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Biogeosciences Discussions

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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I found this an interesting and well written paper addressing a subject of importance for earth-system and global carbon cycle modelling. The message is clear and adds to accumulating evidence of the need for climate-carbon cycle models to account for nitrogen cycling in the terrestrial biosphere.

The key results from the paper seem to be that nitrogen limitations on plant growth reduce the ability of the terrestrial biosphere to absorb CO2, and that warming-induced decomposition of soil organic matter makes available additional nitrogen to the plants and thus stimulates plant growth. The former effect reduces the "beta" in C4MIP terminology, and the latter reduces the "gamma" or even changes its sign. Overall the "climate-carbon cycle feedback" is reduced and although C-only models universally agree that this is a positive feedback, the inclusion of C-N processes could even make it negative.

I have 2 major comments - one a reservation about the magnitude of the results, and one on the emphasis of the results - and several specific comments. I do, though, feel that this work should eventually form an important contribution to the literature.

1. As reviewer#1 I would like to see a discussion at least on the magnitude of the effect, especially in the tropics. As already listed by reviewer#1 it is well recognised that most tropical forests are not nitrogen limited, and yet the magnitude of your results is critically dependent on the degree of N limitation in the control simulation. Were the control simulation to exhibit little or no N-limitation then the scenario simulations would show a much weaker dependence of future C behaviour to N deposition and/or remineralisation.

The combined existing small C-cycle feedback of this model and the (probable) overestimated impact of N given too strong initial N-limitation does appear to call into question the quantitative nature of these results.

The recommendation of Vivek Arora that a non-N-limiting simulation should also be run is a good one - I think this would be very valuable. Given such a simulation it may then even be possible to mix-and-match regions of the world - say taking the non-limited tropics and N-limited extra-tropics - to see how the magnitude of the results varies with initial degree of N-limitation. I appreciate that adding an extra GCM simulation, though, is a big overhead. Hence it may not be feasible on the timescale of publishing this manuscript, but at the very least a discussion of the resulting uncertainty is necessary.

2. The emphasis of the abstract is that the inclusion of C-N processes decreases the climate-carbon cycle feedback. Although this is true in your results I don't think it is the most important aspect of the work. I think that carbon-cycle community has be-

come too concerned with a single aspect of a two-sided issue (at least in part due to the (excellent) C4MIP paper). The bottom line question that such modelling studies are attempting to answer is how much CO2 remains in the atmosphere for any given anthropogenic emission. This should clearly take into account the carbon cycle response to increased CO2 as well as to a changed climate. Both of these (the beta and gamma) are important and uncertain, yet the C4MIP emphasis on "gain" focuses mainly on the additional CO2 due to climate change relative to the case with no climate change. It bases the calculation relative to model simulations with just the CO2 effect, even though this is very uncertain across models.

In the current study, the "climate" effect has been reduced due to release of additional N. But at the same time, the "CO2" biogeochemical effect has also been reduced due to N-limitation. The combined effect of these is that the simulation STILL sees a significant increase in the airborne fraction during the 21st century. In fact this second effect outweighs the first in your results and so including N INCREASES AF relative to not including it.

In my opinion this is the aspect which requires emphasis - without it the abstract may become misleading. The present wording (that the climate-carbon feedback is reduced) implies in some way that "including N makes things better" or that "not including N has over-estimated the problem". Whereas in fact this may not be true - any reduction in AF due to a reduced climate effect will be offset by an increase in AF due to a reduced CO2-fertiliastion effect.

The recent paper by Gregory et al (J. Clim., 2009, available early online) discusses that the carbon cycle needs to be considered as these two separate feedbacks (one from climate, one from CO2). I think it is very important for this current paper to address both, and for the abstract to stress the OVERALL impact of inclusion of C-N processes on the airborne fraction, rather than on a single component (the climate one) of the carbon cycle.

You touch on this in the discussion (p.3324, line 7), "overall effect of CN... reduce fraction of emissions stored in land ... compared to no CN". I think this has to be your headline result.

Specific comments:

- p3306, line3. "a net release of CO2" - this is only true relative to the simulation with no climate change. Better to add " or reduced uptake"

- p3306, line 14 "mater" -> "matter"

- p3307, line 17. The positive feedback is not just due to plant growth, but also soil C decomposition. It seems much of the disagreement between models may be in the response of plant growth, but the response (increase) of soil respiration is a crucial component of getting a positive feedback.

- p3308 line 27. "business as usual scenario". Which one? SRES A2?

- p 3310/11 description of spin-up technique. My goodness, you've made this complicated! I couldn't quite follow every detail here, but it seemed that atmospheric CO2 (Ca) was allowed to vary during the spin-up? does that mean that the eventual spun-up state may not have the pre-industrial CO2 level you would want? can you clarify if this is true?

- p.3312, evaluation of forcing factors. This bit is fine, but as a general point I'd advocate the use of prescribed CO2 in future for such breakdown of forcing (as recommended by Hibbard et al, Gregory et al and for use in AR5)... There is a risk that transferring the betas between experiments (in this case with different CO2 levels) leads to a mis-diagnosis of the gamma values. Especially for small values, such as you gamma_ocean here, the effect could be significant. (as it turned out to be in the Hadley model -see Gregory et al for results)

- p.3313. as with reviewer#1 I didn't understand the need for a 120 year running mean?

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- p.3316. again, as with reviewer#1 I would prefer to have seen land-use emissions added too. Or at least a discussion of whether or not you now underestimate the 20th century CO_2 rise (or if you don't then you underestimate the sink strength). At the very least, this difference in experimental design makes a direct comparison with C4MIP harder. If one of your aims was to compare results with those previous ones then use of a different (total) emissions scenario has muddied the water.

- p.3318, line 3. "excellent quantitative agreement" with Thornton 2007. Isn't this the same model? hence not surprising! You shouldn't imply this reinforces your results!

- p.3319. I wasn't sure how you obtained this range of Ca. Are you imposing your simulated changes in beta-L to the C4MIP range?

- p.3321. When you discuss the impact on AF, you could mention a comparison with AR4 figure 7.13 (I think its 7.13 - the one with observed atmos/ocean fractions). It would also be interesting to quantify the present day and future impact of N directly on AF - i.e. what is the difference between simulations Rn and RN?

- don't forget that CO_2 is not the only greenhouse gas. The role of N in the climate system goes beyond just modulating carbon uptake. Does your model deal with N_2O or NO_x emissions from ecosystems? Would you expect climate change to increase these also? would this offset at all the observed decrease in gamma?

- I also found your discussion of possible reduction in uncertainty interesting (and plausible). It occurred to me there is another reason to expect this - your results show that the inclusion of CN means that future storage will be more dependent on T than soil moisture changes. Given that changes in T are more consistent between GCMs than changes in moisture then will this also drive us towards consensus?

Good luck with the paper - I'd be happy to clarify my comments if required. Chris Jones

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