

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

Anonymous Referee #1

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Cheng et al. present a model-data comparison of ecosystem N recovery at several temperate forest sites, using two versions of the CLM5 land model and a number of 15N tracer field experiments. They also use the results to give estimates of ecosystem C storage responses to changes in N availability. The paper is very well written and thoroughly describes the conducted work in good detail. I particularly like the design of Figure 2 to visualize the model-data aspect. The study design is relevant, as C-N models are best informed by field experiments that include measurement estimates of both C and N processes. It is also an advance over other model studies such as Meyerholt & Zaehle (2015), where only one site was used for a model-data comparison of ecosystem N recovery. However, I am not convinced that this study is a good fit for BG,

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as it suffers from model limitations that make the results not meaningful to a general biogeoscientific audience, whereas the study has interesting insights for the land modelling community that are laid out in the discussion (e.g. section 4.1). Therefore, I see the paper as a fit for e.g. GMD, but cannot recommend publication in BG.

The main issue I see is that neither the standard or the adjusted version of CLM5 used here appear capable of simulating plausible C and N cycle representations, the conclusion being that they cannot presently be used to give e.g. meaningful estimates of C sink responses to N change. The authors are aware of this for the standard version and fully describe the changes they made to come up with the adjusted version that is supposed to be a better fit for the site selection. To my understanding, however, they fix a hole (unrealistic equilibrium C and N stocks) by creating another one (e.g. eliminating "denitrification"). This fixes some site specific measures, but it also creates a C-N model without a plausible N cycle. Apparently on average, live wood C:N ratios are at the level of foliage (Table 2)? The adjusted ecosystem N residence time appears rather arbitrary with a huge range (p9118). Also, although the presentation is commendably thorough, the model formulations of key N processes are not clearly given. Since this is central to what we can expect the model to do as far as N, they deserve explicit description beyond reference to other studies (in particular, Lawrence et al. 2018 is not listed in the references). At this state it is not clear how N fixation is calculated - the model uses FUN, but Shi et al. describe that FUN is only used to determine the partitioning between uptake and symb BNF, whereas total N input uses CLM4 standard? So is BNF still NPP-based? Similarly, it is not clear how loss fluxes are determined in this study - but ecosystem N inputs and outputs a central to how N recovery is calculated. In my opinion, these model-related problems push the study more towards a well-presented model-tuning exercise - which is not bad at all and definitely needed for model evaluation, but not relevant to the broader BG readership. To this end, I disagree with the authors on some of the early discussion points: "Our study provides insight into which model assumptions are consistent, or inconsistent, with experimental results." (p13113) - So if fixed ecosystem N inputs, unrealistic C:N

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ratios and the elimination of gaseous N losses lead to a number that is consistent with experiments, does that make the assumptions correct?

Other things:

- I think the title should state that the study is about N recovery rather than C sinks. Also, I understand "decadal" to mean decade-by-decade, rather than "some experiments last over 10 years".

- Abstract p2l11 ff: It appears that for longer timescales, model plants did not acquire more than twice the experimental N recovered (23% vs 13%).

- very minor point, I was a bit confused by the order of in-text citations for multiple references in the same bracket. Since the order is not by year or by name, it is by relevance? But this can't be the case for the citations in p3l12f.?

- p6l25: There is a double "g N" in the middle.

Reference: Meyerholt, J. & Zaehle, S. The role of stoichiometric flexibility in modelling forest ecosystem responses to nitrogen fertilization. *New Phytol.* 208, 1042-1055, doi:10.1111/nph.13547 (2015).

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