

Interactive comment on “Implementing the Nitrogen cycle into the dynamic global vegetation, hydrology and crop growth model LPJmL (version 5)” by Werner von Bloh et al.

Anonymous Referee #1

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Overview

von Bloh and co-authors present a N enabled version of the LPJmL model that couples terrestrial biogeochemistry and biogeophysics in a dynamic vegetation model. They evaluate global (and regional) changes in terrestrial C and N dynamics over the 20th century and compare their findings with other literature estimates. The work marks an important development for the community of LPJmL users, and I encourage the authors to revise their manuscript to make it more relevant for a broader readership interested in representing global biogeochemistry in land models.

Major concerns

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I appreciate that this is largely a model development and documentation paper, but found some of the discussion surrounding main display items rather hasty and lacking appropriate depth. For example, the spatial patterns of N limitation (Fig. 8) suggest that both tropical and boreal forests are not N limited (indeed, forests globally may have a low sensitivity to N availability)? This is just one example, but on revisions I would encourage the authors to unpack and explore their findings a bit more. Take the opportunity to call out strengths and weaknesses in the current approach and discuss particular model assumptions that are responsible for these features. This doesn't have to be exhaustive, but will help add depth to the results and discussion.

Page 1, Line 6. Significant improvements in crop yields are not apparent in the updated Fig. 10 and SI material uploaded by the authors. Would a plot of global crop yields vs. observations make this point more clearly? If crop model improvements are the big advancement in the current model development, I'd encourage more attention be given to establishing these improvements in the main text. That said, the estimates of N losses (through leaching and N₂O emissions) also have important regional and global consequences and seem to be done well in this version of the model. Should these accomplishments be highlighted in the abstract too?

Minor comments and technical concerns

P1, L 8. This suggest the is still scaling occurring in regions with favorable climate and N inputs? Is this true?

P1, L 18 Zaehle and co-authors (2015) made similar findings, seems worth citing here?

Throughout section 2 is it worth briefly distinguishing the similarities and differences in the approach taken with LPJmL vs. other land models, especially LPJguess (Smith et al 2014) which is referenced throughout?

P4, L 6. It seems odd to introduce table 2 before table 1. Similarly, table 4 is introduced before table 3 (page 7).

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Section 2.2. Is the soil biogeochemistry for this version of the model vertically resolved, as implied with e.q. 7? If so should this be mentioned in section 2: model description?

Eq. 13-14, could pft specific root distribution parameters be easily described in the current tables or included elsewhere?

P5, L 27. As written this sounds like rates of GPP are reduced by respiration rates? This strikes me as strange. Wouldn't autotrophic respiration be subtracted from GPP to calculate NPP rates (eq. 18). This also isn't clear in Fig 1.

Figure 3: check to see the colors for each arrow are labeled correctly and/or defined. Alternatively, the approach here seems pretty standard, I wonder if the distracting rainbow of flux arrows are really necessary?

Section 2.6 How are all the competing fates of inorganic N solved (e.g. sequentially, simultaneously, etc)?

Section 2.6. Are litter and SOM pools have a fixed C:N ratio or are they flexible (w/in bounds).

P 15 L 23 What is a "nodulating leguminous crops pulses", the phrase seems redundant? Maybe just use "soybeans and pulses".

P17, line 13. What happens to the other 40% of the manure? Is it not really applied, or does it go into SON pools?

Section 3. What spatial resolution are these simulations? Does each grid cell have a single pft, or is there subgrid variability of vegetation? If there is subgrid PFT variation, do all plants share a soil column, or to they each have individual columns (that is, does manure and fertilizer applied stay on crop only soils, or is it available to plants throughout the grid)?

Table 4: Although they are described in the main text (section 3) the abbreviations for experiments used in column headings are non-intuitive enough to prevent the informa-

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tion contained in the display item from standing on its own. Consider adding text to the the table heading or columns to make these data more understandable.

Table 4. I'm surprised global NO₃ pools are an order of magnitude larger than the NH₄ pools. I wonder if there is spatial structure to these patterns (e.g. accumulation of NO₃ is warm or arid regions), or if the patterns is relatively globally distributed. Regardless, it seems surprising given the relatively high mobility and multiple loss pathways of NO₃, compared with NH₄, and suggests that nitrification rates may be too high in the model? Alternatively, decomposition rates may be high, supplying excess inorganic N, or plant NO₃ uptake may be underestimated? This may be worth mentioning in the discussion (section 4.1.1 or 5)?

P 18 L 8 Why are agricultural lands (that are presumably being fertilized) becoming increasingly N limited? Is there some metric of N limitation that can illustrate this point more directly, as it's not intuitive from Fig 4a,b. Also, it seems odd to increase carbon use efficiency (NPP:GPP) if the system is becoming more N limited? I see how it occurs in the model, because of higher tissue C:N ration and lower RA costs, but is it ecologically realistic?

Fig 4. How were relative GPP changes calculated, I didn't see this described in the text? Also, consider adding information about line colors to the figure caption, as the legend insets are very small and hard to read.

Fig. 4 It looks like the two control models (3.5 and 5) lose vegetation and soil C throughout the 20th century, but GCP data suggests the land surface should be a C sink, at least over then end of the 20th century (e.g., LeQuere et al, 2015). Given increases in plant productivity in Fig 4a,b- this suggests the land use C change flux must be pretty large?

P 18 L 15. Where is the data showing that N limitation increases by a factor of two? How is N limitation being assessed in this statement?

P 18 L 16. What's causing the higher leaching losses with the control model? Does it have to do with vegetation demand for N, rooting profiles of managed vegetation, or other factors?

Table 4. Its it worth discussing limitations (or uncertainties) of some of the 'observational' estimates presented here

Fig 6. Agreement on crop C:N ratios doesn't seem that surprising, given the ranges for leaf C:N and allocation that are proscribed in the model (Tables 1,2) . I'm assuming the values for R3 in Table 2 were tuned to provide the spread shown in Fig 6? This is fine, but should be acknowledged. In the text.

Fig 7a, it strikes me as odd to have low values (<1) indicative of high leaching losses, especially when points above the 1:1 line show areas of high leaching under present vegetation (fig 7b)

Fig 9, where do the obs come from- especially for NPP and Veg C. Is each point supposed to represent an individual sites? This are from the same FluxNet sites as in the SI figures? Also, where do the observational error bars come from & how were they calculated? Finally, should correlation coefficients & significance be reported?

Section 4.1.3 Where are the LAI data shown that the addition of N biogeochemistry supposedly fixes? If this is the big advancement with the model presented here should these data also be shown? Is it just the addition of N biogeochemistry that's responsible for the proported improvements, or were other parametric or structural changes made?

Fig. 10 A bunch of questions: What are the residuals and how are they calculated? What are units for the y-axis (what is the 'FM')? What are the little numbers in the top of each panel showing? Finally, it looks like global maize production increases over the period shown but the models are all flat. What's driving the increase in yields that the model is apparently missing? Is this true for other major crops?

P 26 L 4, What improvements are necessary? What additional complexity may improve

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things further? What data are critical to getting terrestrial C-N dynamics less wrong? As presented these are kind of empty / throwaway statements. Can the be flushed out with some more detail, both in the main text and in this summary conclusion?

References: Le Quéré C, et al. (2015) Global Carbon Budget 2015. Earth System Science Data 7: 349-396. DOI:10.5194/essd-7-349-2015

Smith et al. (2014) Implications of incorporating N cycling and N limitations on primary production in an individual-based dynamic vegetation model, Biogeosciences, 11, 2027-2054, doi:10.5194/bg-11-2027-2014.

Zaehle, et al. (2015). Nitrogen availability reduces CMIP5 projections of twenty-first-century land carbon uptake. Journal of Climate 28.6: 2494-2511.

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