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Biogeosciences Discussions

Interactive comment on "Coupling carbon allocation with leaf and root phenology predicts tree-grass partitioning along a savanna rainfall gradient" *by* V. Haverd et al.

Anonymous Referee #2

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The manuscript by Haverd et al describes a new coupled carbon-water scheme, HAVANA, that is designed to provide biomass increment data to a previously developed vegetation dynamics model, POP. The approach follows standard modeling approaches for partitioning carbon to leaf, root and stem state variables, based on growth, turnover, functional relationships between leaf and sapwood area, and resource optimization constraints for leaf to root allocation. The model is applied to the North Australian Tropical Transect to test whether the assumptions can correctly predict tree to grass cover gradients in relation to precipitation. A novel aspect of the model is the treatment of non-structural carbohydrates (NSC), which create a temporal shift in carbon uptake and growth.

The manuscript is well-written and the ideas clearly presented. The title and introduction suggest that the manuscript will provide a more detailed understanding of tree-grass partitioning in savanna regions, but the rather standard implementation of carbon allocation scheme and the presentation of the results as more as a benchmarking exercise results in the manuscript being somewhat overall technical.

To address the technical issue, would it be possible to run a 'no-fire' scenario. For example, it would be useful to know how much of the tree-grass partioning along the transect is due to the correct dynamic allocation scheme versus simply being driven by the diagnostic fire information. Secondly, the model is set up to evaluate co-existed for just one woody and one grass functional type – I wasn't clear whether the parameters are global, or whether they vary along the transect. Is it possible to add a sensitivity test or vary the traits along the gradient to also illustrate their importance?

Because the implementation of the NSC is rather novel, I would like to see some more detail in the Results that presents the size of the NSC pool, the size of the NSC pool relative to the leaf/root/stem pools, and some discussion of how realistic this might be (i.e., what happens when the NSC pool becomes too large, if this occurs)? Also, why is the NSC pool not treated as a state variable, i.e., in Section 2.2.

Regarding the water budget, i) does the saturated volumetric water content consider the difference between field capacity and wilting point (perhaps add to Table 1), ii) is the soil evaporative layer considered to be equal the upper soil layer?

The timesteps could be more clearly presented for GPP, NPP, and allocation.

Is there a reproductive cost on GPP?

In the Section 4, I wasn't able to follow which parameters were calibrated – a short list would be helpful.

Interactive comment on Biogeosciences Discuss., 12, 16313, 2015.