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Interactive Comment

Interactive comment on "Implications of incorporating N cycling and N limitations on primary production in an individual-based dynamic vegetation model" *by* B. Smith et al.

B. Smith et al.

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Author response to comments by anonymous referee #1

We thank the referee for constructive feedback and respond to the specific comments below.

"1) To the authors' credit, they do a reasonably good job of addressing the N fixation algorithm in the Discussion section, but I have to agree that this is a weakness in the model. Tying biological N fixation to evapotranspiration rates may work fine at very broad and coarse geographical scales (largely because ET is related to moisture and temperature, which are two important controls on N fixation), however we know that

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this will not hold across landscapes, or even regions, where N fixation is a function of soil N and P, as well as species composition. I think this should be one of the very next enhancements to the model, but I do commend the authors for not ignoring this issue"

Author response: We fully agree that the parameterisation of biological N fixation represents an important uncertainty in our model, and global C-N models in general. We discuss these limitations in some detail on p18638,L26-p18639,L16 and understand both referees to be satisfied with our treatment of the question there.

"I was also curious (as this wasn't clear in the paper), at what temporal scale in the model is N fixation tied to ET, i.e. is daily N fixation calculate from daily ET, or is this done at coarser temporal scales?"

Author response: Annual N fixation is calculated following the Cleveland et al. relationship, based on a 5-year running mean of simulated actual evapotranspiration. The annual value is distributed evenly over the days of a year. This is explained in Appendix C on p18648,L10-17.

"2) Just to clarify (page 5, lines 15-17), gross N mineralization is determined by the C:N ratio of a receiver pool, which I think means that carbon entering the receiver pool drives N mineralization. So, if C and N are being transferred from donor pools to a receiver pool, the amount of available C (and the prescribed C:N ratio of the receiver pool) determines the N to enter that pool. If the supply is greater than the demand, then N is mineralized. If demand is greater than supply, then N is immobilized, assuming mineral N is available. Maybe this can be clarified in the description."

Author response: We have modified the explanation of how mineralisation and immobilisation occurs in conjunction with the transfer of C and N between pools around the passage referred to by the referee on p18619 as well as in Appendix C on p18649 where the information is repeated as part of the detailed description of the model. The new formulation makes it explicit that carbon transfers between pools drive N mineralisation and allude to the supply/demand analogy suggested by the reviewer.

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“Also, what happens if N amount required to meet the C:N ratio is not present in the mineral N pool?”

Author response: in this case, decay rates are reduced until net immobilisation matches the pool of available mineral N. We have added this information on p18649.

“3) The plant N uptake algorithm assumes no luxury consumption of N – how reasonable is this assumption?”

Author response: some luxury consumption is permitted, up to the limit dictated by the maximum capacity of the plant N storage pool. This is explained on p18652,L14-p18653,L4.

“4) The assumption that plants retain half of the N in shed roots and leaves, and the conversion of sapwood to heartwood is extremely general. N resorption represents a large pool of N for plants, and this parameter could be much better constrained”

Author response: we agree that a fixed value for plant N retranslocation represents a potential source of structural uncertainty in our model. The assumed value of 50% comes from the widely-cited review by Aerts (1996, J. Ecol. 84: 597-608), who concludes that N resorption efficiencies of 47 and 54% are representative for evergreen and deciduous trees, respectively. A more recent review by Vergutz et al. (2012, Ecol. Monogr. 82: 205-220) suggests that N resorption efficiency should be higher, around 60%, but this higher value results from accounting for mass loss in conjunction with turnover, which is not represented in our model. Other C-N models, e.g. O-CN (Zaehle & Friend 2010, Global Biogeochemical Cycles 24), make the same assumption of an N resorption efficiency of 50% for all PFTs. We are aware of no coherent theory for how retranslocation fraction may vary among climate zones/biomes/functional types for natural vegetation or what the drivers of such variations might be. We now cite the above-mentioned papers as authorities for the 50% mean resorption efficiency assumed by our model.

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“One other model question that I had was with regard to C4 grasses. The model assumes that C4 grasses are constrained to areas with a coldest mean monthly temperature of 15°C. This seems to me to be grossly incorrect. There are locations, where C4 grasses are about half of the productivity, that have coldest mean monthly temperature less than 0°C. This could be a factor in some of the model misclassifications”

Author response: The assumption that C4 grasses are limited to regions with a coldest month temperature of 15 degrees or higher is retained from the original version of LPJ as described in Sitch et al. (2003, Global Change Biology 9: 161-185). To our knowledge the same rule has been retained in all published studies with LPJ and LPJ-GUESS, and can be motivated by reasons of comparability with earlier studies. While this assumption results in a reasonable overall distribution of C3 versus C4 grasslands at global scale, we acknowledge that the fixed temperature limit should ultimately be relaxed to allow a dynamic partitioning based on competition between C3 and C4 grasses in regions of overlap. However, this does not work “out of the box” and is retained as an issue for future development in the model.

“Related to this, on page 17 (lines 4-9), the authors state that the mechanism for increased CO2 fertilization effects with decreasing latitude (increasing temperatures) are due to suppression of photorespiration, however in C4 grasslands, photorespiration would already be minimal”

Author response: The reviewer is correct, that the general pattern of increasing CO2 fertilisation with decreasing latitude is associated with suppression of photorespiration in C3 vegetation (including tropical forests and the woody component of savannahs) but does not apply to the C4 component of tropical vegetation. We now make this explicit on p18633,L12.

“1) Page 2, line 14 – “Cramer’s” should be “the Cramer”

Author response: changed as suggested

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“2) Page 4, line 26 – “effecting” should be “affecting”

Author response: No, we meant “effecting” = “causing to occur”. To avoid confusion we have substituted “resulting in”.

“3) Throughout document – “savannah” should be “savanna”

Author response: “savannah” is an accepted spelling in the Oxford English Dictionary.

“4) Page 21 – line 12 – “necessary” should be “necessarily”

Author response: changed as suggested

“5) Page 21 – lines 27-29 – remove the sentence”

Author response: this study alluded to in this sentence is now published in Biogeosciences Discussions. We have added a reference to this paper.

Interactive comment on Biogeosciences Discuss., 10, 18613, 2013.