

[**Response**] Yes, these differences are relative to the results of TECO-C. In the revised version, we added "by comparison with the TECO-C version" in this sentence.

Line 351 this sentence is awkward and needs to be revised?

[**Response**] Sorry for the confusion. In the revised version, we deleted this sentence "The NPP and plant N uptake (PNU) jointly determine the N use efficiency (NUE)."

Line 396: this list of abbreviations is neither intuitive, commonly used, nor helpful. I find the later use of the abbreviations confusing and recommend just writing out the processes being discussed in full.

[**Response**] Sorry for the confusion. As suggested, we wrote out these processes in full and deleted these abbreviations in this section.

Line 420: doesn't SM2 use NPP to calculate BNF rates?

[**Response**] Thanks for pointing out our mistake. Yes, SM2 used NPP not ET to calculate BNF rate in this study. We revised the sentence as "… SM2 and SM3 simulated BNF explicitly, which used the modified empirical relationships of BNF with NPP and evapotranspiration (ET), respectively.".

Lines 445-450: Where are these results shown in the work presented here?

[**Response**] Thanks for pointing out what we have neglected. After we added a new figure (Figure 5) about C pools and their C:N ration for different treatments, these results are mainly shown in Figure 3 and Figure 5. We revised those sentences as "N stress increased tissue C:N ratio (Figure 5b), leading to a high microbial N immobilization (Figure 3) and then a lower net N mineralization (Fig 3a, g and m), which allowed plant cell construction with a lower N requirement. The inclusion of flexible C:N stoichiometry appeared to be an important feature allowing models to capture the ecosystem response to climate variability through adjusting the C:N ratio of nonphotosynthetic tissues or the whole-plant allocation among tissues (Figure 9) with different C:N ratios…".

Line 463: where are these oscillations shown in the work presented?

[**Response**] We added the related results in this sentence as "Therefore, the different impacts of ecosystem N status induce oscillating N limitation on MRT (Figure 8) due to the inherently different assumptions of C-N interactions among three C-N coupling schemes".

Line 473: This line really makes me wonder if the approach outlined here is 'right'? Regardless, it makes me think that differences among models are 100% attributable to differences in stoichiometric assumptions among models. If so, should a list of pools and their C:N ratio SM1, 2, and 3 be communicated?

[**Response**] Thanks for your comments. We added a new figure in the revised version (Figure 5). Please see below for details.

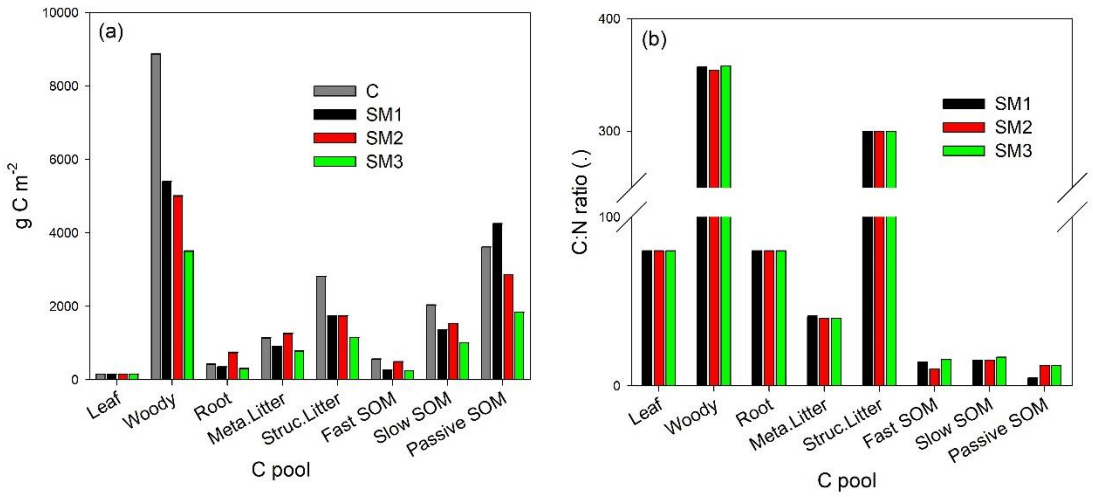

Figure 5. The annual average sizes of carbon pools (panel a) at the steady-state among 1996-2007 for C-only version and the three C-N schemes (SM1, SM2 and SM3) and the C:N ratio (panel b) of each carbon pools for the three C-N schemes (SM1, SM2 and SM3) in TECO-CN model.

Line 483: Ah, so win SM1, is there a progressive decline in litter quality that ends driving high soil N demand as the decomposition cascade tries to meet stoichiometric demand, whereas SM3 allow this extra C to be blown off through heterotrophic respiration? Alternatively, is it higher autotrophic respiration in SM3 (through increased fine root C allocation) that allows the extra C to be blown off (line 501) Sorry, I'm not familiar enough with all of these approaches to understand what each model is doing.

[**Response**] Sorry for the confusion. Yes. For the SM1, our results showed that plant nonphotosynthetic tissues (mainly wood) and litter quality impact the C:N ratio (Figure 5) and further affect their decomposition cascade for fast and slow SOM pools (Figure 6 and Figure 8). However, this was not the case for the passive SOM pool, where microbe immobilization dominates a low C:N ratio and then affects the decomposition cascade (please see response above).

For the SM3, both the hypothesis of increasing respiration to remove the excess C accumulated under N stress and the higher C investment for the BNF led to decrease in C input and then limit the microbe immobilization for the passive SOM pool.

Line 488 what's being absorbed?

[**Response**] Sorry for the confusion. We removed the "absorption" and revised the sentence as "This mechanism promotes the respiration of the faster turnover pools (fast and slow SOM pools), leading to decrease in MRT in these two pools (Figure 8)"

Line 490: I'm still confused about what's causing differences between SM1 and SM3. For readers less familiar with these schemes can the difference between the approached be unpacked a bit more, as this seems like a powerful strength of the traceability analysis?

[**Response**] Sorry for the confusion. Based on the different hypotheses (list in Table 1) between SM1 and SM3, we found that SM1 mainly adjusted plant tissue and soil C:N ratio

to reach equilibrium under N stress, while SM3 mainly cost the excess C via increasing respiration to get equilibrium under N stress. The two different strategies lead to different C allocation (Figure 9) and stoichiometric status (Figure 5), and then affect plant production (Figures 4 and 5), baseline residence time and ecosystem residence time (Figure 8) as well as ecosystem C storage (Figure 7). We added these results in the revised manuscript according to your suggestions.

Anonymous Referee #2

[General comments] In this paper, the authors evaluate three different schemes of Carbon-Nitrogen coupling in a terrestrial model, which can largely change both C and N dynamics reproduced by models. For this, they used an existent framework for analyzing the difference between the models. This paper is clearly written, and the results are informative for readers. I recognize the importance of this study because CN coupling is one of the emergent processes to be evaluated / constrained in such land ecosystem modeling.

[**Response**] Thank so much for your positive comment. No responses needed.

However, I think there are places to be improved: the figures are informative, but the explanation is not enough for readers. My comments will not require a lot of effort to improve.

[**Response**] Thanks so much for your comments and suggestions. We carefully revised the whole manuscript according to your comments and suggestions. We went through the text several times and made necessary changes. Please see below for the detailed responses.

[Detailed comments] P6, L129: "biomass production date" should be "biomass production rate"?

[**Response**] Thanks so much for pointing out our mistake. Here it is not "rate", either. It should be "data". We replaced "date" by "data" as suggested by referee #1.

P6, L129: What purpose the data "standing biomass and biomass production date" used for your study? Do you mean the datasets are used to determine the parameters associated with the processes? In addition, CN concentration for plant and soil (Finzi et al., and Lichter et al.) are also used for your analysis (I suppose the SM2 simulation need such data because of the fixed CN ratio, but it is not clear in the text). Please clarify them.

[**Response**] Sorry for the confusion. In this study, the data of both biomass and CN concentration are used to set initial values of C, N pool sizes and CN ratio for TECO-C and TECO-CN model. To make it clear, we added "To set the initial condition for the models, we collected the related datasets from previous studies." in the Lines 130-131.

P6, L138: It might be better to clearly mention first that the model is newly developed and used in this study for the first time.

[**Response**] Sorry for the confusion. There are two versions of TECO-CN model. The first version is used in Zaehel et al., 2014 and this study, and the second version is a simplified version used for data assimilation (e.g., Shi et al., 2015 and Du et al., 2017). Both versions

are the variant of the TECO-C version published in Weng and Luo (2008). To make it clear in this study, we replaced "TECO-CN" with "TECO-CN 2.0" accordingly.

P7, eq(1)-(6): The detail description of C allocation scheme of TECO-CN v2 is shown here, but it seems the equations are not referred in other places. In my simple thinking, the detail descriptions with the equations are not necessary for your analysis, and it looks no problem if your put them into supplement. If you want to keep the eqs in the main body, it should be qualitative explanations how the C allocation scheme act on CN dynamics in simulations.

[**Response**] Thanks for your suggestions. Under the traceability analysis framework, the C allocation coefficients are used to calculate the baseline C residence time (Eq. 29). In this study, since both the matrix *A* and *C* are the same among different treatments (i.e., C-only, SM1, SM2 and SM3), the allocation coefficients (vector *B*) act as the key factor to determine the baseline C residence time. To clarify it, we added "The allocation coefficients act as the key factor to determine the baseline C residence time in this study" in the Lines 164-165.

P8, L177- : Although it is shown in the Table 1, it will be helpful for readers to mention here again the fact that CN ratio in SM2 scheme is fixed, while other two are flexible.

[**Response**] Thanks for your comments. We added "(i.e., fixed C:N ratio in SM2, flexible C:N ratio in SM1 and SM3)" in the Lines 188-189.

P12, L309: Which level of CO_2 concentration do you give to the model in the spin-up? Are the CO_2 concentration and climate forcing in simulations given as a cyclic manner? Please clarify them

[**Response**] We used the CO_2 concentration of 1996-2007 from 361.3 to 382.0 ppmv. Yes, we recycled the CO_2 concentration and climate forcing in simulations to the steady state (more than 1000 cycles for each simulation). To clarify it, we added "In this study, the meteorological forcings of 1996-2007 with the time step of half an hour were used to run the models to the steady state" in the Lines 328-329.

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- *P13, L319: "S^CRT" should be "S^MRT"?*

[**Response**] Thanks for pointing out our mistake. We corrected to "S_i^{MRT}" in this revised version.

- *P15, L390: It looks less references to your figures and tables in the discussion section: It was a bit difficult for me to figure out which claims in the discussion section are supported by your own results.*

[**Response**] Thanks for pointing out this issue. We added more references in the Discussion section. In addition, we added more figures (Figs 5b and 6) to show our results to support the Discussion section. Please also see the responses to the first comment above.

- P15, L405: You mention here that SM1 has a feedback from leaf N concentration to photosynthetic capacity, but eq.(7) seems not. I have overlooked something, but if the SM1 actually has leaf-N concentration feedback, you should touch it in the section 2.1.1.

[**Response**] Thanks for your comments. The plant N demand in the Eq.7 is calculated as:

$$N_{demand} = \frac{C_{leaf}}{CN_{leaf}} + \frac{C_{wood}}{CN_{wood}} + \frac{C_{root}}{CN_{root}}$$

C_{leaf}, C_{wood} and C_{root} are the current time step C pool sizes of plant tissues, CN_{leaf}, CN_{wood} and CN_{root} are the last time step C:N ratio of leaf, wood and root, respectively. To make it clear, we added this equation to Line 200.

P16, L426 "C cost of fixing": Is the effect of C cost actually considered in your simulation of SM3? Which equation in the section 2.2.2 represents the effect? In addition, if you consider the C cost in the SM3 simulation, does the lowest NPP of SM3 attribute to the increase of autotrophic respiration in SM3? It would be nice if you can discuss on this.

[**Response**] Thanks for your comments and suggestions. Yes, we used the same C cost coefficient for N fixation (BNF) in SM1 and SM3. The different values of C investment for N fixation are due to the different strategies between SM1 and SM3, resulting in the different autotrophic respiration and NPP (Figure 3). For SM3, the calculation of BNF used the empirical relationship of BNF with evapotranspiration explicitly, while SM1 represents BNF as an option combining with the plant N uptake as the N source in terms of C investment (Table 1). In other word, plant actively selects the N source on the basis of investment. Our results showed that the strategy in SM1 lead to higher plant NUE than that in SM3 (Figure 5). We added those information in the Discussion section in Lines 472-474 and 494-496.

- P16, L427: I will appreciate if you can add more explanation why BNF of SM1 lead to the highest NUE. In my understanding, if BNF in SM1 works as the complement to nitrogen uptake, the process works to increase the uptake, and then the NUE(=NPP/PNU) should be decreased. I wonder the SM1 has a mechanism to have BNF that satisfy a minimum N requirement by plants, but it was not clear.

[**Response**] Sorry for the confusion. As our response above, SM1 represents BNF as an option combining with the plant N uptake as the N source in terms of C investment. Our results showed that this strategy lead to the highest NUE among three C-N schemes. In order to eliminate confusion, we revised the sentence as "On the other hand, SM1 applied a different strategy, which set BNF as an option when the plant N uptake is not enough in terms of C investment, leading to the highest plant NUE but the lowest response of BNF to NPP".

- P16, L428: Although same N loss process are shared between the schemes, I suppose the original models (TECO-CN/CLM/OCN) actually differ in that point. Readers can get benefit if you can discuss it briefly.

[**Response**] Thanks for your comments and suggestions. We added "In the original CLM4.5 and O-CN (Oleson et al., 2013; Zaehle et al., 2010), soil mineral N pool is divided into two pools (ammonium and nitrate). The leaching is only active on the nitrate pool,

while the ammonium pool is assumed to be unaffected by leaching. This hypothesis may reduce the correlation between leaching and total soil mineral N." in the Lines 478-482.

- P17, L443: You discuss here how CN ratio in SM1 scheme affects the N regulation on plant production processes. As you discuss in the section 4.2, SM3 also has the mechanism of flexible CN ratio. How did the flexibility of SM3 act on plant production processes?

[**Response**] Thanks for pointing out what we have neglected. In this revised version, we added "However, this was not the case for the SM3 since both hypotheses of increasing respiration to remove the excess C under N stress and the higher C investment for the BNF lead to the decrease in C input and then limits the microbial immobilization for the passive SOM pool." in the Lines 498-501.

- P17, L445 "leading to a high microbial N immobilization": I cannot understand why high CN ratio in plant tissues bring models to have a high microbial N immobilization. Need further detail.

[**Response**] Most previous studies showed that litter quality (ie., C:N ratio) could affect the rate of microbial N immobilization (i.e., Zaehle et al., 2014; Thomas et al., 2015). When the fresh litter inputs soil part with higher C:N ratio than SOM, the microbial demand for mineral N increases to maintain the stoichiometry balance itself, which enhances the N immobilization potential. We revised the sentence as "N stress increased litter C:N ratio, leading to a high microbial N immobilization to keep their stoichiometry balance and then a lower net N mineralization…."

-P18, L475 "Fig. 6c" is likely to be "Fig. 7c"?

- P18, L498: Maybe "Fig. 7" is likely to be "Fig. 8".

[**Response**] Thanks for pointing out our mistakes. In this revised version, we added a new figure (i.e., Figure 5) and changed those figure numbers accordingly.

- P18, L484: What does "structural litter quality" mean?

[**Response**] Sorry for the confusion. In the TECO-CN model, based on different decomposability, the plant litter is divided into two parts: metabolic litter and structural litter. Based on our results, we deleted the "structural" in this sentence.

In the analysis, plant production and C/N status are evaluated in steady state. Although I recognize the usefulness of the analysis using steady states, I believe many readers get interested how your conclusions can be extended to non-steady state simulations, because N limitation on C cycle can be intensified in the condition where CO2 concentration increasing. I will be happy if I can see the discussion on this. In addition, displaying N status in the three simulations will be helpful for readers to get the whole picture of the CN dynamics: mineral N is displayed (in Fig.3), but others (plant, litter, and SOM) are not. Since your analysis is based on steady-state, such information can be a support to understand the relationship between N-fluxes and N-pools. My suggestion is to include it in supplement.

[**Response**] Thanks so much for your comments and suggestions. We agree that analysis of N limitation on C cycle on the non-steady state is really interesting and critical. However, it is difficult to simulate ecosystem C processes on the non-steady state. In this study, the traceability analysis method is only for the steady-state simulations. Our next step is to develop a transient traceability analysis for the non-steady state. In this revised version, we added some discussion to show this caveat for the non-steady state in the Lines 452-454 and 469-471.

In addition, we added a new figure (Figure 5, please see above) for the sizes of C pools and C:N ratios according to your and the fist referee's comments. We also added a single figure (please see below) for N pools in supplement. We hope you will find our revision satisfactory.