

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

### **Anonymous Referee #2**

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Including nitrogen cycling into land surface models is a very important and timely task in the development of comprehensive Earth system models. The N module of UVic described by Wania et al. is an important step into this direction and the level of detail of this model is of adequate complexity for the UVic model. The model is generally well described (but see my detailed comments below). The manuscript is clearly structured and written, and essential parts of the model outcomes are described and compared to similar modelling studies.

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

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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.

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.

Other than this, I have some minor, mostly editorial 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?

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, CO<sub>2</sub> and temperature are comparable to other CN models?

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.

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

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

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the current existing models.

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.

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?

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.

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 CO<sub>2</sub> fertilisation in proportion to the NPP increase).

Page 77, Line 22: 66 Pg C a-1

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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.

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.

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<sub>max</sub>, but surely it is because increasing N availability, thus decreasing leaf CN, would increase photosynthesis until leaf CN reaches leaf CN<sub>min</sub>?

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 v<sub>cmax</sub>, but this needs to be explained as the relationship between leaf N and Rd remains unclear.

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

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.

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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.

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.

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.

Page 86: Without explanation of  $R_d$ , 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  $R_d$  is a function of leaf N, but this needs to be clarified.

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  $R_a$  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.

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

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.

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.

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

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?

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.

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

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

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).

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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.

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.

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.

Reference: Zaehle S, Dalmonech D (2011), Carbon-nitrogen interactions on land at global scales: Current understanding in modelling climate biosphere feedbacks, *Current Opinions in Environmental Sustainability*, 3, 311-320; 310.1016/j.cosust.2011.1008.1008.

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