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Interactive comment on "Nitrogen deposition: how important is it for global terrestrial carbon uptake?" *by* G. Bala et al.

Anonymous Referee #1

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The basic idea of the paper is that ecosystem carbon accumulation is constrained by nutrients, particularly nitrogen, and the enhanced nitrogen deposition due to fossil fuel combustion can help to elevate the ecosystem N uptake. This can lead to significant carbon storage in recipient ecosystems. In order to show the importance of N deposition for global terrestrial C uptake, authors have performed idealized model experiments using the Community Land Model 4.0 (CLM4). I don't think there is anything that is substantively wrong with the work presented in this manuscript. I think this paper has a lot of potentially interesting results, but they need to be pitched well.

I have 3 major concerns with this paper.

(1) The details of the method presented and the discussion of the results, as written, are quite spars. There is a lot of "what", but no "why". Authors have written quite nice

summary of what is on the figures, but they also need to do interpretation of the figures.

(2) The model analysis is emphasizing more on the quantitative prediction, but I find the qualitative results much more compelling than the quantitative predictions. It is now well establish that N is a limiting nutrient. So, the question is not whether nitrogen is limiting to primary production of specific PFT, but for how long, and why, and where. The authors should do more thorough modeling analysis to address these questions. In addition, it is not clear how the model is handling a number of important feedback processes that are crucial to understand the impact of N deposition on carbon and nitrogen dynamics. For example, greenhouse-induced increased temperatures would cause increased decomposition, increased nitrogen mineralization, and hence increased primary production in some regions - thereby offsetting some of the increased carbon release from soils that could otherwise provide a positive feedback to global warming. But authors have nowhere mention about the model response to this important feedback. Similarly, N limitation is also constrained litter decomposition and hence mineral N. The manuscript only describe the effect of N deposition effect on plant N uptake, but have not discussed how the N deposition effects litter decomposition in the model.

(3) Some of the results presented are not consistent with the existing knowledge and data. For example, the model results presented here suggests that 12–17% of the deposited nitrogen is assimilated into the ecosystem. First, it is nowhere discussed why only 12-17%, but most importantly how the model results presented here are compared with previous analysis. Previous analysis suggests that maximum of 10% of the deposited nitrogen supports increased carbon storage. Also, the 15N tracer experiment analysis suggests that the effect of increased N deposition on global forests C sequestration was about 0.25 PgC/yr. But the model results presented here suggests that N deposition effect on C uptake is many times higher than results based on 15N analysis. The discrepancies between the results presented in this manuscript and published results warrant comments by the authors.

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Other Comments:

Page 11078, Lines 12-17: It is nowhere discussed how the different estimated numbers (242, 275 and 153) appearing in this statement are calculated. As stated, these results are based on sensitivity experiments and there are number of experiments performed in this analysis. It is important to state here as to which particular sensitivity experiments these results pertain?

Page 11078, Lines 16-20: I also disagree with this statement. Yes, since preindustrial times terrestrial carbon losses due to warming has partly compensated by effects of increased N deposition, but not of the same amount as calculated in this study. Based on the 15N studies, only about 0.5 Pg C/ yr of the current annual terrestrial carbon sink is a result of nitrogen deposition. But the amount calculated based on this modeling study is many times many times more than the amount estimated based on 15N analysis. Also, 0.5 Pg C/ yr is for the recent years, the estimated amount over the historical time would have been much less than this amount.

Page 11080, lines 3-6: What is the basis of this statement? True, the amount of additional carbon stock increase depends primarily on the C:N ratio, but it also depends on pft type, location and environmental conditions. I believe, this analysis make an assumption that 100% of the N deposition can be satisfied by the N deposition input, but nitrogen deposition does not necessarily occur where nitrogen most strongly limits net primary production.

Page 11080, lines 26-28: I disagree with your statement. As you have shown in Table S1, number of measurement studies have addressed this question and all the modeling analyses, including CLM model, which is used in the current analysis, have compared there model results with these measurement. Having said that, I think this paper has a lot of potentially interesting stuff but it needs to be pitched well.

Pages 11081-1083: It is stated that model experiments are based on near-equilibrium simulations as opposed to transient simulations. Then it is stated that the model is

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forced by a 57 yr (1948–2004) observationally constrained atmospheric forcing dataset at a three-hourly intervals. But later on it is stated that twelve 1000 yr simulations with the same climate forcing are performed. Much further in the text it is also stated that “All twelve simulations start from a spun up pre-industrial state and changes in N deposition, CO2 and climate warming are imposed as step-function changes at the start of the simulations. The 57 yr atmospheric forcing dataset is repeatedly used in all twelve 1000 yr experiments.”

It is not clear what exactly has been done in this analysis. Suggest that authors should clearly define the following (1) how the near equilibrium experiments are performed with transient data, (2) what is the pre-industrial spin-up when there is no pre-industrial climate data, also the pft distribution is based on the present day vegetation cover and (3) what is the difference between spin-up run and perturbation runs?

Page 11083, lines26-27: It is stated that “The spatial pattern of N deposition used in our experiments based on pre-industrial N deposition is similar to present day deposition (Fig. 1)” What is the basis for this statement?

Page 11084, lines 10-14: This is not surprising. It will be good to discuss which pft are showing N limitations and which are not?

Pages 11084, lines 26-28; 11085, lines 1-5: First, what is the scientific basis for these arguments that biological N fixation is saturated at higher NPP and N fixation is limited by other nutrient, such as P. Why limitation at higher NPP level? It is well known fact that P and other nutrient co-limit BNF at all level. Also, BNF rates are reduced when plants are grown in soils with high amount of available nitrogen. This is all our theoretical understanding, but the model may not have included all these effects. It would be good to separate the BNF effect from the N deposition effect first and then evaluate the effect of different rates of N deposition on the N uptake rates.

On page 11085, lines 13-18: it is stated that “Our model-based estimate is conservative when compared to observations in European sites which find a carbon sequestration range of 5–75 kgCper kgN for forests and heartlands and a most common range of 20–40 kgCkg−1N−1 (de Vries et al., 2009) or US sites which find aboveground biomass increment of 61 kg of carbon per kg of nitrogen deposited (Thomas et al., 2010)."

I disagree with this statement, because authors are comparing apple with oranges. The model calculations are for the global mean case, that is average effect of the all 15 pfts, but the measurement results cited are only for the forest pfts. It is not surprising that the N deposition effect for the forests is higher than modeled global mean case, because forests have large C-storage capacity. Therefore, I again suggest comparing the N deposition effect for different PFTs.

Page 11085, lines 2-22: It is stated that "for our equilibrium simulations only 12–17% of the deposited Nitrogen is assimilated into the ecosystem". It will be good to discuss where the rest of the N deposited amount has gone?

Page 11085, last paragraph: The results for 2K warming case in Figure 3 are interesting, but authors do not provide any explanation as to why the results for all variables are decreasing with time. Authors have rightly said that under 2K warming soil decomposition will be enhanced. As a result of this, TEC will be reduced, but it also good to discuss the fate of soil mineral N under 2 K warming case. I expect that amount of inorganic N in soil to increase through enhanced mineralization associated with decomposition under 2 K warming cases. Discuss how the enhanced amount of mineral N impacts the NPP of different PFTs?

Interactive comment on Biogeosciences Discuss., 10, 11077, 2013.