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Interactive comment on "Modelling changes in nitrogen cycling to sustain increases in forest productivity under elevated atmospheric CO_2 and contrasting site conditions" *by* R. F. Grant

Anonymous Referee #2

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The work makes use of a fairly sophisticated ecosystem model, that captures, among other, effects of carbon priming on microbial carbon and nutrient mineralization, effects of nitrogen translocation during plant tissue senescence and effects non-symbiotic dinitrogen fixation response. All these mechanisms are important pathways for plants and ecosystems to acquire nitrogen, and therefore allow maintenance of tissue stoichiometry if elevated CO2 leads to higher photosynthesis rates. Among these possible ecosystem coping mechanisms to mitigate a possible down-regulation of plant productivity and growth, the author found for three sites of free air CO2 enrichment experiments (FACE) the priming effect to be the most important factor to sustain elevated productivity under elevated atmospheric CO2.

The paper gives a very detailed insight into the modeling setup, in justifying physiological relationships and how they are resolved within the model "ecosys". Such introductory text is very insightful for the reader. The introduction clearly lays out how the work is motivated and makes a case that such model explorations are worthwile doing and allow critical insights. The results are nicely presented and make use of the pertinent literature of the measurements at the FACE site for interpretation. Overall, I believe this manuscript is of great interest of the reader of Biogeoscience Discussions since resolving nitrogen limitation arising from CO2 fertilization is a very important topic in global change research, and this paper adds nicely to this discussion. Also I want to congratulate the author on a very carefully crafted manuscript: next to a great readability and organization, I did not find a single typo.

A couple of things that I believe should be addressed a bit more clearly:

1) There is little citation (next to the work of the author), of how other ecosystem model deal and predict the response at the free air CO2 enrichment sites. A number of models have been applied to the FACE experiments, and the difference among models and their interpretation should be taken into account. References here are - Xu, T., L. White, D. Hui, and Y. Luo. 2006. Probabilistic inversion of a terrestrial ecosystem model: Analysis of uncertainty in parameter estimation and model prediction. Global Biogeochemical Cycles 20: DOI: 10.1029/2005GB002468 - De Kauwe et al. 2013. Water use and water use efficiency at elevated CO2: a model-data intercomparison at two contrasting temperate forest FACE sites. Global Change Biology 19, 1759-1779 DOI: 10.1111/gcb.12164

2) Can the author explain a bit more how robust these model results are? The author mentions, that he made no changes in the parameterization from previous publications. Nevertheless, explaining how changing the parameters for N2 fixation e.g. or the coefficients for retranslocation would not increase the NPP response would help to further strengthen the case made here. While perhaps a sensitivity analysis is beyond the scope of the work, insights by the authors (from extensive use and experience with

ecosys) would certainly guide the reader along. Alternatively (or in addition), justification of the most critical parameters will further help.

3) Both, the response to drought at the Duke site and the response to shortwave radiation are interesting, but are not part of the hypotheses posed initially. While interesting, I would suggest deemphasizing, using literature work on this topic and not necessary present them as new findings. Instead I would put more emphasis on discussing robustness and other modeling results – see above.

Minor remarks:

The author mentions, that he cannot distinguish whether increased investment in root an mycorrhizal tissue lead to a greater uptake, but perhaps he can tease out how the available Ammonium produced from microbial turnover is distributed among the different sink. This will help to determine, whether plants will get a greater share of mineralized N and would help to test the hypothesis.

The abstract contains a qualifier: "although such contributions might be greater over longer periods and under more N limited conditions than those postulated here". This part has not been discussed or shown in the main manuscript and I suggest deletion. If the author still wants to qualify he might want to write "Simulating more rapid nonsymbiotic N2 fixation, root N uptake and plant N translocation under elevated Ca was found to make much smaller contributions to modelled increases in NPP over the duration of the CO2 enrichment experiment".

P6795L20: The author states here that the root growth hypothesis was not directly tested, however he could elaborate how else he evaluated the influence or mention that he will look for the extent of root/mycorrhizal growth. Ultimately, this will result in roots competing more effectively for N against other sinks (leaching, denitrification, immobilization, etc.)

P6796L 13: What does a "slight N limitation" mean? The graph suggests quite a bit of

N response. P6797L10: Is the value of 0.03 in soil water content the absolute value or the difference between higher and ambient Ca? Tables 1-3: I presume the last line (delta Total) is the total ecosystem C and N change however, from the formatting of the table it appears to be the total soil change. Please clarify.

Interactive comment on Biogeosciences Discuss., 10, 6783, 2013.