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Interactive comment on "Decadal impacts of nitrogen additions on temperate forest carbon sinks: A data-model comparison" *by* Susan J. Cheng et al.

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First, please excuse my long response time.

This paper tested a terrestrial ecosystem model with coupled C and N cycles against 13 long-term 15N tracer addition experiments at 8 temperate forest sites. It thus targets a central quantity that has received great attention and has served to synthesize the complex nature of C-N cycle interactions: Where and how much N is retained in the ecosystem? How much C is additionally stored in plants and soil per added N? Model evaluation in this respect is thus a highly welcome exercise. The model in its original version is a widely used global vegetation model and is used as a compoenent

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within a coupled Earth System Model. A careful examination is thus important also for understanding the power and limits of respective ESM simulations (which will be part of CMIP6, I guess). Because this model (CLM5) overestimates N inputs and losses (too open N cycling), a re-configured version is used here, where N cycle openness is drastically reduced by reduced prescribed N deposition, artificially supressed N fixation, and artificially supressed denitrification.

Tracer studies suggest important contribution of recovery by immobilisation. Both model configurations overestimated N recovery in plants. The adjusted model version simulated N recovery in soils ok, while the original version underestimated soil N recovery. Regarding implications for C: the model has too low C:N ratios in wood and thereby underestimates the amount of C sequestered per N added, thus balancing the overestimation in simulated plant N recovery. The relevance of the two quantities (% recovery in different pools, and dC/dN) is high and the model evaluation has promise to yield useful insights into a key mechanism.

Confronting models with data is in general a very important way forward to addressing known unresolved deficiencies in models. The present paper takes a step in that direction. Therefore, I am looking forward to some (major) revisions of the present manuscript and consider that the paper (after revision) may be a valuable contribution to this field and suitable for publication in Biogeosciences. However, I have a few concerns regarding the comparability of observed and modelled quantities which need careful additional explanation and justification. In addition, I consider that the discussion of the insights gained and the recommendations given for further research are too generic and am not convinced that proposed steps will advance the field, or even the performance of the model used here. Looking at the fate of N additions also provides insights into the complex interactions between microbes and different soil compartments (surface, depth, mineral N). However, the complex behaviour of the system also points to difficulties related to the model setup for mimicking the experiments. These limitations should be addressed better and possibly alternatives explored before the paper is re-considered for publication. Having said that, I appreciated the well-written text and clear presentation.

MAJOR

* I'm asking myself about the comparability of simulated N recovery (Eq. 1) and 15N-based results and what underlying assumptions have to be met. One is perfect mixing in the respective pool. It could be that the N added in the field enters the system in such a way as to favour accumulation in the litter by being taken up by microbes (immobilisation), and is never fully mixed within the entire mineral N pool available to the plant. In contrast, in the model it is added directly to the mineral N pool and is well mixed (by design). Once N is immobilised (in nature), this doesn't preclude that additional N (that would otherwise have fuelled immobilisation) is now available to the plant and leads to an increase in overall plant N uptake. However, this N then doesn't carry the signature. If this is indeed a possibility, then one would have to either compare total N stocks in observations analously to Eq. 1. This problem of the field-model comparison is discussed on l.16, p. 15 and it is pointed out that the tracer is added (in the field) to litter layer where immobilisation occurs, whereas in the model it's added to the mineral N pool. The suggested solution to this conceptual mismatch is making the model more complex and I'm not convinced this is a path worth taking. Wouldn't it just do the trick to add the N (in the model) to the litter pool and thus decrease its C:N ratio?

* My other concern/question is that 15N data is compared to simulated N pool size changes. The most direct comparison would be to track a 15N tracer also in the model, but I understand that this is beyond the scope here. But can the two be compared in an experimental setup, where no additional N is added as fertiliser (see entries with 0 g N yr-1 m-2 in Table 1). How is this handled in the model? The denominator in Eq. 1 is zero in this case. In that sense, I'm asking myself about what is the advantage of using tracer studies as opposed to just looking at biomass changes in response to fertilisation. Adding a clearer motivation for the approach chosen here in the intro

and/or discussion may address this question.

* Data availability: In the introduction (p. 4, l.26), it is stated that “we first compiled a summary of 15N recovery data from long- term 15N tracer experiments that can be used to evaluate N cycling in other land models. “ However, the data is not made accessible (only upon request). This aspect seriously hinders progress and I was disappointed to see it here.

* I was a bit disappointed by the discussion where it is suggested that the inclusion of more processes (p.14, l.26) would lead to a better model-data agreement. This is a common “reflex” seen in many publications but often doesn’t resolve model deficiencies that are implied by its “core”, and it doesn’t take into consideration possible shortcomings of the nature of model-data comparison itself. Increasing model complexity often leads to more problems and yet more calls for adding processes. Here, this suggestion is rather disconnected from the findings presented here, and I was disappointed to see it here.

* I am intrigued by the issues of N cycle openness in the CLM5 model and would be interested to learn more about how the results for N fate presented here yield new insights into this problem and how it can be resolved. The model was re-configured here with much reduced N inputs (deposition and fixation) and an unrealistic suppression of denitrification. While this allowed authors to achieve plausible results for N recovery, it gives the impression that a wrong model behaviour was “fixed” by an unrealistic assumption. Does this study yield insights into how this could be resolved better?

* p.9, l.17: The calculation of N residence time: Is this calculated from the transient response or after model spinup? It has to be in steady state in order to calculate residence time = stock / loss flux. The enormous difference in residence time may just be the result of reduced N losses in a transient response.

* Sec. 3.1, p.): Quantification of how often N cycles through the system before it’s lost, as the ratio of whole-ecosystem N residence time to plant N residence time. I am

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suspicious of this calculation. Once it's in the soil (and stays there for a long time), it's not actually cycling. As a more insightful charecterization of the N cycle in the two alternative model configurations, I would recommend to calculate an N cycle openness similar to Cleveland et al. 2013 PNAS as the fraction of new N to total N in new production, or Nin / (rN:C * NPP - Nresorb), which is in steady state equal to Nout / (rN:C * NPP - Nresorb).

* I did not learn much from the analysis by PFT. Is there a hypothesis for why PFTs would differ in their response? This part needs a motivation, otherwise it appears here a "just because we can". It also remains unclear why CLM results differ between these PFTs. What mechanisms are responsible for differences?

* Model and observational results are reported as a mean (?) and a relative standard deviation (?). The SD is calculated from year-to-year variability, but is then treated as a standard error and a p-value is calculated to test for the significance of the difference between observed and modelled values. In my understanding, this is not permissible. The model is not stochastic, thus the variability in model results is not uncertainty. I would recommend to present numbers for the mean relative bias as mean((mod-obs)/mean(obs)).

OTHER POINTS

* I liked the intuitive characterisation of the mode used here: Maybe this could be complemented to provide additional (intuitive) model understanding on the following points: How does foliage C:N and Vcmax (which is predicted separately, I guess) interact? What drives what? How is allocation (shoot:root ratio) simulated? How are N losses simulated (scaling with a mineral N pool, or with the mineralisation flux, or. . .)? What happens to C paid as C cost for N uptake (respired as CO2)? Can the litter-soil transfer be N limited (slowing decomposition rates)? A word more on stoichiometry optimisation applied here? Is there a feedback between plant belowground C investments and SOM decomposition rates?

* In the introduction, it may be worth citing Magnani et al., 2007 and the debate on realistic dC/dN values that ensued in response.

* Fig. 2: Unclear what bars represent. Are these the observations? If not, I highly recommend adding observations here as well. Missing info in caption. What is meant by "scenario"?

* It is argued that results for only Harvard forest are shown because it provides the longest record length. I would find it highly revealing if Fig. 2 could include results for other sites too.

SPECIFIC

* p.3, l.6: Add exchanged between the land surface and the atmosphere under future scenarios of increasing CO_2 and climate change.

* p.3, l.31: A soil C:N of 5 seems very low. Where did you get this number from?

* p.6, l.7: GSWP3 data: Didn't find any data under the URL given.

* p.6, l.12: Using locally measured meteorology, elevation, soil parameters, at the experimental sites? This could be quite important.

* p.7, l.16: Equally spread across days?

* p.7, l.18: Site names appear here before being introduced. Would be worth having a short paragraph at the beginning of Methods about the locations and characteristics of sites.

* p.9, l.16: Change "equivalent" to "similar in magnitude".

* p.9, l.17: Add *N* residence time.

* p.11, l.14: Better write "per unit N tracer added" (if that's correct).

* p.11, l.18: Why not use values measured in the field? Not available? What literature?

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