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Biogeosciences Discussions

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Interactive Comment

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.

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Author response to Interactive Discussion comments on: "Carbon-nitrogen interactions regulate climate-carbon cycle feedbacks: results from an atmosphere-ocean general circulation model" by P.E. Thornton et al.

General remarks

All four sets of comments have made valuable contributions to this discussion, and we thank the reviewers for their careful and constructive consideration of the merits and weaknesses of our study, and for the many helpful suggestions – these will significantly improve the final manuscript. Several topics are raised by multiple reviewers, and we address these points first. We then address individual comments from each reviewer as necessary to cover every topic. A revised manuscript will follow.

Response to topics raised by multiple reviewers

Topic 1: Additional simulations to show the carbon-only behavior of the current CCSM

Arora (in Short Comment 1) suggests that an additional simulation be performed which exercises the CCSM in a carbon-only mode, arguing that this would permit a more direct evaluation of the influence of C-N limitations on Ca evolution out to 2100. Arora further argues that this additional simulation could provide a useful contrast in an evaluation of the influence of C-N interactions on predictions of historical trajectory and present-day values of Ca. Reviewer #1 expresses agreement with Arora's comment. Jones (Reviewer #2) also agrees, but notes that the effort associated with a new run may not be feasible on the timetable of publishing this manuscript. Jones indicates that, in the absence of a new simulation, a discussion of the uncertainty associated with this topic is required. Reviewer #3 agrees with Jones' position that including a new simulation may not be feasible at this stage, but stresses the importance of additional analysis and discussion of the C-only behavior in addressing the question of nutrient limitations in the tropics (for more details on this, see author response Topic 2, below). Reviewer #3 also suggests removing two of the four simulations (the low N deposition simulations) from the original analysis.

We gave considerable thought to this topic as we were planning the study. While there are real time and cost issues which affect the feasibility of adding more simulations at this stage, as acknowledged by Reviewers 2 and 3, there is also a scientific case to be made for why the design we selected is appropriate to the analysis we are attempting to deliver. Part of the solution we propose is to incorporate the very helpful suggestions offered by all three reviewers for modifications of the paper's emphasis (changes in the abstract, discussion, and conclusions), and to add early discussion that highlights the following science argument for leaving out a stand-alone C-only simulation:

Thornton et al. (2007) performed a detailed series of experiments forcing the land component used in the present study (CLM-CN) with offline atmospheric drivers. That study did include both C-N and C-only simulations, with otherwise identical model configurations. A valid criticism of that study, addressed during review, was that with a prognostic carbon cycle, the C-only and C-N implementations have very different steady-state accumulations of vegetation and soil carbon at the initiation of transient simulations with changing CO2 (and changing N deposition). The differences in initial condition complicated the interpretation and attribution of differences in the transient simulations. So, for example, it was difficult to assess how much of the difference in CO2 fertilization response between the C-only and C-N versions was due to the N-cycle interactions, and how much was due to a change in base-state (initial condition). We ended up adding additional simulations to that study which attempted to isolate the effects of different base states from the effects of C-N dynamics.

In the present study we were keen to avoid the problem of different base-states while still providing a robust analysis of the influence of N-limitation on the multiple aspects of the climate-carbon feedback. We believe that introduction of a dynamic (coupled) climate component mainly helps to inform the "gamma" part of the feedback question, the "beta" part having been dealt with fairly thoroughly in the offline (2007) study. For that reason it was even more important that we handle the base-state issue carefully in this study, since any spatial variation in the base-state differences are convolved with spatial variation in the climate change responses.

We stress here, and will add stronger language to this effect in the revised manuscript, that the intent of this study is not to provide the most accurate possible prediction of historical and future Ca, but rather to demonstrate that C-N interactions can have a strong regulatory effect on carbon cycle feedbacks with CaÂň and radiatively-forced climate change. It is not our intention to argue here that the C4MIP models have over- or under-estimated the prediction of Ca at 2100 by a particular amount. Our purpose instead is to show that a credible prediction of Ca depends at least on the inclusion of

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the first-order influence of C-N interactions, by demonstrating that these interactions are directly responsible for major sources of variance in carbon cycle components under climate change forcing.

In considering the design for this study, we realized that the CLM-CN model structure provides an elegant solution to the problem of isolating C-N effects from the influence of different base states, through an analysis of potential vs. actual GPP. This topic is covered in some detail in the manuscript (p. 3313, lines 9-12; p. 3319, lines 14-24). A summary is provided here for convenience. On every time step and for every vegetation component of every grid cell, the model provides a calculation of potential GPP, defined as the GPP that could be achieved in the absence of nutrient limitation, given the present ecosystem state. Following the calculation of nutrient supply and demand, and the resolution of competition between plants and microbes for the available nutrient resource, GPP is calculated again, taking nutrient limitation into account (actual GPP). Bear in mind that we are exploring here the hypothesis, stated explicitly in the manuscript, that the effect of the C-N interaction on gamma_land is expressed through warming-induced increases in nutrient availability resulting in a direct stimulation of GPP. By comparing potential and actual GPP from the same simulation we are able to very precisely attribute the influence of C-N interactions on the photosynthesis calculation, while assuring that all other details of the simulation state are identical.

For the purpose of being able to very confidently isolate the influence of the C-N interaction in this particular model, the approach we have taken here is an ideal expression of the general idea of performing a C-only simulation and a C-N simulation and differencing them, as suggested by Arora. None of the review comments addressed this aspect of the experimental design, suggesting that the argument for using potential vs. actual GPP in place of C-only vs. C-N simulations requires clarification in the text. We propose to add a short description of this aspect of the experimental design to section 2.1 (Detailed Methods) in the revised manuscript.

This same argument relates to the suggestion from Reviewer #3 to drop the low N deposition simulations (rn and Rn). By isolating very carefully the N-limitation effects on GPP, the low and high N deposition simulations allow us to compare the influence of climate change on this effect with the influence of direct alleviation of N limitation through external N inputs. This is the most direct analysis possible of the internal model processes relevant to the hypothesis of increased nitrogen availability under climate change. In other words, both the potential vs. actual GPP and the low vs. high N deposition approaches are necessary to provide a robust analysis of this feedback in the model while avoiding the complications of different base states.

Topic 2: Tropical nutrient limitation

Reviewers 1, 2, and 3 make the important point that growth of tropical forests (especially lowland types) is usually observed to be primarily limited by phosphorus, not nitrogen, availability, and yet our simulations show that this is a critical region for the expression of the C-N interaction effect under climate change. This raises the possibility that our results may exaggerate the globally integrated influence of C-N interactions on beta_land, gamma_land, and the overall climate-carbon feedback.

We agree that the treatment of nutrient limitations in the tropics is one of the most important sources of uncertainty in our current analysis, and the revised manuscript will include a much more prominent discussion of the topic, along the following lines. The first point is that the assessment of potential biases in our approach would be very different if the lowland tropical forests were observed to have no significant nutrient limitations. Instead, growth of these forests is usually observed to be limited by phosphorus availability. In Thornton et al. (2007), we set forward the hypothesis (p 13, paragraph 43) that modeling the dynamics of nitrogen limitation might serve as a useful simulation proxy for ecosystems where even more significant limitations from phosphorus, or N-P colimitation, might dominate. Expanding upon that hypothesis here, we note that the very large majority of both N and P taken up by tropical forest plants on an annual basis comes from internal nutrient cycling, through the mineralization of N and P from decomposing soil organic matter. Short-term availability of N and

P are therefore affected by similar physical climate controls on heterotrophic respiration and decomposition. This argument suggests that the use of N-limitation as a proxy for N and P limitation would impose an upper bound on ecosystem growth, while the real nutrient limitation (and therefore the C-nutrient interactions) would be even larger for the case of P-limited or N-P colimited ecosystems. By this logic, our current approach is likely under-estimating the influence of nutrient limitation on both beta_land and gamma_land components of the overall feedback.

An important counter-argument to this statement of the N-proxy hypothesis is that the coupling of N and P dynamics is complicated by the connection between P-availability and biological N-fixation, which could lead to a coupled N-P limitation that is smaller than the simple N-proxy limitation. In that case our results could be overstating the importance of the real C-nutrient interaction effect on the carbon-climate feedback components. Recent development of a C-N-P modeling approach (Wang et al. 2007; Houlton et al. 2008) highlights the importance of these interactions. We have taken this counter-argument into consideration, and have tried to mitigate against the possibility of severe biases in this direction by incorporating a prognostic calculation of biological N fixation (BNF) into CLM-CN. This formulation (described in detail in Thornton et al. 2007, supplemental text) tries to capture the first-order dependencies of BNF on climate and carbon availability by making BNF a saturating function of net primary production. Our algorithm captures observed large-scale geographic patterns of BNF, and results in a simulated present-day global total BNF that is within the range of current estimates based on observations.

Our revised manuscript will also include expanded discussion of results from LeBauer and Treseder (2008) and Elser et al. (2007) regarding the degree of N-limitation in tropical forests and grasslands. The meta-analysis of LeBauer and Treseder (2008) shows that the degree of N-limitation in tropical forests is comparable to that for temperate forests, even excluding a subset of strongly N-limited tropical forest sites on very young volcanic soils in Hawaii. Their sample size for tropical forest N response

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was small, with eight studies after excluding the studies on young Hawaiian soils. Of these, four were in lowland tropical forest, and of these one was in primary forest while three were in secondary forest. N limitation in lowland tropical forest was stronger for secondary than primary forest (50% vs. 9% growth stimulation). They note that secondary forest makes up a significant fraction (40%) of tropical forest world-wide, challenging the conventional wisdom that lowland tropical forest should be considered strictly P-limited. Elser et al. (2007) carried out another meta-analysis of studies examining responses to both N and P fertilization, and showed that the growth responses to N and P fertilization are indistinguishable across terrestrial ecosystems as a whole, while the N+P response was significantly larger than the independent responses. They show that forest responses (dominated by tropical studies in their dataset) were significant for both N and P, but were stronger for P fertilization.

Given these meta-analyses, it seems reasonable to assume that N limitation is real, even in the tropics, but that for the lowland tropical forests, especially primary forest, P limitation is the more important limitation. This conclusion is consistent with our hypothesis that N-limitation serves as a lower-bound proxy for the full effect of N+P nutrient limitation. On these grounds, we disagree with the statement from Reviewer #2 (in their point 1) that “it is well recognised that most tropical forests are not nitrogen limited”. It is more accurate to state that lowland primary tropical forest is more limited by P than by N, while lowland secondary and upland tropical forest may exhibit the same degree of N-limitation as observed in temperate forests. We are participating in a synthesis of observed N-P interactions in tropical ecosystems, and as stated in Thornton et al. (2007) we have the near-term goal of including C-N-P interactions in our terrestrial model. We hope to be able to quantify the impact of these assumptions and uncertainties in future studies.

Topic 3: Comparison to recent observations of land and ocean carbon uptake and CO2 concentrations

Arora suggests that it would be best to make a thorough analysis of the influence of

C-N coupling on predicted vs. observed land and ocean carbon uptake and CO2 concentrations before exploring, as done here, the potential influence of C-N interactions on the carbon-climate feedback components out to 2100. Reviewers 2 and 3 suggest additional emphasis on the comparison of modeled and observed atmosphere and ocean fractions of anthropogenic emissions, highlighting in the abstract and conclusions some of this material which is currently treated only in the results section. Reviewer 1 also requests a comparison of predicted and observed land uptake as part of the evaluation of C-N influence on beta_land.

We take seriously the need for some level of evaluation against observations in a study such as this, where the primary objective is establishing a credible basis for the sensitivity of the system to a previously ignored biogeochemical interaction. A comparison of predicted and observed CO2 concentrations over the historical period is a valuable exercise, but large uncertainties in the magnitude and timing of the net flux due to anthropogenic land use and land cover change (LULCC) make this a poorly constrained problem. At the time we began this study we were not satisfied that the available sources of LULCC information were adequate to make a robust estimate of the resulting net carbon flux. We know for example that our model predicts significant interactions among disturbance history, CO2 fertilization, N availability, and climate change. We were therefore reluctant to use the C4MIP approach of specifying a LULCC flux (with a large uncertainty), without any of the accompanying changes in physical and biogeochemical ecosystem structure and function that are the drivers of the net flux. We are pursuing the important problem of producing our best-effort predictions of historical trends in Ca and comparing them to observations as a separate study, which includes a mechanistic treatment of LULCC and its interactions with the C-N dynamics.

We decided to rely here on an analysis of the airborne, ocean, and land fractions of anthropogenic emissions as a primary point of evaluation against recent global scale carbon cycle observations. Some influence of the treatment of LULCC still affects this approach, but we feel it is a suitable compromise, given that our primary intent was not a model evaluation, but rather a demonstration of a potentially important sensitivity. The results of this analysis are detailed in Section 3.1 of the original manuscript. In the revised manuscript we will follow the suggestion of Reviewers 2 and 3 to revise the wording in the abstract and to add a major point in the conclusions to highlight the results in Section 3.1. That section already emphasizes the comparison of our predicted uptake against observational constraints, contrary to the assertion in the first three lines of the second paragraph of the comment from Arora. Reviewer #1 also noted that the manuscript is missing "a critical comparison to the observational evidence" on the point of land carbon uptake (their point 1), but we stress that the original manuscript already makes a direct comparison of our best-estimate land sink fraction (0.19) against values from the same study cited in the IPCC AR4 for the range of uncertainty in this quantity (0.1 to 0.41, from Sabine et al. 2004). As requested by Reviewer #1, our revised manuscript will reference this comparison in the discussion of differences between our study and the study of Sokolov et al. (2008). Reviewer #2 (Jones) also requested that we include in our discussion of the airborne fraction a comparison to Figure 7.13 from the Working Group 1 report of IPCC AR4, but we note here that the Sabine et al. comparison in our original manuscript (Section 3.1) is the source reference for Figure 7.13 in WG1 AR4.

The abstract already mentions the airborne fraction analysis, but we intend to add the following sentences in the abstract to strengthen the connection to observations: "Estimates for the land and ocean sink fractions of recent anthropogenic emissions are individually within range of observational estimates, but the combined land plus ocean sink fractions produce an airborne fraction which is too high compared to observations. This bias is likely due in part to an underestimation of the ocean sink fraction."

In addition, following the comments from Jones and Reviewer #3, we will add a new top-level bullet to the conclusions referencing this result.

Topic 4: Relevance of nitrogen deposition and fate of added nitrogen to current study

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Reviewer #1 feels (their General Remark 5) that including analysis of the fate of new N in land ecosystems defers from the main point of the manuscript (the C-N interactions), adding that the study would have been stronger if focused on evaluation of the two critical processes: N limitation of CO2 fertilization, and net C balance response to N additions of warming. They amplify this comment later in their review when stating, in reference to our Section 3.2, that the "fate of added N is only one factor, the question is whether or not the response of C stored for a unit of added N is the correct order of magnitude." We thank Reviewer #1 for pointing us to some very interesting new literature on this topic, which we will incorporate in our revised manuscript. But we maintain that this material is in fact a critical aspect of the study, for the very reason that Reviewer #1 gives in the amplification of their comment. What matters most for the C-N interaction with the climate system in this regard is how the fate of newly added N interacts with the net carbon balance, and as Reviwer #1 rightly points out, the main issue to get right is the partitioning between soil organic matter and woody biomass, owing to the approximate order-of-magnitude difference of their C:N ratios. This is precisely the point of our analysis of the topic, to show that the modeled response of accumulation of new N in soil and wood pools is at least in qualitative agreement with long-term studies. We agree that the original manuscript did not draw the connection to the carbon question strongly enough, and we are strengthening this in the revised manuscript in the methods, results, and discussion sections.

We note that several experts in the field of nitrogen-carbon cycle ecology were asked to provide external reviews of this material as the study progressed, and the section on fate of added N was important in convincing that community that the ecology represented in the model was robust. We also note that the results shown in our Figure 9 represent a new model evaluation metric, of a type that will be increasingly required as the climate system models adopt more realistic representations of nutrient cycling.

Reviewer #3 questions the need for the two simulations which used pre-industrial levels of nitrogen deposition. As argued under Topic 1, above, this set of experiments is a critical component of the experimental design which relies on the difference in potential vs. actual GPP to assess the C-only behavior of the warming influence on the carbon cycle. These low-N deposition experiments are essential because we are arguing that the difference between C-only and C-N responses in GPP under radiatively forced climate change is mainly due to the increased availability of mineral N, which fertilizes primary production. We make this case in part by showing that the response of the C-N simulation under climate change is mechanistically similar to a direct forcing of the N cycle through atmospheric deposition.

We will increase the value of these extra simulations in the revised manuscript by following Jones' suggestion of adding information on the change in airborne fraction due to anthropogenic N deposition.

Topic 5: Revision of emphasis in abstract

Reviewers 1 and 2 both suggest that the emphasis in the abstract, and specifically our first sentence, should be shifted to better reflect the discussion and conclusions regarding the overall influence of C-N interactions on the land carbon cycle contribution to climate system forcing. We agree that by narrowing the focus in the abstract to just the response of the land carbon cycle to radiatively forced climate change we are leaving too much room for possible misinterpretation of our results. We agree that modifying the emphasis in the abstract will improve the comprehension of our overall message. We suggest the following revision: "Inclusion of fundamental ecological interactions between the terrestrial carbon and nitrogen cycles in the land component of an atmosphere-ocean general circulation model (AOGCM) leads to decreased carbon uptake associated with CO2 fertilization, and increased carbon uptake associated with warming of the climate system. The balance of these opposing effects is to reduce the fraction of anthropogenic CO2 sequestered in land ecosystems." This statement is more consistent with the discussion in Section 4.1 of our original manuscript, and with the priority given to results in the conclusions section.

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Response to individual reviewer comments

Response to comments from V. Arora

We have addressed all of Arora's comments in our Topics 1 and 3, above.

Response to comments from Reviewer #1

Response to General Comments: 1. We will modify the statements comparing our results to those of Sokolv et al. (2008), as suggested. The issue of comparison to observations is addressed under Topic 3, above. 2. These concerns about relationship between our results and current state of knowledge for tropical systems are addressed under Topic 2, above. 3. We agree that the discussion of the importance of relative sizes of the multiple components of the overall gain needs to be addressed more concretely. Our abstract will be revised to highlight this important point, and we will add a short section in the discussion that addresses this topic directly. We propose the following revision in the abstract (new text in italics): "... leads to increased carbon storage on land under radiatively-forced anthropogenic climate change, and, for this particular model, an overall negative climate-carbon cycle feedback." 4. We agree that reduction in the ranges of beta_land and gamma_land as models introduce C-N coupling does not necessarily correspond to a decrease in the feedback gain. Our treatment of this topic in Section 4.2 is accurate, as is the treatment under item 2 in our Conclusions section, but our wording in the final paragraph of the conclusions (p 3330, lines 1-5) needs to be clarified. We suggest the following revision: "We argue that between-model variation in land carbon cycle responses to both CO2 fertilization and climate change would be reduced by the introduction of C-N interactions in other climate-carbon cycle models, which would tend to reduce the range of uncertainty in predictions of future climate from the coupled models." 5. Addressed in Topic 4, above.

Response to Specific Comments

(3305 l1-5): Covered in Topic 5, above.

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(3313 l1): These calculations are performed on a grid cell basis, then averaged or totaled for global values, as specified in the caption for Figure 4. This is clarified in the methods section of the revised manuscript.

(3313 l25): Selection of 120-year moving window for analysis of transients in feedback parameters. Contrary to this comment, we are not using a 120-year moving average of the results in the calculation of the transient feedback parameters. Rather, we are using a 120-year moving window, within which an unweighted regression is performed on the n=120 individual (unsmoothed) annual values to calculate the relevant slopes. This applies the same regression methodology for calculating the feedback parameters that we introduced in Thornton et al. 2007. Some moving window approach is necessary to obtain a transient, and we could have selected an endpoint analysis that either kept one end fixed (at 1870) while moving the other end forward, or that moved both ends together. The advantage of moving both ends together is that the length of record influencing each time value in the resulting transient is the same, so that changes later in the simulation period are expressed with the same likelihood as changes early in the simulation. The advantage of the regression method in general over the endpoint analysis is that it is not unduly influenced by interannual variation. We agree that the exact choice of window length is arbitrary, but since that choice has little bearing on the results, as recognized by Reviewer #1, we feel it didn't require additional explanation in the text.

(3315 l23): We agree, and will remove this sentence in revision.

(3316 l5): We agree that this needs to be addressed earlier in the methods. This sentence will be moved in the introduction of the Methods section in revision. In addition we will add the following statements of justification in that section, addressing the numbered points in this specific comment: 1) "Imposing LULCC fluxes as an external forcing factor ignores the known interactions among disturbance, CO2 fertilization, and nitrogen availability (Thornton et al., 2002). Here we address the C-N interactions in the absence of potentially confounding anthropogenic disturbance effects". 2) "Excluding effects of LULCC means that our predicted values of Ca and associated climate change are very likely underestimates.” This second point is also called out in the first paragraph of the Discussion in the original manuscript (p 3324 l3).

(3317 l16): Repetitious lines removed.

(3318 l3-5): We agree that the wording here gives the wrong impression. It is not a foregone conclusion that the same land model exercised in offline mode and coupled to a fully prognostic climate system model would deliver the same results for the globally integrated CO2 fertilization effect, since climate biases in the coupled model might very well produce a different result than the offline model. What we meant to convey here is that the quantitative agreement between the two model implementations gives us some confidence that the very detailed analysis of the effects of C-N coupling on beta_land performed for the offline model in Thornton et al. (2007), including the explicit analysis of the C-only vs. C-N simulations, have relevance for the fully-coupled case. We propose to revise as follows: “. . . and results here from simulations with the fully-coupled model are in quantitative agreement, in spite of known biases in the coupled climate.”

(3318 l6): We agree: “confirmed” will be changed to “demonstrated” in revision.

(3318 l24): This is revised to read: “Simulated ocean carbon stock declines by 35 Pg C under the influence of radiatively forced climate change over the period 1870-2100. That decline is more than offset by a net increase of 47 Pg C on land over the same period, leading to a small negative climate-carbon cycle feedback gain at year 2100, the opposite sign compared to all previous studies (Fig 2f).”

(3319 l1): We propose adding the following sentence here to clarify: “The influence of anthropogenic N deposition on the feedback parameters mainly conforms to the geographic distribution of the increased deposition (results not shown).” There are some spatial details associated with increased drought stress in a few tropical regions under higher N deposition, but we do not feel that there is space or the necessary supporting information in the present manuscript to explore the issue.

(3319 l4): The original text was not clear on this point. These comparisons are based on mean values of the beta_land and gamma_land from C4MIP simulations, calculated as transients (as shown by the gray lines in our Figure 2) from the model results in the C4MIP archive. We then used the standard feedback analysis (linearization) and the C4MIP fossil fuel flux boundary condition to estimate the associated differences in Ca. We agree that comparing only to the C4MIP means tends to exaggerate the influence of C-N coupling in this analysis. Although there are many differences in addition to the introduction of C-N coupling between our current model (CCSM3.1) and the predecessor model used in the C4MIP exercise (CCSM1), we agree that it is still useful to compare the two models directly in the feedback analysis, particularly given our approach to the C-only simulation with CCSM3.1 (Topic 1, above). We are comfortable including the comparison to C4MIP mean values as a part of our results since we have shown previously (Thornton et al. 2007) that the C-only version of the CCSM3.1 model behaves quantitatively like the C4MIP mean for beta_land. We revise this analysis to show the influence of substituting C-only and C-N versions of the feedback parameters under four different configurations: using CCSM3.1 for the full set of feedback parameters and substituting either (1) C4MIP mean or (2) CCSM1 values for beta_land, gamma_land, and the combined substitution, and likewise substituting CCSM3.1 values for beta_land, gamma_land, and the combined substitution within (3) the C4MIP set of mean feedback parameters, and (4) the CCSM1 feedback parameters. We propose to include the mean and range of these four methods in our results, substituting the sentences in question as follows: “Using a transient feedback analysis, we estimate the influence of C-N coupling on Ca in year 2100 by substituting feedback parameters from our model with transient feedback parameter values calculated from the C4MIP archive (Friedlingstein et al. 2006), using multi-model mean parameters as well as single-model parameter substitution from the predecessor C-only CCSM model (CCSM1). Reduced land CO2 fertilization (smaller beta_land) with the introduction of C-N coupling increases Ca on average 104 ppmv (range +65 to +178 ppmv). Stimulation of carbon uptake under a warming climate (reversal of sign for gamma_land) decreases Ca on average 82 ppmv (range -133 to -35 ppmv). These two effects of C-N coupling acting together increase Ca on average 16 ppmv (range +31.7 to -8 ppmv)." This revision represents the primary point in the results which backs up the revision of emphasis in the abstract (Topic 5, above).

(3320 l3-4): We agree that this sentence can be left out without affecting the clarity of the result.

(3320 l16ff): This is an interesting question, but to provide more than a qualitative comparison, as done here, requires a more detailed analysis than the current study supports. To compare the magnitude of responses we would need to replicate the experimental setup at the sites, to capture the influence of pre-treatment disturbance and treatment details. This is a valuable exercise, but more than we can tackle in the current study.

(3321 l3): Modified as requested in revision.

(3.1): This is an important point, and we will include mention of it in revised section 3.1.

(3.2): We address this comment in detail under Topic 4, above.

(3324, l7-9): See our response under Topic 1, above. Also, further clarification is now provided with the revised results section from comment on p 3319, l4.

(3324, l26): Replaced "lower" with "low" in revision.

(3325, l8ff): This paragraph will be modified as suggested in revision.

(3325, l12): We agree and will include a short statement to this effect in revision.

(5.1): We agree, and propose the following revision: "This conclusion is supported by previous studies,. . ., and now here for the case of a fully-coupled climate system model. We note that each of these studies is based on either the TEM or the CLM-CN land model." We thank Reviewer #1 for pointing out the relevant figure in the Cramer et al. 2001 study. We had not previously included this paper in our discussion since it does

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not address the C-N interactions as part of the explanation for any of the differences among the DGVMs. We propose to include the following statement in the discussion section 4.2, near p3326, l18: “A comparison of dynamic global vegetation models suggests that inclusion of dynamic biogeography might complicate the influence of C-N coupling on the land ecosystem response to warming (Cramer et al., 2001).”

(Figure 3): This comment was not clear – the two line types refer to simulations with preindustrial N deposition (solid) and anthropogenic N deposition (dashed), and that convention is the same for all the panels in Figures 2 and 3.

(Figure 7): The requested change has been made.

Response to comments from Reviewer #2 (Chris Jones)

Response to major comments: 1. The concern and suggestions regarding treatment of nutrient limitation in the tropics is addressed in Topic 2, above. The part of this comment dealing with the lack of a C-only simulation is addressed in Topic 1, above. 2. We agree with the suggested shift in emphasis, and our proposed solution is outlined in Topic 5, above. See also our response to Reviewer #1 (p. 3319, l4), above, for details on how we plan to further emphasize and quantify this point in the results. We thank Jones for pointing out the new paper from Gregory et al., 2009. We strongly agree with the arguments there for giving attention to both the CO2 and the climate parts of the feedback. We feel that, in conjunction with the Thornton et al. 2007 paper focused on the CO2 effect, the revised manuscript is an example of that approach.

Response to specific comments: (p 3306, line 3): Done.

(p 3306, line 14): Done – thank you!

(p 3307, line 17): We agree and propose to revise as “For land, this positive feedback has been attributed to an increase in soil organic matter decomposition and the sensitivity of plant growth etc.”

(p 3308, line 27): Yes, SRES A2. Revised to include this information.

(p 3310/11): Yes, Ca is allowed to vary in the Sim1 step. We found this to be necessary to avoid a very long ocean spin-up to eliminate the last small air-sea net fluxes. The negative ocean-atmosphere CO2 feedback quickly resulted in a stable pre-industrial Ca that was within a few ppm of the target 1870 value. We argue that this approach is reasonable since we were not primarily concerned with exact reproduction of the historical Ca time series in these simulations.

(p 3312): We appreciate this argument, and will be performing a series of simulations for AR5 that use prescribed Ca, following the CMIP5 protocols.

(p 3313): See response to Reviewer #1 (3315, l25).

(p 3316): We recognize the importance of considering the LULCC effects, and as argued above we feel this is so important that it requires its own study, so as not to complicate the interpretation of the uncertain climate-carbon-nitrogen interactions with additional uncertainty due to disturbance-climate-carbon-nitrogen interactions. We agree with the suggestion to introduce additional discussion highlighting the likely influence of this decision on our predictions of Ca, as described in Topic 3, above.

(p 3318, l3): Agreed, please see response to Reviewer #1 (3318 l3-5).

(p 3319): We apologize that this was not presented clearly enough. We have also taken the suggestion of Reviewer #1 to revise the analysis. See response above (3319 l4)

(p 3321): See Topic 3, above. We will also include the following information on the N-deposition influence on AF: "In the absence of anthropogenic nitrogen deposition, aE over the period 1959-2006 is 0.59. Relative to the case with anthropogenic N deposition, the land fraction drops to 0.14, with a compensating increase in the ocean fraction to 0.27. For the period 2050-2099 in the absence of anthropogenic N deposition the airborne, land, and ocean fractions are 0.67, 0.13, and 0.20, respectively." (other greenhouse gases): This is an excellent point. Our model includes bulk gaseous

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emissions from ecosystems, but does not yet adequately treat the speciation of these emissions, or interface adequately with the atmospheric chemistry modules, to quantify the additional GHG forcing. We do expect that N2O emisisons will be increased under a warmer climate, and will add a statement to that effect in the discussion. (possible reduction in uncertainty): This does seem like a possibility, but we would rather examine results from other coupled models before speculating on the importance of this result.

Response to comments from Reviewer #3

1. We disagree with the argument that removing the rn and Rn simulations and associated analysis would strengthen the paper, for the reasons detailed under Topic 4, above. This is a critical aspect of the experimental design, and is a necessary component of our demonstration that the simulated N dynamics play the role we have hypothesized in regulating the response to radiatively forced climate change. 2. We agree that these results need to be highlighted in the conclusions. See our detailed response in Topic 3, above. 3. We agree that there needs to be a more detailed discussion of tropical nutrient limitations, how they are represented in the present model, and how that representation is likely to influence our results. See our response in Topic 2, above, for details on how we propose to address this in the revised manuscript. See also the response under Topic 1 regarding the representation of C-only dynamics in these simulations.

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

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Interactive Comment