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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 #1

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General Comments

In their manuscript, Thornton et al. provide a very interesting new look into the mechanisms that control the carbon-climate feedback and ultimately affect the rate of C accumulation into the atmosphere by accounting for terrestrial N dynamics. They do this using a comprehensive carbon-nitrogen land surface model inside a comprehensive Earth system model, which is the first study of this type that I am aware of. Accounting for nitrogen limitations has major qualitative changes for predictions of current carbon-climate Earth system models and further demonstrates the importance of understanding terrestrial biogeochemical cycling beyond carbon to understand the future evolution

of the climate system. Having said this, there are some aspects of the manuscript that require further discussion to avoid erroneous interpretation of the results of the study in addition to the issues raised by V. Arora in his Short Comment on 14/05/09, to which I agree.

1) Concerning the reduction of the C uptake on land in response to CO2: It is true that TEM-CN (Solokov et al. 2008) shows a strong reduction of β_l due to the inclusion of terrestrial N dynamics. However, that reduction is a reduction by about 50%, less than the 70% predicted by CLM-CN. More importantly, the final value of β_l in CLM-CN (0.4) is about substantially lower than the value predicted by TEM-CN (0.68-0.77). This is hardly a (quantitative) confirmation of the predicted change in terrestrial net C storage due to N limitation, as argued by the authors. It supports the results in that a reduction of β_l is to be expected from accounting from N dynamics, not more. There are reasons to believe that the CLM-CN response might be too low (without the presence of any land-use change; see Thornton et al. (2007), as well as p3324 l24ff of the present manuscript). I am missing a critical comparison to the observational evidence at this point. I don't object to the argument that land-use history makes this comparison difficult, but the observational evidence and how good CLM-CN matches these observations ought to be mentioned together with the comparison to TEM-CN to place the results into perspective.

2) Most of the C becomes sequestered in tropical rain forests following a moderate warming. This is arguably due to the positive effect of soil organic matter decomposition on N availability, and thus plant growth. However, to my knowledge this effect has never been demonstrated for tropical systems, and all references cited by Thornton et al. refer to temperate to arctic ecosystems. The strong response w.r.t. to warming and increasing N availability is contrary to the general idea that low-land tropical forests are rich in N and mostly limited by Phosphorous availability (Matinelli et al. 1999, Vitousek Sanford 1982; Uehara, G Gillaman 1981, Jordan 1981, Townsend et al. 2008), and I am missing a critical discussion of results in the light of this hypothesis.

Some studies have shown that N additions indeed stimulate growth in some tropical forests, but these studies have also shown that this is not generally the case (Davidson et al. 2004, Cleveland et al. 2006). The authors need to discuss their findings with respect to these studies, and give an assessment of how different the response of the terrestrial C cycle to climate change, and thus the carbon-climate feedback, would have been, if their strong positive tropical response was much lower than estimated by their current model accounting for potential Phosphorous limitations.

3) A critical factor of the feedback gain reported by Thornton et al. is the difference in the ocean response to climate change (γ_o: -10 PgC/K) relative to the ocean model in CCSM1 (Friedlingstein et al. 2006: -17Pg C/K), which is probably mostly due to the new ocean model formulation. The implication of this model development is that, without any changes in the land model (for instance related to C-N interactions), the new CCSM3 model would show a smaller carbon-climate feedback than its predecessor in the C4MIP study, and thus requires less C to be taken up by the land in compensation. Furthermore, the predecessor model, CCSM1, already had one of the smallest feedback gains due to small γ_o and γ_l (see Friedlingstein et al. 2006). If CLM-CN had been implemented into another Earth system model that showed a stronger oceanic C loss resulting from climate change, the effect of terrestrial C-N interactions would likely have been a reduction of the positive feedback, but not a cancellation or even slightly negative response, and thus the importance of C-N interactions would have been less pronounced than the present study suggests. The authors should discuss this very important point concerning the generality of their conclusions more than in just the general terms (p3325 l 1ff) and revise their abstract respectively.

4) I do agree with the author's qualitative argumentation of the likely compression of the range of both β and γ due to the incorporation of an N cycling constraint. However, depending on the relative adjustment of these two parameters, this can, but does not necessarily need to imply a decrease in the feedback gain. This happens to be the case for the two published studies, but it is not guaranteed that this is the case for all

models and that thus the range of feedback estimates would indeed contract for all models.

5) I see the value analysing the fate of newly added N to ecosystems, but I think that is defers from the manuscripts topic (i.e. the interactions between C and N cycling). Evaluation of the two critical processes, N limitation of CO2 fertilisation and net C balance response (i.e. soil C loss versus vegetation C increase) to N additions or soil warming would have made the manuscript much stronger than a mere comparison of the fate of newly added N, without discussing the growing body of literature of the C sequestration response to newly added N (e.g. Sutton et al., 2008, de Vries, in press., accessible online).

Specific Comments

3306 l1-5: The conclusion about the net effect of carbon-nitrogen interactions is much more definite in the abstract than in the conclusion. The sentence in the abstract should be rephrased such that it matches the more appropriate formulation in the conclusion.

3313 L1ff: It does not become clear whether these calculations have been done for the global mean or on a grid cell basis. Please specify.

3313 L25ff: A methodological point that will not affect any of the conclusions, but seems a bit peculiar: It is not clear to me what the justification of the 120years moving average is. Wouldn't it have been simpler (and more correct) to compute this as cumulative value up to a certain point in time? This would avoid ignoring past changes older than 120 years and would remove the arbitrariness of the choice of 120 years, and the need to a statistical approximation.

3315 23ff: irrelevant for the present manuscript.

3316 l5: Emission data ignore land use component: This needs to be made clear already in Section 2.1, since this is an essential part of the model set-up; it has nothing to do with the way to calculate of the sink fraction directly. It will certainly affect the estimated land sink fraction. The omission of land-use related C fluxes in the simulation furthermore requires a justification (in 2.1), since 1) land-use fluxes are not calculated from the simulations so that there would not have been the risk of double counting and 2) given the large C flux from land-use change this set-up implies that the present-day atmospheric CO2 content will be likely too low, and the rate of climate change lower than predicted from models that do take account of these emissions. I am not arguing against this approach, but the consequences (see 3317 l 22-24) must be made clear already in the description of the method.

3317 l16ff: Repetition from line 7-8, delete.

3318 l3-5: Is this surprising? This is a comparison of two studies using essentially the same land-surface model driven by different meteorological forcing but comparable changes in Ca.

3318 l6ff: 'confirm' is not the right word. The authors should consider using 'demonstrated' or alike instead. The two model predictions agree qualitatively (less so quantitatively, see above), and this does lend some support to the findings of the present study, not more.

3318 l24: Give absolute numbers of cumulative ocean and land C storage here.

3319 l1: Is this response globally uniform or geographically variable? Which ecosystem types show a particular response and how large is this? My question could be answered either here or as part of 3.2

3319 l4 This is not clear. Did you use the 2100 Ca values of the coupled and uncoupled runs from Friedlingstein et al. 2006 (note that this is then confounded by the fact the C4MIP runs did not start at exactly the same prehistoric CO2), did you use the change from the initial concentrations, or did you use the ocean and land sensitivities to derive the change in Ca? The comparison to the mean C4MIP Ca in 2100 (see also specific comment below) is partially misleading, as the predecessor of the model used has one

of the lowest responses to both Ca and climate change. Simply comparing the CCSM1 simulation in Friedlingstein et al. 2006 to the mean of the simulations of the C4MIP project would yield a higher Ca for CCSM1 in the uncoupled case, and a lower Ca due to the coupling. In other words, this comparison exaggerates the effects of the C-N interactions on Ca, and should be revised.

3320 l3-4: This point is repeated one paragraph further down (l16ff), and I'd suggest to only mentioning it once.

3320 l16ff: Does CLM-CN correctly predict the magnitude of response of soil respiration, N release and vegetation growth to soil warming observed in the soil warming experiments mentioned in 3320 l16ff?

3321l3 ...is largely mitigated (add:) 'but not completely compensated for' by...

3.1 While it is true that the treatment of land use in previous studies is simplified and does not take account of the interactions of disturbance with N deposition and changing Ca, the net effect is likely to be a further reduction of the land sink due to the large tropical deforestation flux.

3.2 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 in the correct order of magnitude

3324 l7-9: See major comment on the comparability of C4MIP results with this study. To properly derive this result, I would have expected a separate simulation with the same model set-up, but no explicit consideration of N dynamics.

3324 l26: lower than what?

3325 l8ff: The authors should mention here that the range of ocean responses from the C4MIP study suggest a range of -16 to -67 Pg C / K, which is lower than the value simulated in this study (-10 Pg C / K). Taking any other than the ocean model used in this study would increase the C required to balance the feedback by at least 50

3325 l12: The authors should also mention that apparent estimates of beta and gamma for land and ocean are never completely independent from each other, for example since a higher beta of the ocean implies a slower rise of Ca, with consequences for the saturating response of land C accumulation.

5.1 This statement suggests to the non-expert reader as if there were many models suggesting this type of feedback, whereas in reality it is these studies are essentially based on two models, TEM (McGuire et al. 2001 and Solokov et al. 2008) and CLM-CN (Thornton et al. 2007, in review). This should be made clearer. The authors should also mention that other dynamic global vegetation models including a carbon-nitrogen cycle coupling do not show an increase in C storage from climate change alone (see results of HYBRID and SDGVM in Cramer et al. 2001, Fig. 6).

Figure 3: use a different line type for the uncoupled experiment in part c). Plotted like this it is confusing although the figure caption specifies which line refers to which simulation.

Figure 7: Plot C fluxes for the simulations of Ca and Ca+CC+ND in part a and b also to allow a comparison of the effects.

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