

Interactive comment on “Carbon-nitrogen interactions regulate climate-carbon cycle feedbacks: results from an atmosphere-ocean general circulation model” by P. E. Thornton et al.

Anonymous Referee #3

Received and published: 27 May 2009

1 Overall Evaluation

This manuscript represents a generally comprehensive analysis of a fully coupled carbon-climate model that includes terrestrial carbon-nitrogen interactions in the land component of an atmosphere-ocean-land general circulation model. This analysis is the first of its kind, although there has been one previous analysis involving a model of intermediate complexity. I think that this is an important study to publish, but I do feel that the manuscript is much more complex than it needs to be and I would suggest simplifying the manuscript so that presentation is much more direct. I think that this issue has come out in the three comments already received on this manuscript. My

C541

key suggestion is to re-examine the presentation of the manuscript by working backwards from the primary take-home messages of the manuscript, to the results needed to present those take home message, to the methods needed to present those results, to some clearly articulated questions in the Introduction. Below I'll primarily discuss the take home messages, as identified (or not) in the Conclusion, from the perspective of developing a tighter paper. I feel that the other reviewers have provided plenty of specific comments, so I won't be providing any in this review.

(1) While I am very interested in the importance of nitrogen deposition, I feel that the analysis of simulations with and without future nitrogen deposition detracts from the manuscript since the nitrogen deposition effect was not a very important feedback (similar to the opinion expressed by reviewer 1). It will clearly simplify the manuscript to only present results from rN and RN simulations and to remove sections 3.2 and 4.3. I think that section 3.2 could go into an appendix to support model evaluation if that is desired.

(2) I was surprised that there were no conclusions from the analysis of airborne, land, and ocean fractions (similar to comments by Chris Jones).

(3) It is clear from the other reviewers that there needs to be a conclusion on the regional importance of nitrogen limitation, and this probably calls for another analysis to clarify the analysis. This really brings up the issue raised by Arora's comment about the suggestion for an analysis that compares simulations with and without interactions. I recognize that this is probably not feasible at this stage. I'm comfortable with the inference that a carbon-only version of CLM-CN would result in Beta-L much higher than for CLM-CN in Figure 2d. But as both Jones and reviewer 1 indicate, there are substantial questions about what role nitrogen limitation of the tropics plays in the model world and what role nitrogen limitation of the tropics should play in the real world.

Interactive comment on Biogeosciences Discuss., 6, 3303, 2009.