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Interactive comment on “A simple parameterization of nitrogen limitation on primary productivity for global vegetation models” *by* G. Krinner et al.

G. Krinner et al.

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Reviewer 1.

Increased root allocation is indeed a possible response to increased CO_2 levels. However, increased root allocation will not necessarily relieve N limitation if the mineral N pool is depleted. Competition for N might become stronger, but the total N uptake would not increase. In principle, increased root allocation could be taken into account in the model, as the allocation scheme we use (Friedlingstein et al., 1998) does calculate root allocation as a function of the limitations undergone by the plant - for the moment, as a function of humidity and temperature, the latter being a surrogate for N mineralization. Apart from the fact that unambiguous observations of increased root allocation are lacking, the question is what point of complexity the N limitation scheme should attain. We think that there is no point in increasing the complexity of the scheme

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beyond what is the case now, because in that case, one might as well go for including an explicit N cycle in the model - which, in turn, may not be timely yet, because “we still lack key knowledge of how to properly simulate C-N interactions, especially in a transient environment.”, as referee 2 states. Moreover, we want the parameterization to be transposable to other models. Therefore, we tried to limit the representation of the effect of PNL on one specific part of the model. We modified the discussion (subsection (“ Formulation of the scheme ”) in response to the reviewer’s remark.

In the humid equatorial forests, N limitation is generally not observed at present. Our model does simulate the onset of progressive N limitation by the 1970s or so in these ecosystems. This means that there is clearly a misfit. One reason is that N fixation is strong in these ecosystems, which seem to be unique in this respect. Indeed, we decided to keep the model as simple as possible, without taking into account features specific to certain ecosystems or regions. But in principle, “external” N sources (that is, other than microbial mineralization) could be accounted for in our scheme either by

- Adding a fraction of the NPP of equatorial evergreen trees (dominant PFT in the humid equatorial forests) to the same upper term, yielding something like $a = \frac{R_h + xNPP}{NPP}$. This term would then account for the presence of N fixing species in these ecosystems; the variable x would, in a more general way, be close to zero in boreal systems and have a higher value in the the equatorial lowland rainforests;
- Adding a source term S to the upper term in the formula used to parameterize the nitrogen availability a, yielding something like $a = \frac{R_h + S}{NPP}$. This source term could then, for example, represent N deposition due to human activities, and would apply in Europe and similar regions.

However, this would add additional degrees of freedom to the parameterization, and it would be fairly hard to come up with justified values for the additional parameters. As stated above, one might then as well go for an entirely prognostic N cycle - but then the remark by referee 2, which was cited above, applies again.

Another reason for the model misfit might be an overly strong simulated increase of photosynthesis by the tropical evergreen tree PFT in response to the prescribed CO_2 increase, or an overestimated time lag in the response of heterotrophic respiration in these regions to increase litter deposition. Because these latter possibilities cannot be ruled out, and because progressive N limitation in the equatorial lowland ecosystems cannot be ruled out for the future, it seems preferable to acknowledge the model misfit in a revised version, discuss potential reasons for it, and indicate how the particularities of the equatorial lowland ecosystems could in principle be addressed. We modified sections 5.1 and 5.2.3 accordingly.

We made no attempt to simulate limitation of productivity by other elements, such as phosporous. In principle, such limitations could also be taken into account in a simple scheme similar to the one we propose here, by defining an availability such as $a = \frac{S}{NPP}$, where S would be a (probably constant, or weakly climate-dependent) source term. This is now clearly stated in section 5.1.

Reviewer 2.

Referee 2 also states that modified carbon allocation could lead to a decreased N limitation. Our remarks given in response to referee 1 therefore apply.

Referee 2 then states that the heterotrophic respiration as a basic index of N supply might be misleading in many cases. Of course, as soon as a basic, simplified index is sought, one has to accept the fact the it might not be the best index in every special case. This is what happens, for example, in the equatorial lowland forests. However, on a global scale, we see no other reasonable candidate for being this basic index than the heterotrophic respiration R_h. The current understanding of the continental N cycle is that N mineralization by decomposition of organic soil matter is the main source of N in most natural ecosystems on time scales of interest here. This naturally leads to chose R_h as the basic index of N supply in such systems. In the revised version, we now state more clearly the reason for this choice in the section where the model is described, and refer to the discussion where the important issue of equatorial lowlands is treated.

Referee 2's remark on N limitation in equatorial lowland forests is similar to the remark of referee 1 (see reply there). We are somewhat surprised that referee 2 categorically excludes the possibility of N limitation in these ecosystems in the future. The fact that N limitation is frequent globally at least points to the possibility that this could also occur under certain circumstances in equatorial lowlands. Our model simulates onset of N limitation by the 1970s, which is in error, but we still maintain that a similar effect might occur in the not too distant future (neglecting climate change, that is, drying, which could compensate for the CO_2 increase in Amazonia), for the reasons underlying our understanding of progressive nitrogen limitation. We discuss this issue in two places now ("Formulation of the scheme" and "Degree of realism of the model results - Global simulations: Comparison with C sequestration and productivity data").

Interactive comment on Biogeosciences Discussions, 2, 1243, 2005.