Biogeosciences
Open Access
Discussions

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

G. Bala et al.

dev@caos.iisc.ernet.in

Received and published: 4 October 2013

Referee #1 Comments:

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.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Response:

We thank referee#1 for appreciating the results of our paper and for the useful suggestions, which have improved the manuscript substantially. Please see below the point by point responses. (Note: Please also see the attached pdf contains revised manuscript and revised supplemental material as a supplement to this comment)

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 sparse. 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.

Thanks for the suggestion. In the revised manuscript, we provide elaborate discussion of the figures when we address the referee#1's many comments below.

(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 established 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.

Good points. As we discussed in the introduction section, as per the reviewer's observation, this study is not about whether N is a limiting nutrient or not. This study is about the effect of increased N deposition in the industrial era. The spatial pattern of the effect of N deposition on carbon uptake is illustrated in Figs. 4 and 8.

Thanks for the suggestion on warming induced decomposition. We have now included a new Figure (S1) that shows the time evolution of soil N mineral changes relative to the control for all experiments. We have now discussed the effect of N deposition on decomposition under warmer climate in the 6th paragraph, results and discussion section of the revised manuscript. The following are the lines: "Warming is expected to cause increased decomposition, increased nitrogen mineralization, and hence increased primary production - thereby offsetting some of the increased carbon release from soils that could otherwise provide a positive feedback to global warming. The model does simulate larger soil mineral nitrogen per unit soil carbon for 2K warming (Fig. S1). However, we find that the total amount of mineral nitrogen declines in the warming cases (Fig. S1) because the amount of soil carbon is smaller due to decline in ecosystem productivity (Fig. 2) in the 2K warming cases. This is consistent with declines in TEC in these cases (Fig. 3)."

(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.

We appreciate the reviewer's comments here. In the revised manuscript, in the 4th para of section 4, we write ".…..Carbon and nitrogen flow in parallel between vegetation, litter and soil organic matter respecting the stoichiometry of the various organic matter pools. In CLM4, the C:N ratio for leaf, wood, root and soil pools are 30:1, 130:1, 55:1 and 10:1, respectively. When carbon stocks are weighted with the fraction of carbon and nitrogen in these pools in the 1N case, we find an average C:N ratio of about 20:1 which is consistent with the approximate ratio of TEC to TEN in Table 1. Therefore, when N deposition is increased by 20.3 TgN/yr (2N-1N), we find an increase in TEN of 3.4 PgN and an associated TEC increase of 69 PgC."

Further, we have now discussed in the 5th paragraph ".…..….and the remaining N lost to atmosphere through denitrification, fire loss and leaching (Fig.5)."

There is confusion about the N deposition effect on global forests carbon stocks because the reviewer's unit (PgC/yr) and our units (PgC / (TgN/yr)) are not the same. If we convert our value to the units of the reviewer, we obtain a value (0.17 PgC/yr) that is about the same as quoted by the reviewer (0.25 PgC/yr). When we discuss δL in the results section, we add now "Our value of δL is consistent with a previous study (Nadelhoffer et .al. 1999) which suggests a carbon sequestration of 0.25 PgC/yr from increase in N deposition: our value of 3.41 PgC/(TgN/yr) over 1000 years translates to 0.17 PgC/yr for an increase in N deposition of about 50 TgN/yr since the pre-industrial period.

Other Comments:

Page 11078, Lines 12-17: It is nowhere discussed how the different estimated numbers (242, 175 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?

Thanks for the pointer. In the revised manuscript, we have now mentioned the ex-

periments used to find the sensitivity numbers in the 9th paragraph of Results and Discussion section. Also, in the 1st paragraph of conclusions section, we show the calculation for the numbers 242, 175 and 153 in parentheses.

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.

We appreciate the reviewer's comments here. This comment is closely related to the reviewer's 3rd major concern. Please refer to our response to that comment above.

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.

We appreciate and agree. We change the sentence to "While the amount of additional carbon stock increase depends on the plant functional type, location, type of ecosystem (N limited or not) and other environmental conditions, the upper bound can be estimated from the C:N ratio of the ecosystems: . . . ."

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.

We agree. In the revised manuscript, we delete this statement.

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 forced by a 57yr (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 started 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?

We appreciate the reviewer for these comments on our experimental methods. In the revised manuscript, the 3 issues raised by the reviewer are addressed. Section 3 now starts like this: "CLM4 simulations in this study are started from a well spun up state (restart files supplied along with source code by NCAR) corresponding to pre-industrial levels of atmospheric CO2 concentration at285 ppm and N deposition (20.3TgN/yr). When we continue this case for 1000 years (the control experiment 1N as discussed below) the drift in global total ecosystem carbon is only 0.015 PgC per year, suggesting that the control simulation is in near-equilibrium state. From this well spun-up prein-dustrial state, we initiate twelve 1000-year simulations with the same climate forcing but varying N- deposition, CO2 concentrations…………….." The second para of this section now reads "In the above simulations, changes in N deposition, CO2 and cli-mate warming are imposed as step-function changes at the start of the simulations. It

should be noted that we refer to 1N as pre-industrial control though we use vegetation types corresponding to present day and use a 57-year forcing dataset that corresponds to 1948-2005. During the last 100-year period, net ecosystem exchange (NEE) has a magnitude between 0.01 and 0.1 PgC/yr in all the simulations and hence considered to be in near-steady state. The 57-year atmospheric forcing dataset is repeatedly used in all twelve 1000-yr experiments. The influence of the 57-year cycle or short term trend in our simulations is removed by either applying a 57-year running average or by showing differences between the experiments (subtraction one experiment from another removes the 57-year trends cycles) in our analysis."

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?

Thanks for this comment. In the revised manuscript, we quantify this by stating the value of correlation (0.87) in this sentence.

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?

We appreciate this comment. In the revised manuscript, when discussing Fig. 4, we write "Most of the increase in TEC is located in regions where trees are the dominant plant functional types"

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.

We appreciate the reviewer for detecting the incorrect implication of our sentence. We have rephrased the sentence as below:

"The sensitivity for N deposition decreases at higher N deposition levels in real world because of other factors such as water or availability of other nutrients especially phosphorous would eventually limit ecosystem productivity. These limitations are represented in the model by parameterizing biological nitrogen fixation (BNF; an input of N to terrestrial ecosystem) as BNF = 1.8(1-exp [-0.003NPP]) This formulation captures the observed broad-scale dependency of BNF on ecosystem productivity (Oleson et al., 2010)."

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 kgC/kgN (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.

We appreciate and agree with the reviewer. After this sentence, as per the reviewer's suggestion, we write "Our model calculations presented here are for the global mean case, that is average from all 15 pfts, but the measurement results cited are only for the forest pfts. Therefore, 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." The suggestion of the reviewer to investigate the PFT wise sequestration of carbon due to N deposition is appreciated but the main focus of the paper is on global scale. We defer a detailed investigation of PFT wise sequestration and regional analysis to a

future paper. In the 5th paragraph of the conclusion section we add "Since the main focus of the paper is on global scale, we have not studied the pft wise carbon sequestration in detail here. We have not also performed regional analysis of carbon uptake due to N deposition."

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?

Agreed. In the revised manuscript, at the end of that paragraph, we now add "and the remaining N is lost to atmosphere through denitrification, fire loss and leaching (Fig.5)."

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 2K warming cases. Discuss how the enhanced amount of mineral N impacts the NPP of different PFTs?

This is discussed in reply to your major concern#2 above.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C5709/2013/bgd-10-C5709-2013-supplement.pdf

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