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Interactive comment on “Modeling the effects of organic nitrogen uptake by plants on the carbon cycling of boreal ecosystems” by Q. Zhu and Q. Zhuang

Anonymous Referee #1

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Biogeosciences Review (Zhu and Zhuang 2013) Modeling the effects of organic nitrogen uptake. ... Over the last couple of decades the idea that plants in natural ecosystems may derive a portion of their annual nitrogen (N) requirement directly from uptake of organic N (ON) compounds has gained some traction. Experimental and observational studies of soil N dynamics, plant N uptake kinetic characteristics and plant growth in the field and laboratory settings present a consistent, albeit more complex, picture of plant – N relations in natural ecosystems. These findings notwithstanding, incorporation of the new insights to terrestrial N cycling have largely been omitted from ecosystem models, despite the importance of nitrogen availability as a driving variable in these models. This paper by Zhu and Zhuang represents one of the first delightful

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steps in assessing the possible effects of incorporating ON uptake in these models and the consequences for NEP and other aspects of carbon cycling. The authors coupled an ON uptake algorithm into the process-based Terrestrial Ecosystem Model (TEM) by informing the model with soil amino acid dynamics and root ON uptake kinetics. They also performed a very interesting sensitivity analysis to evaluate the relative importance of the parameters driving the model of ON uptake for both an arctic tundra site and boreal forest site. Great. Since I am no modeler and thus have a little understanding of the innards of TEM, I had difficulty evaluating the consequences of the assumption that plants take up inorganic N (IN) first and, if not sufficient for survival and growth, absorb amino acids to make up this deficiency. I suggest you delete the reference to the rather tricky issue of plant survival. Low N availability can certainly decrease growth, but seldom kills the plant outright! More importantly, however, is the assumption of temporal separation of IN and ON uptake. It seems to me that because IN and ON vary in space and time, root – nitrogen encounters should vary likewise. Given what (we think) we know about plant ecophysiology it seems more parsimonious to assume that IN and ON uptake is controlled by their relative supply rates (root-ion encounter rate) without invoking an absolute preference between these forms of N. Briefly discuss how this assumption may affect the model output, particularly the estimates of the relative contribution of ON and IN to total plant N uptake. ON-TEM clearly identified the potential importance of ON as a fraction of total plant N uptake (36-87% for the tundra site; 26-50% in boreal forest). Do these results imply that TEM (-ON) may have underestimated total plant N uptake by an equal magnitude – with sundry effects on GPP and NEP? Moreover, did the increased uptake from ON scale to increased NEP in a reasonable fashion? Briefly discuss - and take into consideration the issue raised in the previous paragraph regarding preference. Equation 4 (Model development) describing changes in vegetation nitrogen (as plant IN and ON uptake minus N loss in litter) appears incorrect. Please clarify if the first minus sign in the equation is a typo or if this expression indeed was incorporated as written into the model runs. Results of the sensitivity analyses were surprising. These results should have received

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more discussion insofar as the analysis identified a plant factor (maximum root uptake) as the primary control over plant ON acquisition, rather than the hitherto prevailing idea that edaphic characteristics (e.g., diffusion rates) primarily exert such control (e.g., Tinker and Nye 2000). Such intriguing findings warrant a great deal more attention. One issue of concern regarding the generality of this paper pertains to the source data for parameter estimations (as acknowledged in the text by the authors). Whereas I have no problem allowing models to go forward with moderate amounts of data, or off site data for that matter, I was somewhat perplexed by the absence of ecosystem-specific data employed here. Surely a wide suite of information from the primary literature on arctic and boreal ecosystems could have been accessed: Amino acid concentration data (Kielland 1995; Nordin et al 2001; Weintraub and Schimel 2006; Werdin-Pfisterer et al. 2009), protease activity (Weintraub and Schimel 2006; Kielland et al. 2007), as well as plant uptake kinetics for a variety of species (Kielland 1994, 2006; Persson and Nasholm 2003). This is all water under the bridge at this point, of course, but I was somewhat flummoxed by their omission. These limitations notwithstanding, the paper opens up to question a range of ecological issues which warrant further discussion. My hope is that the authors will take some of these comments to heart and elaborate on these fascinating, albeit tentative, conclusions regarding ON uptake for NEP and other ecological characteristics of arctic and subarctic ecosystems.

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