Anonymous
Referee
#1

## General Comments

The study employs a highly process based ecosystem model to simulate three forest FACE experiments in the US. The study makes excellent use of the model to test competing hypotheses on the mechanisms that could have increased rates of N cycling or retention necessary to sustain NPP responses to elevated CO2.

The abstract states that algorithms representing the N cycling hypotheses were tested at all three simulated FACE sites but results presenting the simulations for each hypothesis are only shown for the Duke experiment. The study also investigates the effect of temperature, precipitation and plant functional types (PFTs) on the elevated Ca response but no clear hypotheses are stated. Again, the last sentence of the introduction is not true given that the four N cycling hypotheses were not tested at each site, so were not tested with contrasting climates or PFTs.

I strongly suggest that the four N cycling hypotheses are tested at all three sites and that the analysis of the NPP response to elevated Ca in interaction with precipitation, temperature and PFT is either; 1) refocussed on the impact of temperature, precipitation and PFT on N cycling in interaction with Ca or, 2) removed from the manuscript altogether.

In my opinion the paper will receive many more citations if the N cycling hypotheses are tested at each site and the rest of the paper more focussed around the N cycling hypotheses. The conclusions would be more complete and it would be interesting to test if the decline in the response of NPP to elevated Ca at Oak Ridge can be explained with the removal of priming. Many of the comments below are with a more focussed manuscript in mind.

*R: All four N cycling hypotheses have now been tested at all 3 sites (now Fig. 13) with additional text in the Results on p. 27 and in the Discussion. Sections on the effects of precipitation and temperature on NPP have been removed, along with the former Figs. 4 and 15 in which these effects were shown. Results and Discussion are now confined to N effects on NPP under elevated Ca as summarized in the 4 hypotheses.*

## Specific Comments

### Are the scientific methods and assumptions valid and clearly outlined?

The methods are generally well described and it is nice to see full model equations in the supplementary information along with good descriptions of the important model processes in the main text.

Description of how the model treats tissue C:N and C:P stoichiometry (Section 2.2 would be appropriate) would aid interpretation of the results. Specifically, how are tissue C:N ratios determined? And how does tissue stoichiometry limit tissue growth? I spent a while trying to find explanation of these mechanisms but they are not described in the main text or appendices. The maximum and minimum leaf N and P concentrations (Nleaf, N'leaf, Pleaf, P'leaf; Appendix C), in ecosys are they really the same for both N and P? Leaf P is usually around an order of magnitude lower than N.

*R. The coupling of structural and nonstructural C, N and P to calculate $CO_2$ fixation is more fully described in Appendix C of the Supplement, as now stated on p. 10. Max. and min. Leaf P concentrations are set to 0.1 those of N.*

It would be helpful for the reader to differentiate between the simulations by including key PFT, soil and other parameters that differ across the three sites in a table in section 3. Also, when were the processes representing the different hypotheses disabled, from the beginning of the simulations or just the start of the experiments? Presumably it was at the beginning of the Ca elevation and only in the elevated Ca treatment but this needs to be stated.

*R. That is correct, as now stated on p. 13.*

Are the results sufficient to support the interpretations and conclusions? The main conclusion that priming was the explanation for sustained NPP responses was only tested at Duke and the analysis of the deeper root hypothesis should be more complete.

*R. The responses have now been tested at all three sites, including the deeper root hypothesis, in Fig. 13 and related text. These additional tests further corroborate the hypotheses.*

At Duke the large drop in the non-structural (sigma) N pool under elevated Ca 5.5 g m-2 is intriguing. This is touched on in the results but explanation of the mechanism is lacking. How does the priming hypothesis as implemented in the model interact with this labile pool to sustain NPP and why do the other hypotheses not allow this non-structural N pool to be reduced? The reduction in non-structural N suggests that growth could be C or P limited under ambient Ca. Why do the other sites not have this labile N pool in the ambient treatment. It would be useful for the ecosystem C and N breakdown in Table 1 to be presented for the simulations of each hypothesis tested. I would like to see some consideration of stoichiometry; how this changes in ecosys in response to elevated Ca and it's contribution to the NPP response. Hypothesis three (greater N uptake form deeper and denser root growth) is not tested in the same way as the other hypotheses; this has been acknowledged in the methods and it would be difficult to test with the model but a better attempt could be made to analyse results with regards to this hypothesis.

*R: Reasons for the drop in nonstructural N is now more fully described on p. 17. It may be an artifact of the sudden onset of elevated Ca in the experiment.*

*R: All PFTs in the model maintain reserves of nonstructural C, N and P to hedge against adverse growing conditions. Annual variation in these reserves is much greater for deciduous vs coniferous PFTs because deciduous PFTs use more N and P for foliar growth in spring and lose more N and P from foliar senescence in autumn. They therefore maintain lower reserves that can be drawn upon if N becomes more limiting under elevated Ca, as seen in Tables 2 and 3 vs. Table 1. This is now clarified on p. 21.*

*R. A test of the deeper root hypothesis is now included, although results did not indicate a clear effect because of other interacting processes as stated on p. 30.*

Figures 3c and 13c suggest root growth was greater but not necessarily deeper at Duke and Rhinelander. Figures 8c and 10a do not convince me that simulated root mass was substantially greater or deeper at Oak Ridge under elevated Ca. The PFT analysis aims is similar to that of (Franklin et al., 2009). However, Franklin's hypotheses that the difference between Duke and Oak Ridge was due to differences in root turnover and C:N ratios could not be assessed in this study as it appears that these parameters were the same for both PFT's.

*R: Yes, the effect of PFT in the model was attributed more to nutrient turnover vs. retention in the canopy rather than in the roots, as now further explained on p. 23.*

## Is the overall presentation well structured and clear?

The paper would benefit from better structure, mainly because the study is assessing too many hypotheses. The results could be more concise and would benefit from a clearer focus. The discussion lacks synthesis both of the simulations across the different sites and to some extent with previous studies. The discussion needs to be more integrated and refined, acknowledging difference from other studies and limitations of the study. For example, (Drake et al., 2011) hypothesise priming from root exudation, not increased litter inputs and microbial mass transfer between decomposition pools. This is important because root exudation is an active plant process while litterfall is more passive. The limitation with regards to testing of hypothesis three should be discussed.

*R: In the model, both exudation and litterfall were raised under elevated Ca, and hence drove priming, as stated at the beginning of the Discussion. Exudation as proposed by Drake et al, as well as litter, will necessarily drive priming by increasing microbial activity and hence in the rhizosphere as modelled in this study, so this distinction between exudates and litter in priming is not significant.*

*R: The sections on temperature and precipitation effects have been removed from the Discussion so that it now focusses exclusively on the 4 hypotheses given in the Introduction. References are made in the Discussion to several other studies which further corroborate these hypotheses as tested at the three sites.*

The manuscript ends very abruptly and needs some concluding remarks and recommendations for future work.

*R: A Conclusion section has been added to p. 32 summarizing the importance of the findings in this paper and recommending future work.*

'Model Application' would be a better title for section 3. The sentence on pp6787 ln3-5 should be part of the initial paragraph in section 3. Section 3.4 would be more logically placed at the end of section 2.

There are too many figures and they should be consolidated on analysis of the primary hypotheses. Figures 4, 11 and 15 are unnecessary. Figures 3, 8 and 13 could be condensed to show root mass for all three sites on the same figure (the other tissue growth increments should go in the supplementary information).

*Figures 4 and 15 have been removed. I wish to retain Figs 3, 8 and 13 (now Figs. 3, 6 and 11) because they are explicit model test of how rises in NPP are allocated to different forest components.*

Error bars on the figure should be explained. To compare with model results they should be measures of uncertainty and so should be standard errors or confidence intervals, not standard deviations.

*R: These error bars were taken from the cited literature as were the data to which they refer. I would prefer not to alter them as they are not my data.*

Figure 9: all data should be on the same y-axis.

*R: Then comparison of the relative magnitudes and time trends of the modelled vs measured data would be less clear.*

### Is the language fluent and precise?

The language in places is difficult to follow and the paper would benefit from general editing for clarity and precision. For example, the final paragraph of the introduction is not very clear, nor concise:

'. . . was assessed by testing output from algorithms for these processes in ecosys against changes in forest N and C cycling under elevated vs. ambient Ca mea-sured or calculated over several years from ...',

I suggest:

'. . . was assessed by comparing simulated responses of forest N and C cycling to elevated Ca with multi-year observations from ...'

*R: This sentence has been simplified.*

pp6792 ln3 'Each of the four changes in N processes described above contributes to maintaining foliar N vs. C under elevated Ca , '. What 'changes'? Responses to elevated Ca would be more accurate.

*R: This sentence has been rephrased.*

pp6797 ln22 'These increases in NPP were driven by ones in . . :' suggest 'These increases in NPP were driven by increases in ...'

In places 'the model' is referred to, it would be clearer in some cases to refer to the model as 'ecosys' (e.g. pp6793 ln3, pp6799 ln9).

*R: OK*

## Are the number and quality of references appropriate?

Some statements that should be referenced are not (e.g. pp6785 ln2,10); use of more up-to-date references would be good and some citations do not altogether support the statements to which they are referenced.

Pp6785 ln 8 (McCarthy et al., 2010) could also be cited along with (Oren et al., 2001) and for the above statement regarding the interaction of water limitation and elevated Ca.

Hypothesis 2 on pp6785 In the introduction (Hofmockel and Schlesinger, 2007) are cited to say that hypothesis 2 has been observed experimentally but later (Section 5.1.2) (Hofmockel and Schlesinger, 2007) are cited to support hypothesis 1and they did not observe hypothesis 2 experimentally.

*R: Yes, but they proposed that hypothesis 2 exists.*

Pp6786 ln7 (Iversen, 2010) is cited to support the fact that models do not represent processes that govern the four hypotheses but (Iversen, 2010) only states that models do not resolve soil processes (decomposition and root N uptake) by depth.

*R: 'represent' in this sentence was changed to 'resolve'*

Drake, J. E., Gallet-Budynek, A., Hofmockel, K. S., Bernhardt, E. S., Billings, S. A., Jackson, R. B., Johnsen, K. S., Lichter, J., McCarthy, H. R., McCormack, M. L., Moore, D. J. P., Oren, R., Palmroth, S., Phillips, R. P., Pippen, J. S., Pritchard, S. G., Treseder, K. K., Schlesinger, W. H., DeLucia, E. H. and Finzi, A. C.: Increases in the flux of carbon belowground stimulate nitrogen uptake and sustain the long-term enhancement of forest productivity under elevated CO(2), Ecol. Lett., 14(4), 349–357, doi:10.1111/j.14610248.2011.01593. x, 2011. Franklin, O., McMurtrie, R. E., Iversen, C. M., Crous, K. Y., Finzi, A. C., Tissue, D. T., Ellsworth, D. S., Oren, R. and Norby, R. J.: Forest fine-root production and nitrogen use under elevated CO(2): contrasting responses in evergreen and deciduous trees explained by a common principle, Glob. Change Biol., 15(1), 132– 144, doi:10.1111/j.1365-2486.2008.01710.x, 2009. Hofmockel, K. S. and Schlesinger, W. H.: Carbon dioxide effects on heterotrophic dinitrogen fixation in a temperate pine forest, Soil Sci. Soc. Am. J., 71(1), 140–144, doi:10.2136/sssaj2006.0110, 2007. Iversen, C. M.: Digging deeper: fine-root responses to rising atmospheric CO(2) concentration in forested ecosystems, New Phytol., 186(2), 346–357, doi:10.1111/j.14698137.2009.03122. x, 2010. McCarthy, H. R., Oren, R., Johnsen, K. H., Gallet-Budynek, A., Pritchard, S. G., Cook, C. W., LaDeau, S. L., Jackson, R. B. and Finzi, A. C.: Re-assessment of plant carbon dynamics at the Duke free-air CO(2) enrichment site: interactions of atmospheric CO(2) with nitrogen and water availability over stand development, New Phytol., 185(2), 514–528, doi:10.1111/j.1469-8137.2009.03078.x, 2010. Oren, R., Ellsworth, D. S., Johnsen, K. H., Phillips, N., Ewers, B. E., Maier, C., Schafer, K., V. R., McCarthy, H.,

Hendrey, G., McNulty, S. G. and Katul, G. G.: Soil fertility limits carbon sequestration by forest ecosystems in a CO2-enriched atmosphere, Nature, 411(6836), 469–472, 2001.

*R: These papers are already cited.*