**Geoscientific
Model Development
Discussions**

Interactive comment on "Carbon-nitrogen feedbacks in the UVic ESCM" *by* R. Wania et al.

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We thank the reviewer for a good and thorough review of our manuscript. We provide replies to their questions and concerns below. The reviewer's comments are in *italics*, our responses in normal text.

1 Major comments

I have two major concerns with this study. First, the new part of this study is the introduction of N dynamics. However, the overall discussion in this manuscript is very carbon centric, i.e. the discussions on the role of N in affecting the carbon fluxes and pools are very limited. It is not clear from the manuscript what the mechanisms are

responsible for the difference between CN version and C version of the model.

The main improvement in terms of mechanistic understanding in the CN version is the fact that leaf N concentration – which determines the rate of photosynthesis – is now a prognostic variable rather than a fixed parameter. This implies that in the case of an N deficiency, leaf N concentrations will decrease and reduce photosynthesis rates and hence GPP. The calculation of autotrophic respiration is also affected in the new version: N content in leaf, root and stem – on which autotrophic respiration depends – are now simulated based on stoichiometry whereas in the C-only version the N content of these tissues were derived allometrically.

Another improvement is the fact that N influences decomposition processes in the soil, leading to faster decomposition under higher soil mineral N concentrations.

We will explain these changes at the beginning of the methods section of the new manuscript and where the relevant equations are mentioned in the text. We will also add discussion on the role of N on the C fluxes and pools. One reason why the manuscript is very carbon centric is well summarised by Zaehle and Dalmonech (2011) – due to the lack of global data to constrain the N cycle, CN cycle models often focus on the evaluation of the better constrained carbon cycle.

Second, denitrification, which is the largest output flux for terrestrial N cycle, is ignored in this study. The lack of representation of this process might lead to incorrect estimate of mineral N in soils, which in turn affects the credibility of the CN model here.

A recent study finds that denitrification only contributes 35% to N losses globally, whereas leaching contributes 65% Bai et al. (2011). Nevertheless, we agree that denitrification is an important process within the global N cycle and it will be desirable to include this process in the next version of the UVic-CN model. We omitted denitrification in the current and first version of UVic-CN for two reasons.

1. We followed the approach by Gerber et al. (2010), who also omitted denitrifica-

tion.

2. While denitrification is a microbial process that depends on small-scale environmental conditions, the UVic model has a coarse resolution of $3.6\,^\circ \times 1.8\,^\circ$. Including such a small-scale process into a coarse resolution model would therefore add a significant uncertainty. Zaehle and Dalmonech (2011) discuss the difficulty of modelling denitrification in global models and identify denitrification as both the most uncertain and most poorly constrained part of CN cycle models.

Our aim with this study was to obtain a first-order representation of CN feedbacks and we think that approximating the loss of N from soils via leaching is a valid approach (Gerber et al., 2010). Both leaching and denitrification depend, amongst other factors, on the concentration of nitrate in the soils. Since denitrification is omitted from our model, it can be assumed that nitrate concentrations in the current version are slightly overestimated. However, if nitrate concentrations rise in the model, leaching rates will consequently increase and more or less counterbalance the lack of denitrification.

In terms of an "incorrect" estimate of the mineral N in soils, we are unsure about which number(s) to choose for a "correct" estimate for mineral soils – values in the literature range from $< 1\,\mathrm{Pg\,N}$ to $25\,\mathrm{Pg\,N}$ (Xu-Ri and Prentice, 2008; Lin et al., 2000) and a more recent modelling study estimates the mineral soil pool to be $17\,\mathrm{Pg\,N}$ (Esser et al., 2011). When better global estimates become available they will help to constrain model estimates.

A new paragraph discussing the lack of parameterization of denitrification (and its implications) will be added to the new version of the manuscript.

2   Minor comments

Page 75, Eqs 17 and 18: *the unit for QT is ms-1, so a time conversion factor is needed in the second part of the equations*

We thank the reviewer for pointing this out. We will add a conversion factor.

Page 76, line 3: *the critical soil C/N ratio for immobilization is set as 13, any more evidence to support this (theoretically or experimentally), except for the fact that it is in the range used by other models?*

The global soil C/N ratio in the IGBP-DIS data is 10, with values < 10 in some tropical areas and values > 20 in the boreal zone. These are best estimates for soil carbon and nitrogen based on data. A summary of C/N ratios for soil organic carbon taken from (Zinke et al., 1984) lists values ranging from 13.2 for boreal deciduous forests to 20.8 in temperate deciduous forests (Esser et al., 2011).

We will add the information detailed in the above paragraph to any revised version of the manuscript.

Page 77, Eq. 22: *For the sake of consistency, why not calculate BNF using NPP for both steady state run and transient run?*

We had to use the relationship between evapotranspiration (ET) and BNF in the steady state run (around 5000 model years) in order to retrieve the relationship between NPP and BNF, which we did not know at the beginning of the steady state run. Once we had this relationship, we did not consider it necessary to rerun the entire spin-up procedure, i.e. all 5000 model years, but we ran the model for a short time at steady-state to find a new equilibrium with the BNF-NPP relationship before starting the transient runs.

Page 77, Eq. 23: *I am not sure this linear relationship obtained from steady state can be applied in transient run.*

Modelling BNF is inherently difficult as it is not an easily observable flux and may depend on phosphorous availability and perhaps also other factors such as molybdenum (Zaehle and Dalmonech, 2011), neither of which are well know on a global scale. The variety of approaches used in CN cycle models to estimate BNF (see Table 1 in Zaehle and Dalmonech, 2011) may be read as an indication of how little we know about modelling BNF. We tried two approaches in the UVic CN model. The first was to relate BNF to ET, which led to rather strong N limitation over the 20th century due to the effect of increased atmospheric CO_2 concentration on stomatal conductance and therefore on ET. We therefore opted to relate BNF to NPP as is done in the CLM-CN model (Thornton et al., 2007) and now also in the JSBACH model (Goll et al., 2012).

Page 78, line 7: *was the N deposition data linearly interpolated or interpolated based on the fossil fuel emissions?*

The N depositions were interpolated linearly – we will add a comment in the manuscript.

Page 81, lines 4-7: *GPP is comparable for C and CN versions of the model at steady state run on the global scale. How about at grid cell level? How different are the vegetation C and soil C from the two model versions at steady state (in terms of both distribution and global sum)?*

Global sums of vegetation C and soil C in steady state (Pg C):

Pool	C-only	CN-coupled
Vegetation C	544	651
Soil C	1197	1421

The spatial differences between the C and CN version are higher vegetation C in the CN version mainly in the boreal zone (by 2–4 kg C m^{-2}) and in some tropical areas (1–4 kg C m^{-2}). The soil C is also higher in the CN version by 5–7 kg C m^{-2} in most of the

boreal zone, by 3–6 kg C m^{-2} in mountainous temperate zones and by 1–3 kg C m^{-2} in some tropical and subtropical areas. Soil C losses in the CN version occur in central Europe, eastern China and central United States.

A grid cell by grid cell comparison of the C-only and the CN version result in an $R^2 = 0.74$, $p < 0.001$ for soil C and $R^2 = 0.88$, $p < 0.001$ for vegetation C.

Page 85, lines 17-21: *Since N leaching in this study is actually including N loss from denitrification, the comparison here does not make sense to me.*

We agree with the reviewer and will take this paragraph out.

Page 88, lines 4-10: *more discussion is needed on the difference between UVic-CN and UVic-C results? What are the roles of N dynamics here? Is it possible that the difference shown in Figure 7 is caused by the difference between CN model and C model at steady state?*

Yes, the differences in vegetation and soil C pools between the C-only and the CN version are definitely caused by the differences in their steady states. Instead of showing the 1980–1999 C pools, we could have shown e.g. the period 1800–1819. That would have slightly changed the results and would have shown a slightly larger difference in vegetation C between the two model versions but about the same difference in soil C.

The steady state C pools of the C-only and the CN models are not the same. The CN model was tuned to obtain similar GPP and heterotrophic respiration (HR) to the original model. Under given GPP and HR, the CN version finds its equilibrium at different vegetation and soil C pool amounts than the C-only version.

N dynamics contribute to the changes in C pools through an increased NPP in the boreal zone (Fig. 6e and f) leading to increased accumulation of vegetation C and soil C (Fig. 7c and d).

We will add a paragraph to the new version of the paper to discuss this point.

Page 88, lines 22-25: *this is counter-intuitive. How N dynamics leads to a faster humification rate? The common scenario is that N availability limits decomposition when the N demand of decomposer is larger than possible supply of mineral N.*

The reason for our statement can be seen from a comparison of the expressions for the humification rate in the C-only and CN versions of the model. For the UVic-C version:

$$C_{\mathsf{HUM}} = f(T)f(\Theta)k_{\mathsf{L}}C_{\mathsf{L}}\tau \tag{1}$$

For the UVic-CN version (Eq. (5) from the manuscript):

$$C_{\mathsf{HUM}} = f(T)f(\Theta)k_{\mathsf{L}}C_{\mathsf{L}}(1 + \xi[N_{\mathsf{min,av}}])\tau \tag{2}$$

All variables are explained in the manuscript. Of importance here is the term $(1 + \xi[N_{\mathsf{min,av}}])$ where the parameter ξ is $45\,\mathrm{m}^3\,\mathrm{kg}\,\mathrm{N}^{-1}$ (Gerber et al., 2010) and $[N_{\mathsf{min,av}}]$ is the available mineral N concentration in $\mathrm{kg\,N\,m}^{-3}$. Because Eq. (2) contains the term $(1 + \xi[N_{\mathsf{min,av}}])$ it follows that it will always be equal or greater than Eq. (1) if all other factors are equal.

However, the turnover rate of soil C is *not* influenced by N concentration, again following Gerber et al. (2010), which means that if humification rates (i.e., input into the soil carbon pool) increase but soil C turnover rates (i.e., output from the soil carbon pool) stay the same, it will result in an increase in the soil C pool.

Page 90, lines 8-11: *again what are the mechanisms behind the difference between the two model versions?*

The main mechanism behind the difference between the vegetation C pool in the C-only and the CN version after 1960 is the fact that the growth rate of NPP in the C-only version is greater than the growth rate of NPP in the CN version after 1960 (Fig. 8b). The higher NPP growth rate permits recovery of the vegetation C after 1980 in the C-only version. The lower NPP growth rate in the CN version is caused by the limiting

effect of N availability which can be inferred from the increasing leaf CN ratios (data were not shown). We will add this information to the manuscript.

Page 91, lines 23-25: *what is causing the disappearance of tropical C sink in CN version of the model? N limitation or other mechanisms? It is generally agreed that tropical lowland forests are not N limited.*

We agree with the reviewer that the general opinion is that tropical lowland forests are not N limited. We also do not think that this is the case in our model, as we cannot actually see a N limitation – GPP values correspond nicely to (Beer et al., 2010) in the tropics (Fig. 6a) and NPP values in the tropics are actually higher in the CN version than in the C-only version (Fig. 6d). The neutral NEP in the tropics is due to an increase in heterotrophic respiration in the tropics in the CN version. This increase is caused by a faster rate of litter decomposition due to the inclusion of the effect of N on humification discussed above, but also by the increase of the soil C pool (Fig 7d). We think that this is a good example of how interactive the C and N pools and fluxes in a model can be and that there still exists a lot of uncertainty around the CN feedbacks.

Page 92, line 10-11: *what kind of N effect is referred to here? The limitation effect due to N availability or the enhanced effect on GPP/NPP effect due to increased N availability?*

The effect that N has on the NEP in Figure 9a is caused by an increase in heterotrophic respiration due to mineral N availability. This increase leads to lower NEP despite equal or higher NPP in the tropics as shown in Figure (6d). We will add this information to the manuscript.

Page 95, line 5-10: *The CN version of the model here is considered as an more accurate version of the model, how can we trust the model results if it has an unrealistic representation of NPP:GPP ratio?*

Zhang et al. (2009) use MODIS inferred NPP and GPP data to derive NPP:GPP ra-

tios that ranged from 0.51 to 0.52 between 2000 and 2003. The UVic CN version is actually closer to these results than either of the other two CN models we used for comparison in Table 6. Therefore, we do not consider our model's NPP:GPP ratio to be a less realistic representation. We will include some discussion on this topic to the manuscript.

Model	C-only version	CN-version
UVic	0.50	0.58
CLM Bonan and Levis (2010)	0.35	0.35
ORCHIDEE Zaehle et al. (2010)	0.44	0.43

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