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Interactive comment on "The Impact of a Simple Representation of Non-Structural Carbohydrates on the Simulated Response of Tropical Forests to Drought" *by* Simon Jones et al.

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Jones et al. describe a simple model of NSC dynamics which is aimed at incorporation in large-scale ecosystem models, particularly JULES. They evaluate the model based on observations at a throughfall exclusion experiment in the Amazon and provide a demonstration of how the new model formulation affects carbon fluxes at the scale of the Amazon. Dealing with NSC storage dynamics in large-scale models is of clear conceptual importance for capturing tree mortality and intra-annual carbon fluxes, but presents a significant challenge given the lack of parameterisation data. Simple, flexible representations of NSC dynamics, such as that presented here, are therefore needed to underlie progress in ecosystem model development as knowledge of this topic develops. The paper is very well written, and the introduction in particular is excellent, giving a clear, well-structured and thorough overview of the topic and problem. Overall, this seems a very nice development, but I was particularly missing discussion/testing of some important assumptions.

Reflecting the lack of observations of NSC stocks and dynamics, the model is designed to be parameterised based on more commonly-available measurements such as biomass and GPP. This is an elegant idea, however some fairly substantial assumptions are made to achieve it, and those assumptions are not well explored here. One key assumption, made in Eq. 8, is that the NSC dependence of maintenance respiration is the same as that for plant growth. The implicit hypothesis is that respiration and growth are equally prioritised under resource limitation. As far as I am aware, there is no clear evidence to support this and it is just as likely that the plant should down-regulate growth to maintain respiration-related functions. No reasoning or citations are presented to support this key choice. In my opinion a thorough discussion is necessary.

I am also a bit curious about the assumption implied in Eq. 3 that NSC in the heartwood is available to trees to use. Is it posited that there are mechanisms that trees can use to extract this NSC from dead wood, in which case citations are needed, or is this simply an assumption to enable use of the widely available total biomass information? The latter would seem perfectly reasonable to me, but then does seem to warrant a short discussion about the fraction of total NSC that is typically found in heartwood (e.g. <15%, Richardson et al., 2015).

Similarly, there has been clear effort to minimise the number of parameters, but choices of parameter values are not well justified in the text. Parameters a_{Km} and q_{10} are assigned default values with no justification given for the choice. In particular, a_{Km} appears to be central to the results. If clear, strong, justification for the choices cannot be given, then the sensitivity of the results to these parameters should be included in the tests. The parameter f_{NSC} is appropriately treated as a range, although I would have been inclined to set upper limit of the range a bit above that actually observed in a tropical forest to explore the parameter space a bit, but fair enough!

Pg. 4, lines 11-12. So this means that SUGAR assumes that trees actively allocate to storage to maintain a certain store size. Perhaps acknowledge this decision explicitly here, given the active debate on this (which was introduced in the introduction)?

Pg. 7, line 8. Is this above-ground NPP or total NPP? If the former, how is the output from SUGAR being adjusted to compensate for this in the evaluation?

Pg. 7, line 16. How is the CV calculated? Annual mean of daily (or monthly) values, followed by taking the mean across the simulated years? Or directly over the whole dataset? I'm trying to understand if this is showing intra- or interannual variability.

Pg. 8, line 31. Doesn't this imply that the NSC store has been underestimated? Perhaps worth exploring how much storage would be required to maintain respiration and growth and how this is affected by parameters like a_{Km}? This could provide a useful hypothesis for future investigation.

Pg. 8, lines 42-44. Maybe a bit of overinterpretation of small differences here? Overall JULES and SUGAR seem equally good for the control plots in Figs. 5 and 6.

Pg. 8, line 51. Whilst the downregulation of respiration in response to depleting NSC may help buffer NPP, it clearly doesn't improve the simulation of respiration, which is more strongly underestimated in SUGAR than in JULES from 2005 onwards. I think this discussion needs to reflect that whilst SUGAR provides a very useful representation of a process we are confident is important, including this process does not by itself radically improve the overall carbon flux simulation for the drought experiment investigated.

Fig. 5. It would be helpful to also see timeseries of respiration and NSC storage to allow a full and balanced interpretation.

References

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Discussion paper

Richardson et al. (2015) Distribution and mixing of old and new nonstructural carbon in two temperate trees, New Phytologist 206(2), 590-597.

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