Geoscientific
Model Development
Discussions

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

R. Wania et al.

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

1 Major comments

My main concern is the counter-intuitive finding that including an N cycle representation changes the GPP to NPP ratio to such an extent that global NPP is increased by 10% even though GPP remains unchanged, and that this change seems to be mostly occurring in boreal regions (see Figure 7). I do not think that this is merely a question

of recalibrating the respiration coefficient to match a global ratio of NPP:GPP of 0.5 (see detailed comments below), but a fundamental property of the model that needs to be discussed and its likely consequences for future projections (for which the model is intended to be used) need to be drawn out better. I am slightly concerned that reworking the model to have a more realistic estimate of NPP will affect all of the simulated quantities, and hence reduce the usefulness of this manuscript as reference for future simulations using UVic-CN.

We agree that the shift of the NPP:GPP ratio from the UVic C-only (NPP:GPP=0.50) to the UVic CN version (NPP:GPP=0.58) is undesirable. The next version of the UVic CN version will certainly address this problem. However, the NPP:GPP ratio of 0.58 for the UVic CN version is still in the realm of possible values and closer to 0.5 than, for instance, the CLM-CN model, which has a NPP:GPP ratio of 0.35 (see last comment in reply to reviewer 1). We would therefore not regard our results as less useful than previously published results. In our opinion, this study shows that there is a wide range of possible modelling results given the observational constraints we currently have.

The main reason why the ratio in the UVic CN model differs from the original UVic C-only model is that we did not heavily tune the model. We ensured that GPP at steady state is comparable between the two versions, but did not tune NPP separately to fix the NPP:GPP ratio at 0.5. We decided to leave as much as possible of the original MOSES/TRIFFID code untouched. One of the algorithms inherited from TRIFFID is the relationship between autotrophic respiration and N content in leaf, root and stem (page 86, Eq. (30)). TRIFFID (and consequently the UVic C-only version) uses allometric relationships to derive N content in the different plant organs, whereas the UVic CN version derives the N content stoichiometrically. This means that N content in plant organs have become more realistic, as can be seen in Table 2.

We will add a paragraph to any new version of our manuscript to discuss the validity of our approach. We will also discuss whether it might have been better to tune NPP:GPP ratio to the expected value of around 0.5 and what the likely consequences would have

been.

I have been missing a critical discussion of the results in the light of the ecological understanding of nitrogen limitation, in particular the fact that N limitation seems to be very strong in tropical regions but very weak in tundra regions, which is somewhat counter to current understanding (and some, but not all global C:N models). A critical appraisal of these differences and likely causes would make this a much stronger manuscript and useful reference for future applications of this model.

We will add some discussion to the manuscript along the following lines:

There are a couple of reasons why we do not see N limitation in the boreal forest in the UVic CN model. One is that BNF is relatively high in the UVic CN model. The total BNF in the year 1999 for the BNF-NPP version is 207 Tg N a^{-1}, whereas for the BNF-ET version it is only 162 Tg N a^{-1} and Thornton et al. (2007) use a value of 100 Tg N a^{-1} at an NPP value of 60 Pg C a^{-1}. The derivation of these relationships is described in Section 2.2 in the manuscript.

The other potential reason is that we consider only N leaching but not denitrification as N loss, which may lead to an overestimation of available mineral N especially in boreal regions where leaching rates are lower than in tropical areas (Figure 5b). However, there are two points that indicate that the UVic CN model may not overestimate mineral N due to the lack of loss via denitrification. One is a study by Bai et al. (2011) that estimates that extra-tropical biomes loose 77% of N via leaching (and the rest via denitrification) and the other is the fact that the UVic CN model has a very high amount of N leached from the soil. However, this may be a circular argument since the amount of N leached depends on the N input and therefore this argument comes back to our first one, namely that BNF may provide too much N to the terrestrial ecosystems.

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2   Minor comments

Page 68, Line 13ff: The reference to "temperature" is misleading here. Consider rephrasing to radiatively coupled or similar, as the idea is not that temperature is kept constant, but radiative forcing?

Yes, that is correct. We will change this.

Page 68, Line 20: This is not entirely correct. See comment on NPP and vegetation carbon above. What about stating that N effects in terms of the net C balance and its sensitivity to changes in climate, co2 and temperature are comparable to other CN models?

Good suggestions. We will change this sentence.

Page 68, Line 25: The first sentence does not link to the paragraph, therefore the beginning reads rather strange. Consider rearranging with the second paragraph.

We will add a smoother transition between the two sentences.

Page 69, Line 6ff: It is not clear to me why the Zaehle et al., 2010, GRL study has been omitted from this paragraph.

We will add numbers from Zaehle et al. (2010a).

Page 69, Line 16ff: Spatial resolution is not the major difference between the models (as this largely depends on the forcing data/climate model), but the detail with which important processes are represented. Zaehle & Dalmonech 2011 give an overview on the current existing models.

Thank you for pointing out this paper, we had not seen it by the time we submitted our manuscript. We will point towards the very useful overview of N cycle models in Table 1 in that paper.

Page 70: Please clarify what has been changed between version 2.7 and 2.9 if this

is relevant for this study (or state so if this is not the case). I.e. is it understood why the gamma term has changed between the versions? Also, be more specific about the input data used. Which scenario ,which land-use data etc. The description of the N deposition data might go here as well.

There are many differences between early versions of 2.7 and 2.9 but the largest would be the inclusion of ocean biology and sediments. Ocean biology and sediments have a large effect on ocean γ but probably little effect on land γ. Small improvements in the vegetation and soil carbon distribution are the most likely reasons for the relatively small increase in land γ between versions 2.7 and 2.9.

We realise now that we actually used the wrong γ_L values for comparison in the original manuscript – we took the γ_L from a UVic run forced with carbon emissions and for the year 2100. However, in this current study, we used atmospheric CO_2 concentrations to force the model, which will reduce γ_L compared to using emissions as forcing data. Further, it is important to use the same time period for the calculation of γ_L, which we had not done in Table 7. Therefore, we recalculated β_L and γ_L and compare the values only to Zaehle et al. (2010a) as they provide data for a comparable time period to the one used in this study. We will replace Table 7 of the manuscript with Table 1 presented here.

In Table 1, we see a similar absolute change in γ_L between the UVic C-only and the UVic CN version compared to the ORCHIDEE C-only and CN versions. The relative changes in the UVic are smaller though. There are various factors that influence the response to the CN coupling. One is the absolute size of the land C pool and another is the difference in that pool between the two versions. Another factor is how strongly N limited the ecosystems are and whether an increase in temperature will lead to enhanced availability of N that will increase GPP. In the case of the UVic model, we see less N limitation than in other models, which may explain why the relative difference in γ_L between the C-only and the CN version is not as large as in other models. We will add these new comparisons to the revised manuscript and update the discussion from

Table 1. Land carbon sensitivity to atmospheric CO_2 concentration (β_L) and climate (γ_L) for the UVic ESCM and models that were forced by concentrations rather than emissions and calculated β_L and γ_L for a comparable period.

Model	Period	$\beta_{\mathbf{L}}$ $(\mathrm{Pg\,C\,CO_2^{-1}})$	$\gamma_{\mathbf{L}}$ $(\mathrm{Pg\,C\,K^{-1}})$
UVic C-only	1800–2000	1.23	-102.9
UVic CN	1800–2000	0.77	-86.5
ORCHIDEE C-only (Zaehle et al., 2010a)	1860–2000	1.9	-48
ORCHIDEE CN (Zaehle et al., 2010a)	1860–2000	1.2	-35

page 90, line 26 to page 91, line 14 accordingly.

We will also add a more detailed description of the input data and the land-use change scenario to this section.

Page 71, Line 8ff: This paragraph should mention that the approach follows Gerber et al. apart from the parameterisation of biological nitrogen fixation, which is treated completely differently. I could also not find an explanation of the time step of the nitrogen model. The units given are per year, hence, am I to assume that the time step is also a year?

The time step varies with the respective parallel processes used in the UVic model. Microbial processes, leaching, photosynthesis, leaf turnover and N uptake are updated on an hourly basis. The values are accumulated over five days and fed into TRIFFID, which calculates changes to the vegetation and soil C and N pools and updates CN ratios. We will add this information to the manuscript.

We will also mention that our parameterisation of biological nitrogen fixation does not follow the approach of Gerber et al.

Page 73. *Insert eq 19 and 20 (and the related text on page 76) after eq 14, as otherwise one is left with the wrong impression that the humified soil pool has the same C:N ratio as the Litter pool, which does not seem to be the case. I also have been missing a description as to how the C:N of the soil is determined and whether or not and how it changes over time. I can guess, but a reader should not have to.*

We would like to thank the reviewer for pointing this out. We will rearrange the text to put the relevant information where it is needed. Our intent was to separate organic and mineral pools, which separated the explanation for the immobilisation of ammonium and nitrate from the place of their first use.

We say on page 76 line 3ff that we set the soil CN to a value of 13 following other modelling approaches (see also reply to reviewer 1).

Page 77, Line 7ff: *The Cleveland equation was not designed as a process-description, but as an upscaling method to synthesise different data sets. One should not confuse the correlation between ET and BNF with a process framework that can be applied transiently. The correlation of the Cleveland data BNF data with NPP is very poor if existing at all. I don't think that there is a basis for such a relationship as the one applied, also on grounds that this is a circular argument in which high productive plants fix a lot of nitrogen whereas low productive plants don't, irrespective of their respective nitrogen demands. The consequence of assuming such a relationship should be clearly drawn out (ie N fixation increases with CO2 fertilisation in proportion to the NPP increase).*

We completely agree that the Cleveland equation should not be used transiently, although it has been used by other models (Zaehle and Friend, 2010; Yang et al., 2009) and therefore we also tried that approach. After the spin-up time and our first transient runs however, we realised that relating BNF to ET is not a good idea because of the effect that CO_2 fertilisation has on ET via stomatal conductance in the UVic ESCM. ET decreases during the 20th century and with it so did BNF, which caused N limitation and a reduction in NPP. We therefore used the relationship between BNF and ET only

for the steady state runs and switched to using the BNF-NPP relationship for all transient runs (also used by the CLM-CN model (Thornton et al., 2007) and now also by Goll et al. (2012)). In that case, N fixation increases with NPP increases.

Page 79, Line 5ff: The description would profit from an explanation of the carbon allocation scheme and whether or not its influence by nitrogen. This is particularly important because root C is a factor in the nitrogen uptake equation and if nutrient status would alter root allocation, the plants would possess a mechanism to actively control N uptake when nitrogen stressed.

The C allocation scheme has not been changed in the UVic CN version and the factor driving total plant C is the leaf area index. Carbon is allocated equally to leaves and roots and wood C is related to LAI via two allometric parameters. We will add a sentence to the manuscript to clarify.

Page 79, Line 14: I do not understand reasoning behind the statement that maximal nitrogen concentrations is optimising (ie maximising?) production in this model (see also comment on respiration later). The different temperature functions of respiration and photosynthesis and non-linear dependency of photosynthesis on light should lead to different maximising leaf N concentrations depending on light and temperature regime of a location.

We were not quite clear here. We will rephrase this sentence to "If both CN_{Leaf} and CN_{Root} are at their minimum level, the plant N status is at its maximum and will result in the highest modelled $V_{c,max}$ values."

Temperature and light effects are then used to modulate $V_{c,max}$.

Page 79, Line 21: This section requires a special heading as it deals with the very important competition of plants and soil microbes for N under severe N stress, and this approach is different from a number of other models (and very empirical). This competition is one key uncertainty in these models. The text suggests that nitrogen is

not limiting when CN less than CN max, but surely it is because increasing N availability, thus decreasing leaf CN, would increase photosynthesis until leaf CN reaches leaf CNmin?

We will add a section heading to this part. The reviewer has a very good point here – it is true that N will have a limiting effect already before CN reaches CN max, in fact, a N effect on photosynthesis becomes inevitable as soon as the CN ratio increases from CN_{min}. We will change this paragraph accordingly to make this clear.

Page 80, Eq. 30 *and associated explaining text need to be here as well, as the model description is uncomplete, and one cannot expect for a reader to check the results section for missing equations of the model. The text of page 86 does not explain how Rd is calculated – I assume that this is a fraction of vcmax, but this needs to be explained as the relationship between leaf N and Rd remains unclear.*

Yes, R_d is related to $V_{c,max}$ via the quantum efficiency, which depends on $V_{c,max}$ and temperature. We did not include all of the basic equations of the photosynthesis model in order to constrain the length of the model description but we realise that it will be useful to have the equations included. See also comment below about the rearrangement of Eq. (30).

Page 83, Line 8: *mention your global vegetation C estimate here next to the Saugier number for comparison or refer again to table 6.*

We will do that.

Page 84, Line 1: *give simulated average numbers for litter corresponding to the types and numbers reported here from White et al. This is not evident and obvious from Fig. 3.*

We will correct that. Since there are only five plant functional types in the UVic ESCM, we can make only an approximate comparison.

Page 84, Line 9: *I'm surprised by this problem: How do these numbers actually come*

about? NPP is low here, so BNF should be, and N deposition over these areas is also very small. These high numbers seem an artefact of the initialisation procedure rather than a model outcome.

Yes, NPP and therefore BNF are very low over desert areas, but N deposition is non-zero. The model was spun-up for 5000 model years, over which time natural N deposition could accumulate. We could have reset the nitrate pools before starting the transient runs, but we decided not to since some observations suggest that nitrate does accumulate in deserts.

Page 84, Line 27: The previous section has argued that the vegetation N and CN ratios were reasonable, here it is said that they were higher than in other models. I would agree that the latter is correct (as a simple check of table 6 reveals), but this should then be made clear also in the previous section. Please revise accordingly.

We think that this was a misunderstanding as we cannot see a contradiction in what we said.

On Page 82 from line 12 to page 83 line 13 we describe the differences in vegetation N and CN ratios between the UVic CN version and other models and observations. We show that vegetation N contents are lower and CN ratios are higher in the UVic CN model than in other models. We think that based on a global assessment of the vegetation C and N pool the UVic CN model gives more reasonable total vegetation CN ratios than other models (described on page 83, line 7ff). On page 84, line 27 we repeat that the vegetation C/N ratios in the UVic CN model are higher compared to other models which is in line with what we said in the preceding section. Table 6 also shows that the vegetation C/N ratio in the UVic CN version is high ($635.4/2.94 \approx 216$).

We will add some examples of global C/N ratios taken from other models to the revised manuscript.

Page 85, Line 9: these estimate refer to which year? It is probably worthwhile to

present the loss number as fraction of total input to make the numbers comparable across models, as all these estimates assume different levels of terrestrial N input, hence the absolute numbers are not really comparable.

We can certainly add the percentages for the UVic model for future comparison to other models, but we do not have the data available to calculate percentage data for other models as the amount of BNF for different models is not easily available.

Page 86: Without explanation of Rd, equation 30 does not make much sense. This equation suggests that increasing leaf N reduces respiration, and I cannot understand the reasoning behind this. I assume that Rd is a function of leaf N, but this needs to be clarified.

Yes, R_d is a function of leaf N but also of wood and root N. We will move Eq. (30) on page 86 to the methods section and add the derivation of R_d and its dependence on leaf N there to make this clearer as it is an important part of this paper.

Page 86: I can also not follow the explanation in lines 20ff. On page 79 it was said that woody CN is constant at 330, and nothing is mentioned about changes in allocation due to changes in plant N. On page 86 the text suggests that sapwood N contents vary, which is a direct contradiction to the previous text. Since root and leaf N can vary, and GPP is more or less reasonable (hence probably also leaf N), but Ra is too low globally compared to GPP, the conclusion has to be that root N concentrations are too low. As a consequence, I don't think that equation 30 needs recalibration, but the nitrogen allocation algorithm revision. This at least should be discussed in the revised manuscript.

Sapwood CN ratios ARE fixed to 330 in the UVic CN, but they were NOT fixed in the UVic C-only version. Hence, what we meant – and we apologise for obviously not being clear enough about this – is that sapwood CN ratios in the UVic-CN version are lower than in the UVic C-only version. However, the reviewer pointed out correctly that based on Eq. (30), a lower CN ratio (i.e., a higher N content) in the UVic-CN version

would lead to a higher maintenance respiration R_m, i.e. the opposite of what we have been saying. The fact is that not only is N_{stem} higher in the UVic CN, but also N_{leaf}.

To make this point clearer, we provide an example calculation in Table 2.

From this example, it becomes clear that C/N ratios for leaf and stem in the C-only version were unrealistically high. When using the N contents above and using them in Eq. (30), then we obtain a maintenance respiration that is 1.5-times higher for the C-only version compared to the CN version.

We will add a paragraph to the new version of the paper to explain this point in greater detail.

Page 87, Line 9: *Hickler et al. mention also that the 23% are not representative for tropical and boreal regions.*

We will add this to the sentence.

Page 87, Line 14ff: *The facts are correct, but I don't think that the experiments alone allow identifying a systematic difference between high N demanding deciduous and low N requiring evergreen trees. In fact, N requirements are rather similar for both stands.*

We will replace the sentence with this one: "However, the decrease in NPP in the deciduous sweetgum is not reproduced in the evergreen *Pinus taeda* at the Duke FACE experimental site, which showed a continuous enhancement of NPP by 22-30% (McCarthy et al., 2010)."

Page 88, Line 6: *This finding is very strange and requires more discussion in terms of its ecological realism: tundra and boreal regions are usually subject to very low N availability due to very limited N fixation, therefore one would expect NPP and vegetation C to decrease when N dynamics are accounted for. UVic predicts the inverse, and it is not at all clear how this can work given the equations of the model.*

As discussed above, the N input/output ratio in the boreal zone in the UVic CN model

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Table 2. Example of C, N and CN values for a grid cell in tropical South America derived from the UVic CN-version. Conversion from wood to stem C: We assume that 20% of wood are sapwood/stem (Friend et al., 1993) and that N is 1.3 times higher in sapwood than in heartwood (Meerts, 2002).

Example vegetation characteristics
Coordinates	63°W, 20.7°S
PFT	broadleaved
Height	22 m
LAI	6.436 $m^2\,m^{-2}$
C_Leaf	0.2413 $kgC\,m^{-2}$
C_Root	0.2413 $kgC\,m^{-2}$
C_Wood	15.16 $kgC\,m^{-2}$
C_Stem	3.03 $kgC\,m^{-2}$

In the CN-version
N_Leaf	0.0089 $kgN\,m^{-2}$	
N_Root	0.0055 $kgN\,m^{-2}$	
N_Wood	0.04594 $kgN\,m^{-2}$	
N_Stem	0.0119 $kgN\,m^{-2}$	see caption for conversion from wood to stem.
CN_Leaf	27.1	
CN_Root	43.9	
CN_Wood	330	
CN_Stem	255	

Calculations based on C-only version
N_Leaf	0.0019 $kgN\,m^{-2}$
N_Root	0.0053 $kgN\,m^{-2}$
N_Wood	0.00156 $kgN\,m^{-2}$
CN_Leaf	124
CN_Root	45.5
CN_Stem	1942

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is greater than 1, which means that there is no N limitation for boreal forests. This means that the GPP for the C-only and the CN version are comparable. However, NPP is higher in the CN version compared to the C-only version which is caused by a reduction in autotrophic respiration due to consideration of more realistic N contents in leaf and woody tissue in the CN version than in the C-only version. See above for more details about the autotrophic respiration.

In the revised manuscript, we will discuss how the lack of N limitation in the boreal zone might impact the realism of the model.

Page 88, Line 22ff: *Note that this could also simply due to the higher NPP with N limitation.*

That is true. We'll change that sentence.

Page 89: *This is a very interesting finding that is not reproduced by other global models. E.g Zaehle et al. 2010b only find a very moderate change in the IAV of carbon fluxes, when N dynamics are accounted for. The question is whether the authors think that this behaviour is realistic, and if so, why and based on which evidence?*

This is an interesting point. Zaehle et al. (2010b) shows the interannual variability for NEE fluxes between 1988 and 2002. When we plot the same fluxes (i.e. NEE), we see only a slightly higher interannual variability (IAV) in the CN version than in the original model (Figure 1). It can be seen that the UVic model has a greater IAV than the ORCHIDEE model in general (Figure 4 in Zaehle et al. (2010b)), but we do not see a large effect of the introduction of N into the UVic model.

We think that the reviewer had GPP, NPP and HR fluxes in Figure 8 of our manuscript in mind when making this comment, but Zaehle et al. (2010b) does not show IAV for these variables and therefore we cannot make a comparison.

Page 90, Line 13: *This comparison is not useful, as the lack of a loss is due to Thornton et al. not accounting for land-use change.*

We will take this sentence out.

Page 90, Line 23: are you referring to the ORCHIDEE model (which version of it?) or O-CN (as in Zaehle et al. 2010b)?

The information was taken from Table S1 in Zaehle et al. (2010a) and we referred to the ORCHIDEE-C and ORCHIDEE-CN versions. We'll make that clearer in the manuscript.

Page 91. I would move the heading sensitivity of land C uptake two paragraphs up.

This is a good idea. Thank you.

Page 91. Line 12: gamma is a very time-scale dependent value. I don't think that it makes sense to compare absolute numbers for 2000 and 2350. I would remove those from the text and the figures, and remain with 2000 and 2100 ones (even the 2100 ones are quite a stretch, as the betas tend to reduce over time and significantly so between 2000 and 2100).

We agree with the reviewer and will follow this advice.

Table 7: Remove the entries referring to the year 2100 and 2350, as the gammas and betas are time-scale dependent, and these values are not comparable to the UVic values.

We will do.

Figure 2: Revise the color scale. In Figure 2b only two values are distinguishable, which is not enough to allow for a good comparison to the data.

We will revise the color scale for this figure.

Figure 3: Foliar C:N would be a more useful indicator of N cycling, as it is not confounding changes in leaf C:N with changes in the ration of wood and leaves.

We will add leaf CN ratios as a third sub-figure to Figure 3.

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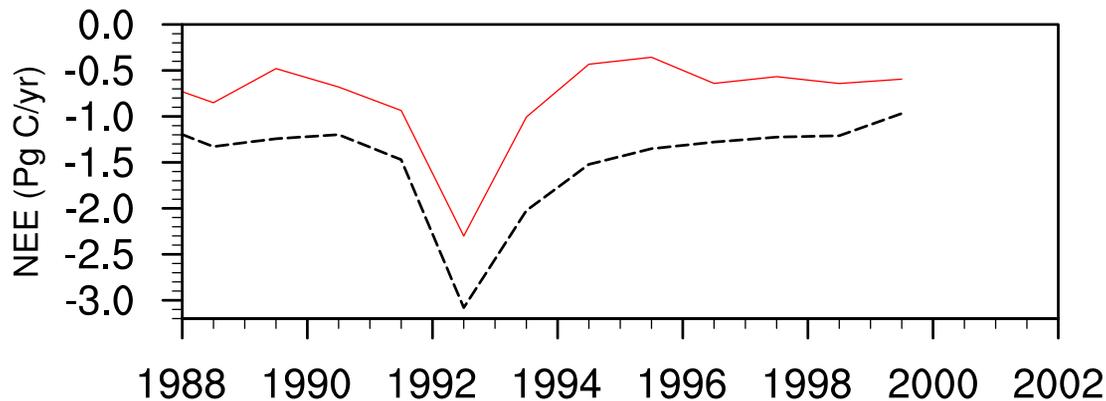Interactive comment on Geosci. Model Dev. Discuss., 5, 67, 2012.

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Fig. 1. Interannual variability of land-atmosphere C fluxes for the UVic C-only (dashed black) and the UVic CN version (red) for a similar period shown in Figure 4 in Zaehle et al. (2010b).