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Discussions

Interactive comment on "Carbon / nitrogen interactions in European forests and semi-natural vegetation. Part II: Untangling climatic, edaphic, management and nitrogen deposition effects on carbon sequestration potentials" *by* Chris R. Flechard et al.

Anonymous Referee #1

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General remarks: Magnani et al. (2007) reported very large responses of forest carbon sequestration to nitrogen deposition. Several authors rapidly pointed out that the response proposed was way above previous estimates and direct observations in N addition studies. This apparent discrepancy has been discussed at length for more than a decade now, but there is still a need for a more stringent analysis of how dC responds to dN. The effort made in this manuscript is, therefore, most interesting and commendable. However, this model analysis is very complex. Many hours of careful

reading is needed to get an insight into how the model is constructed and how it handles the critical assumptions involved. A reader will also need to read the companion paper. Most readers will still be left with many queries. This is not uncommon in the case of modelling papers. Vital assumptions are deeply embedded and not clearly visible although the outcome is constrained by the assumptions. In fact, trust in the many reputed authors, rather than the apparent quality of the manuscript, drove me to read it once again. Could these complex matters and their analysis be made more understandable and transparent, respectively? I am not sure how, but would like to ask the authors to do their utmost. Hopefully, the comments below will be helpful when revising the ms. The treatment of the relations between Ndep and the internal forest N cycle is pivotal. A step ahead here is the use of local data on Ndep where possible and not just regional estimates. As regards the internal N cycle, the authors do not appear (e.g., lines 264-265) to handle that organic N sources (chiefly amino acids and peptides) are used by plants and probably dominate in less fertile systems, especially boreal forests. Inorganic N sources become dominant when the N supply is large relative to the biological demand. The authors may also reflect on the trends of decreasing leaching of inorganic N from forests in NE USA and N Europe. What in their models could drive this phenomenon? It could be related to higher tree growth (more C) in response to management or environmental change? How should C be coupled to N? On p. 15 potential net effects of N supply on C sequestration efficiency are discussed. The authors mention that C sequestration in a high C/N component like wood would be one explanation (used also by Magnani et al.) among the many complex and non-linear interactions between N and C. Is it at all possible and in line with findings in N-15 tracer studies that the majority of the N added goes into wood? The answer from many studies appears to be no (e.g., Nadelhoffer et al. 1999). This calls for an analysis of the physiological processes in which interactions between the cycles of N and C are particularly important. Modelling is necessary, there is no doubt about that, but it needs to make best use of all the data available including recent findings. These are many, but the authors could perhaps consider some, which describe non-linear biological controls

(e.g., Kallokoski et al. 2013, Tree Physiol. 33, 1145- , show that wood cell formation is similar in N-limited and N-fertilized trees during early summer, but then cease in the former but continue in the latter, and Högberg et al. 2010, New Phytol. 187, 485- , show that tree belowground C allocation is greatly reduced by additions of N; such relations may be interconnected). I would suggest that the authors rethink and reword a part of the reporting of results (lines 387-393). Firstly, forests 30-60 years are not young, especially not in Central Europe, in the sense that they have a low demand for N because of a low biomass as stated by the authors. Older forests may have a larger biomass for sure, but this is because of their trunks, tissues with much less biological activity and N demand than foliage and fine roots. On the contrary, 30-60 yrs-old forest most probably have fully closed canopies and a very high demand for N. Secondly, the idea of such forests leaching more N because of less canopy interception of water (and hence greater runoff), is also unlikely. Check with hydrologists if they see more runoff from forests 30-60 yrs-old than from older forests! Moreover, foresters would describe forests < 30 years old as young; in the context of rotational forestry in Europe 30-60 yrs-old forest are middle-aged.

More specific comments: Lines 73-74: Shouldn't this be phrased the other way around: "... with no further C uptake response at high Ndep levels (Ndep > 2.2-3 g m-2 yr-1) followed by large N losses by leaching and gaseous emissions." Line 140: add fires and insect attacks here. Line 145: in some regions, e.g. in N. Europe, there are many N-fertilizer experiments. Line 216: you write even, but maybe mean seven? Line 266-268: the use of another definition of NUE is widespread; I understand that you want to use an acronym, but it is unfortunate to use one that commonly has another meaning. May I also suggest that you use Nmob = N mobilized, rather than Nmin, which means that you overlook organic N compounds as N sources. Line 373: do you have clear evidence that Nmin does not change over time? Other authors discuss N oligotrophication and report that runoff of mineral N from forest decreases. Line 402: it is interesting to learn in which direction the non-linearity develops. Line 525: BASFOR may be mechanistic, but the vital interactions are not discussed and clarified in the

description of the model. Line 595: it is unclear if internal N supply is a component of soil fertility. Line 616: more thorough discussions about optimal allocation theory, especially C-N interactions, are found in, e.g., Franklin et al. (2012, Tree Physiol. 32, 648-). Line 716: It is OK to cite Fog here, but why not cite authors, which discuss similar phenomena in forests (like Berg & Matzner 1997 Environ. Rev. 5, 1-). Lines 743-744: below-ground autotrophic respiration does not exactly follow photosynthesis, but is also affected by seasonality in C below-ground allocation (Högberg et al. 2010 New Phytol. 187, 485-). Line 780: what is the difference between fertility and nutrient availability in this context? All texts to Figures and Tables should be self-explanatory. Thus, acronyms should thus always be explained in these texts. Figures 3-6 and 8: these take some time to comprehend. A reader will need some guidance. And the text in the boxes are difficult to read and understand. Figures 9 & 10: the texts by the symbols are difficult to read as sometimes they come on top of each other.

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