Biogeosciences Discuss., 10, C5706–C5708, 2013
www.biogeosciences-discuss.net/10/C5706/2013/

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

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Received and published: 4 October 2013

Referee #2 Comments:

Bala et al. report on the results of a global simulation study with an off-line land ecosystem model which tries to disentangle the sensitivities of terrestrial ecosystem carbon(TEC) to changes in nitrogen deposition, rising atmospheric CO2 concentration and temperature. Their main finding is that since the pre-industrial period nitrogen deposition had a positive effect that was counteracted by warming and that increases in TEC were due to rising CO2. The paper is highly relevant as it provides, within the limits of the chosen approach, first data on the relative importance of these three drivers. The topic of the paper fully fits with the objectives of BG and the paper is mostly well written and the presentation is excellent. I thus recommend the paper for publication

once the following minor issues have been tackled.

Response:

We thank the reviewer for recommending our work for publication in Biogeosciences. The minor issues are addressed below.

(Note: Also please see the attached pdf contains revised manuscript & revised supplemental material as a supplement to this comment)

Minor comments:

(1) p. 11079, l. 20: the CO2 fertilization effect may be "well documented" in the (global) modeling world, however among experimentalists the degree to which elevated CO2 causes increases in plant carbon uptake is much more controversial,with some authors suggesting that mature ecosystems with carbon and nutrient cycling in equilibrium should and are not sensitive to elevated CO2 (see Körner 2006New Phytologist 172, 393- for an excellent review). Please modify the text to reflect this discrepancy between modeling and experimental world.

We agree with the reviewer. We have modified the text as below. We now write "There have been many observational studies on CO2 fertilization which yield a range of results (Körner, 2006). For example, free-air CO2 enrichment experiments in forest stands (Norby et al. 2005) indicate a 23% median increase in net primary production (NPP) in response to a CO2 concentration increase from 376 to 550ppm (parts per million) but some studies (Newingham et al. 2013) show no significant effect. It appears that the effect strongly depends on the availability of nutrients and soil water, the plant species and state (young vs. mature) of the ecosystems (Körner, 2006).

(2) p. 11083, l. 25: section 4 is actually more a combined "Results and Discussion" section and should be name accordingly, while section 5 should be name "Conclusions".

As suggested by the reviewer, we renamed section 4 as Results and Discussion, sec-

tion 5 as Conclusions in the revised manuscript.

(3) p. 11084, l. 11-13: in my view this demonstrates in the first place the assumptions underlying the model

We agree. We changed the sentence to “It demonstrates that the model adequately represents the N limitation in the terrestrial ecosystems (Vitousek and Howarth, 1991) as addition of N deposition results in increase in simulated NPP.”

(4) p. 11089, l. 6: here and already before in the discussion of the sensitivity parametersI was wondering whether an ANOVA on the model results would not be a suitable tool to tease apart main effects and interaction terms; see Galbraith et al. (2010; New-Phytologist 187, 647-) for a nice example.

We appreciate the reviewer’s suggestion of using ANOVA for quantifying the main effects and interaction terms. However, we have quantified the main individual effects and interaction terms (Table S3) without the use of ANOVA in the present manuscript. The usage of ANOVA is beyond the scope of this paper. We intend to use the ANOVA method in our future investigations. We have discussed in a separate paragraph in the last section of the revised manuscript.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C5706/2013/bgd-10-C5706-2013-supplement.pdf

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