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Biogeosciences Discussions

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. present a new storage model that could be readily incorporated into LSMs, allowing them to decouple growth from photosynthesis in line with emerging experimental evidence. This manuscript tackles a very interesting scientific question, is well written and the results are clearly presented. I think with revision this manuscript will make a nice contribution to the literature. Below I've outlined a number of small issues. I have a few larger issues with the current manuscript.

1. I felt the details on how the model was parameterised could be improved. I strongly felt the paper needs a table with parameters used, which would make this paper repeatable.

2. I felt a number of the plots were a little redundant. In fact, if you run their code, I'd say the timeseries of NSC, perhaps with the addition of changing water availability, would be more insightful as to how the model works.

3. I was a little bothered about how different the implementation of NSC actually was from LSMs that assume excess GPP goes into a labile pool, which is then used for growth/respiration? If I'm doing the authors a disservice here then I apologise, but perhaps a few more words outlining this distinction are required. I guess the bigger point I'm making here is that I was anticipating clear hypotheses about *how* and *when* such a labile pool would be used. I do not see these. For example, does the plant aim to maintain a minimum labile pool? What sets this? How big a pool does it accumulate? How would these things vary between PFTs? In the same way, what about the timing of utilisation? The authors spend the introduction sets up a clear link to water stress and this model as a plausible buffering mechanism. But the treatment of water stress in the manuscript is insufficient. It occurs to me that the authors are assuming that a plant will regulate GPP in the same way with and without a NSC pool (this is implied by the offline implementation). But does this make sense? The details are not given, but presumably in JULES water stress reduced the assimilation rate via reducing Vcmax. But if you have a NSC pool, would that imply a plant might be a little riskier? If it has some stockpile, why be quite so sensitive to water availability? I have nothing to support this line of thinking, but it seems pretty testable and logical (if only to me!).

4. As I said above, the results are pretty convincing, but they are also indirect. There is nothing to support the model results being for the right mechanistic reasons. We have nothing that shows us the NSC timeseries (not shown) is supported experimentally from the throughfall experiment. We have nothing to say the respiration from this pool is supported experimentally. In both cases, I suspect a reader will anticipate such plots, I certainly did. Do such data exist? I have no idea if they do or not.

5. Finally, the authors put forward an argument that the NSC model results at the throughfall experiment are limited by the poor representation of water stress on GPP in JULES. This is testable. All the authors have to do is make the GPP reduction less sensitive to water stress and plug these GPP values into their model. My suspicion is that the agreement with the obs may not improve, but I might be wrong. It would be worth testing this rather than speculating.

Merry Christmas,

Martin De Kauwe

Introduction ————

* To be honest, I found the whole first paragraph completely unrelated to the focus of the paper. I think the paper would really benefit from a more relevant opening paragraph, entirely up to the authors what they do here, just a suggestion.

* Pg 2, ln 24: "and so plants rely heavily on their NSC reserves". Are there any numbers to support this statement? How heavily? For how long?

* Pg 3, ln 41: what is the evidence for carbon starvation leading to mortality? My understanding is that it is essentially non-existent outside of a few potted experiments? See for example, Adams et al. A multi-species synthesis of physiological mechanisms in drought-induced tree mortality. I think this could be more carefully phrased.

* Pg 3, Second paragraph "Despite...". Obviously biased, and feel free to ignore...but I will draw your attention to "Mahmud et al. Inferring the effects of sink strength on plant carbon balance processes from experimental measurements. Biogeosciences.", which I think is a nice attempt to exploit experimental data to help mechanistically unpick the role of sink control. I highlight this paper because the focus was specifically to aid model development - "This can largely be attributed to a scarcity of ecosystem-level data (NSC content and distribution) that can be used to parametrise and evaluate models for a range of species and climates that covers all plant functional types (PFTs)

used in LSMs".

Methods ——-

* Is there any supporting evidence for the assumptions in SUGAR? It's is fine if there isn't but it might be nice to cite some relevant literature if there is. For example, section 2.2 ...

* In 2.5, it would be useful for the parameter ranges to be given to the reader? The section is titled parameter estimation but I've got no idea after reading it what values were used. I think a table with assumed parameters and/or ranges would be very useful for a reader who wished to repeat any of this. Currently, the only defined terms are the Q10 and Yg. For example, in the results: "All other parameters (Yg, aKm , q10) are kept constant at their default values (see model description)." Where was the value of aKm given?

* I think the methods would benefit from a few sentences/paragraph explaining how the model works beyond the equations. Most of the introduction set up an interpretation of the use of NSC during periods of water stress but this theme is not returned to once in the methods. How does water stress interact here? It clearly isn't directly, but just comes about due to growth demand? What about the timescales of utilisation or storage increase? My reading of the model is that there is no specific hypothesis being tested here about increases in NSC. It is simply the difference between C uptake and utilisation. I don't really see that this goes beyond what many LSMs currently assume with respect to a excess carbon storage pool. I was expected a hypothesis about how plants might aim to maintain a storage pool, which I do not see. Equally, something about how they might prioritise a draw-down of this pool.

* Pg 6: "optimised so that annual GPP and NPP in the control forest agree with observations." What specifically was optimised here?

* Surely the SUGAR model was simple enough to just embed in JULES. Some further

explanation is warranted here as to why this wasn't just done...

Results ——-

* As a general statement, I found it odd to start with the spatial interpretation rather than a site-level analysis. Doing it this way round is harder to see how the model is really working and to me (at least), it would make more sense to reverse the presentation of the results. Or alternatively, it would be useful to see a timeseries extracted from Fig 2. For example, "This decline in seasonal variation is caused by an increase in dry season carbon expenditure and a decrease in the wet season carbon expenditure.". It would be nice to see this...

* I understand Fig 2 is a sensitivity experiment, but how are we meant to interpret whether the SUGAR model is improving / degrading growth predictions? I can see that increasing the fSNC dampens the variation, but is this dampening supported in any way? I wonder if Fig 3 is strictly necessary? It seems to be implied by Fig 2, I feel like you need one or the other. Perhaps it is a supplementary figure. I'd much rather a few timeseries plots!

* The implication of Fig 4 is that the respiration assumption in the model becomes more important as fNSC increases. How sensible is the assumed respiration eqn...this seems quite important.

* In the first paragraph of 4.2, it would be good to explain why JULES NPP and SUGAR NPP differ at all? If there is no water stress where does this difference come from? Do the models have different respiration assumptions? A shift in timescale of growth?

* Following the sensitivity experiment in Fig 2 onwards, my interpretation was that it was therefore not obvious how to parameterise the model. As such, I was expecting to see some form of uncertainty envelope around the SUGAR model line in Fig 5? How was SUGAR parameterised in this set of runs? I found this very unclear in my head at this point of the manuscript.

* With Fig 5, arguably you don't need panel b, you could perhaps then include a respiration comparison between JULES and SUGAR? No idea what that looks like...

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