**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "C allocation among fine roots, above-, and belowground wood in a deciduous forest and its implication to ecosystem C cycling: a modelling analysis" *by* M. Campioli et al.

Anonymous Referee #1

Received and published: 24 October 2008

This paper aims to develop and test a new carbon allocation model applicable to deciduous forest stands. Unfortunately, the paper falls short in a number of aspects.

1) The new model is not adequately described. Not all equations are given the processes, and where they are (in Appendix A), the logic behind them is not explained. In many cases, it is completely unclear how a certain process was represented in the model. For example, about fine root growth the authors note that "It depends on specific flush rates, current standing biomass, root-leaf ratio, and annual leaf biomass maximum." It is also not clear to me how this model can both

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

incorporate sink hierachy as well as allometric ratios (which is explained, strangely, under "growth efficiency", but without details) in determining C allocation (using allometric relations to grow compartments does not require sink hierarchy and vice-versa). A more thorough description of the model might have shed light on this.

2) A number of debatable assumptions are made in the model that are not well defended. Particularly, the model uses phenological stages, each of which lasts a fixed number of days. Fine root mortality seems to only take place in the summer (only in stage 3?). Turnover seems to not take place at all in the autumn period.

3) It would help to cite original publications (that contain experimental data), rather than previous modelling studies (such as Bossel) or general reviews (such as Lacointe, Le Roux). For example, maintenance respiration is calculated with the method of Penning de Vries (1975), but instead a 2005 modelling study is cited.

4) The model is not adequately tested. A carbon allocation model would ideally be tested against biomass proportions, but only stemwood increase is shown. For this comparison, the authors note that "CAF slightly overestimated wood production in 2000 and 2004", even though overestimations were 20 and 16%, resp. (Table 3). A test against GPP is really irrelevant, as many more processes (and hardly allocation) go into estimation of GPP. Why was there no test against leaf biomass (or LAI)? How does the model predict shifts in allocation with tree size, and how does this compare to other models and/or data? Does the model improve allocation estimates over a null-model, which uses fixed allocation ratios?

5) The allocation model is mixed with other model components, so that it not only describes purely the process of C allocation. Specifically on page 3786, line 2, the authors state that "We simulated C allocation using four modules: (i) autotrophic respiration, (ii) phenological development, (iii) assimilate allocation and biomass growth, and (iv) biomass losses". Sure, the actual predictions of the amount of C allo-

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

cated to each compartment will depend on biomass losses (turnover), respiration rates, etc., but these processes are more inputs to the allocation routine than really part of it.

Interactive comment on Biogeosciences Discuss., 5, 3781, 2008.