

We very much thank the reviewers for their constructive comments, which added substantially to the assessment of uncertainties in the model results. The referee comments are in bold text and the author replies in normal text.

Response to Review #1

The assumption of globally uniform nutrient ratios for P model is restrictive for a global scale study. Although the authors discuss simulations where these are PFT specific these results are not presented and dismissed as not different enough to impact the results. I would prefer if the authors had presented the result of the PFT specific C:N:P contents and as a sensitivity analysis allowed the P concentration to be reduced from fixed ratios as is observed under low P availability.

We performed additional simulations accounting for stoichiometric flexibility. The design of these simulations and the results are presented in the appendix. We refer to the outcome of these simulations in discussion of the land C uptake. We prefer not to present the results of the simulations with the PFT-specific stoichiometry in more detail as suggested. The main findings are already mentioned in the discussion (page 3195) and the corresponding figure is shown in the appendix (page 3232). A more detailed discussion in the main text would put to much emphasize on stoichiometry. The uncertainties in the stoichiometric parameterization are now quantified by the perturbations simulations (see reply to reviewer #2).

Response to Review #2

Most importantly, the authors' framework assumes fixed C,N,P stoichiometry, without substitutions and in the absence of many known element cycle synergies. The authors' main assumption is that nutrients don't substitute or interact in positive ways – which stands in contrast to decades of work. [...] I appreciate that it is difficult to build a model that considers important nutrient interactions; but in the absence of a model that including interactions and feedbacks among N and P availability, it is essential to qualify the results accordingly.

In favor of simplicity we omitted N-P interactions and positive synergies, as the understanding of these processes is fractional and data to constrain such on global scale is not sufficient. However, it is yet not clear if N-P interactions are of importance for global scale given the large projected anthropogenic disturbance of the C cycle. We agree that our model may tend to overestimate nutrient limitation and that a possible overestimation must be stated clearly in the manuscript. We emphasize a possible overestimation of nutrient limitation in the abstract and in the conclusions.

I'm also concerned about parameterization of plant N/P. The authors assume fixed stoichiometry for plant tissues and soil, yet N/P of plants vary widely within biomes, and within and across plant species and microbial functional groups. The authors make several statements in this regard – but I'm wondering if this could be done in a more rigorous way? For example, data in Townsend et al., Ecology, (2007) show that the N/P of tropical plants – the most important biome for terrestrial C exchange – varies considerably within and across plant species. I recommend that the authors consider plasticity in their model analysis via probability distribution (of N/P) and monte carlo type simulations, to examine how much leverage plant stoichiometry has on CO₂ uptake and storage. If done well, this would also add considerably to our understanding of plant traits and N/P adjustments in overall CO₂ drawdown; and it would provide limits, something that this model is best equipped to do.

In response to the reviewers concern about the stoichiometric parameterization, we performed simulations with parameter perturbation using the Latin Hypercube Sampling (McKay, 1979) a procedure for testing the robustness of the modeling results against the uncertainty of our stoichiometric parameterization. Monte Carlo simulation, suggest by the reviewer, are not realizable with our model due to the high computational effort. The simulated range of land C uptake for all three nutrient setups are presented in the appendix. In combination with the results from the simulation with flexible stoichiometry done in response to the suggestion by the reviewer #1, these new results show that the stoichiometric parameterization is of minor importance compared to the parameterization of other processes discussed in the main text such as biochemical mineralization and soil P sorption. The results from these experiments are presented in the appendix.

Finally, the model should be further validated by taking advantage of empirical data. One thing that comes to mind are data on C/N uptake ratios observed for several studies, for example Thomas et al., Nat Geo (2010). It would be interesting for the authors to calculate their modeled C uptake/N (or P) input to see how the model functions relative to empirical understanding. This would add depth to the discussion, and might help further the idea behind fixed C/N/P.

We do not see other useful possibilities to further evaluate the model. A quantification of the growth enhancement by nitrogen deposition, as proposed, is not possible with our model due to our concept of nutrient limitation. In the model, an increase in nutrient availability is unable to increase productivity in the absence of plant growth stimulation by elevated CO₂. As CO₂-induced nutrient limitation is nearly absent under present conditions, the simulated C fluxes are only marginally affected by the nutrient cycles. To mention this point in the conclusion section In respect, to the high uncertainties in observational based estimates of the C cycle (Beer et al. 2010, Welp et al 2011), empirical data on cycle is not suited to evaluate the small differences in simulated C fluxes due to the nutrient cycles. JSBACH is commonly used to study the land-atmosphere C exchange (e.g. Raddatz et al, 2006 , Pongratz et al, 2009, Bathjany et al, 2010, Jungclaus et al, 2010) and the model has contributed to several model intercomparison projects such as C4MIP (Friedlingstein et al, 2006), CMIP5 (ongoing), and LUCID (Pitman et al, 2009)). A more detailed evaluation of the C and N cycles is given in Parida et al (2010), as already stated in the manuscript.

The treatment of N fixation is way too simple, and needs to be further discussed. N fixation responds to many factors – NPP is only one – and clearly NPP can't predict many patterns of N fixation known to occur in nature (see Vitousek et al., Biogeochemistry, 2002; Houlton et al., Nature, 2008; Wang and Houlton, GRL, 2010). Please qualify the results as such.

We agree with the reviewer that the N fixation is handled in a very simplistic way. The reasoning for such kind of representation is that relationship between N fixation and NPP is able to produce the broad pattern of N fixation. N₂ fixation is mainly controlled by supply of carbohydrates to the N₂ fixing organism and the need of reactive N. In the case of a symbiosis, NPP is the main source of carbohydrates as well as the main driver for the need of reactive N. It has yet to be shown that more complex N fixation models, including N-P interactions, are more reliable for global scale studies. We added a sentence to the discussion, where the omission of possible negative effects of warming on N fixation at low latitudes could have affected our results.

The authors assume that rock weathering doesn't contribute substantial N into ecosystems; but it's worth mentioning that this assumption is undergoing revision (Morford et al., Nature, 2011).

We mentioned the possible importance of N input by weathering in the conclusions and why we think these N inputs are of minor importance for our study.

The paper reads well overall, but the introductions could benefit from further editing.

Before the final submission of the manuscript we will further edit the text.