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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.

Anonymous Referee #2

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Krinner et al address an important issue in this paper – namely, that current global vegetation models do not include the ability to simulate nutrient constraints on the C cycle. They correctly point out that a full, mechanistic representation of the N cycle remains a longer-term goal for such models, and one that cannot be met quickly. Thus, they argue that a "stepping-stone" towards the longer goal could be approaches such as the one they employ: i.e. a simple, non-mechanistic forcing of model structure to include an estimate of nutrient effects. In this case, they attempt to simulate the possible effects of progressive N limitation. That's done by using the ratio of "long-term" NPP to heterotrophic respiration as an index of plant demand relative to microbial supply.

I applaud the authors' goal of incorporating nutrient constraints into GVMs; without such constraints, their estimates of C cycle dynamics must be interpreted with signifi-

cant caution at best. However, I believe that the approach taken here does not advance our understanding of C-nutrient interactions, nor does it result in a model that is a more reliable prognostic, or even diagnostic, tool. To begin with, forcing an N limitation when NPP exceeds respiration is a blanket assumption that may not always occur, is somewhat of a circular approach, and does not contain even a basic attempt to address some key mechanisms in the N cycle that will ultimately determine N constraints over C storage and loss. Instead, it's a simple mathematical way to generate a response in some ecosystems that we know to occur, but not necessarily for the right reasons. Of course if NPP exceeds N supply for a sustained period of time, the system will run into increasing N deficits. But an NPP$>$Rh does not necessarily mean the system is hitting increasing N limitation quickly, or even in the relative long term. For example, a simple change in allocation could lead to a sustained ration greater than 1 without a significant increase in N limitation for a substantial period of time. In addition, while I appreciate the difficulty of trying to come up with a basic index of N supply in a way that can work in a GVM structure, the basic assumption of Rh being that index is likely to be misleading in multiple cases, even when using time-relaxed averages such as those used here.

Most importantly, in my opinion, we have multiple ecosystem models with far more sophisticated N cycling routines, and even those do not always do a great job of capturing N-C interactions – the authors themselves point this out. In other words, we still lack key knowledge of how to properly simulate C-N interactions, especially in a transient environment. The highly variable responses of decomposition to changes in N availability are but one of many examples of our uncertainty (something that is not sufficiently stressed in this paper). Thus, to propose a model structure that is clearly too simplistic and potentially misleading, regardless of the well-accepted need to improve GVMs in this regard, is to me a dangerous game. Simple models can be enormously useful diagnostic tools, and the paper suggests that this approach may fall in that realm. But that's not what this model is. Rather, it's a fairly complicated model with a very simple add-on that, as stated above, does not lend new mechanistic insight, or improved

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confidence in predictions.

Finally, the paper discusses responses of the tropics in the global simulations, suggests a “simulated impact [of the N limitation paramerization] that is very strong. . . ”, and even makes some comparisons to latitudinal gradients in potential terrestrial C sinks. Yet, the paper shows no recognition of the fact that N limitation is probably not widespread in large portions of the tropics. Rather, in many tropical forest regions, N often appears in relative excess while P, Ca and/or K are a greater nutrient constraint. Thus, the simulations are producing N-mediated effects on tropical C exchange in regions where N may not be limiting, either now or in the future, and yet these are regions with globally very significant C fluxes. Thus, my opinion is that while the “N limitation” approach taken here should probably not be employed in any system, it should certainly not be imposed in a blanket way in tropical latitudes.

In sum, I wish to restate that I sympathize with and support the author’s primary goal of incorporating nutrient feedbacks into GVMs. However, I believe that the approach taken here is not the best way to go, and has the potential to produce misleading results.

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