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Interactive comment on "Inferring the effects of sink strength on plant carbon balance processes from experimental measurements" *by* Kashif Mahmud et al.

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Overall Review

The manuscript uses a data-assimilation technique to combine an essential but well thought model of carbon balance and plant growth with observations of photosynthesis, maintenance respiration, changes in biomass and NSC obtained in seedlings of Eucalyptos tereticornis planted in containers of different volumes and freely in the soil. The original experiment of Campany et al 2017 reproduced indeed different degrees of sink limitations. The data-assimilation technique allows the authors to infer time dynamics of model parameters (e.g., allocation fractions) and to quantify the relative

importance of different processes in downregulating plant-growth under sink limitations. The article shows that the reduction of photosynthesis rate due to sink limitation is not sufficient to explain the reduction of plant growth since other adjustments in NSC utilization, allocation dynamics, and modified respiration and leaf turnover rates are playing an important role. The inclusion of a NSC storage pool and the capability to account for sink limitations emerge as key model components if results of the experiment are to be reproduced. While the path toward modeling plant growth under sink-limitations in mature ecosystems and under various environmental conditions remain long, this contribution is surely an important advancement in the right direction. The article is very well written and presented and most important it is very novel with comparison to the existing literature. As far as I know, it is the first time evidence of carbon sink-limitations is presented so markedly and modeled in a realistic context. In summary, I am very positive concerning the content and conclusions of the article. I think the manuscript is making a very important contribution to the field and I sincerely congratulate the authors for this nice piece of work. In the following, I just have some minor comment that can be helpful to improve further the presentation of this work, especially the comments: P.16 Line 352-358 and P.19. Line 416.

Sincerely,

Simone Fatichi

Minor comments

P.2 Line 42. The reference "Bonan 2008" is not present in the reference list and if the authors refer to the article "Forests and climate change: forcings, feedbacks,and the climate benefits of forests. Science 2008, 320:1444–1449." I do not think it would be an appropriate reference here. I would rather search for something more related to "plant-growth and forest-growth modeling" rather than something related to Earth System Models.

P.2 Line 53 . I would suggest to add also Paul and Foyer 2001, very relevant here.

Paul MJ, Foyer CH. 2001. Sink regulation of photosynthesis. Journal of Experimental Botany 52: 1383–1400.

P.3. Line 67. It must be "Fatichi et al 2014".

P.5. Line 146. Maybe this is not a case that has been encountered in this article. However, how does the model work when NPP is negative and therefore maintenance respiration is larger than carbon assimilation? Is maintenance respiration generally subtracted by the non-structural storage or is it done for each of the tissue separately?

P.5 Line 171. The relative amount of NSC in the roots appear to be a very small number, 9% of the total, while generally one would expect a significant amount of non-structural C-storage in roots especially for seedlings and grasses. Do you have any explanation for this?

P.8. Line 202-206. How are you dealing with the heterogeneity in photosynthetic properties among leaves and among plants? Were they significant? I know that you wrote in Line 211, that you use the mean for each treatment; is this the mean of how many replicates? Did you average the photosynthetic and stomatal model parameters (Vc-max, g1, . . .) or the A-Ci and stomatal conductance values?

P.9 Line 222-223. If I am reading correctly there are 18 (3x6) coefficient to determine for each treatment, maybe this can be written explicitly to compare with number of measurements (44 points) in line 231-233. This allows some redundancy even in the case of separating each container size.

P. 9. Line 243-247. Please explain better this part of the data assimilation methodology, as it is now it is not very clear to me.

P. 14. Line 319. There are not "bold values" in Table 3. Probably a formatting issue.

P.16 Line 352-358. The lowest utilization rate in seedling in small containers would theoretically lead to an accumulation of NSC, at least in relative terms, which is something we do not see in Figure 2 and Figure 4. The explanation for such non-intuitive results is

only provided in the discussion (Line 505-510) and justified as a temporal effect, where NSC first accumulates in seedling in small containers but then they are depleted by the higher respiration costs and leaf turnover rates. I think it would be quite interesting to see in Figure 2, NSC (Cn,f) reported as fraction of total C mass in foliage (Ct,f), e.g., Cn,f/Ct,f. This would serve the double purpose of explaining such a different dynamic in the use of NSC as the season progress and will provide the percentage of NSC in leaves that can be compared with other studies. This will likely highlight a higher concentration in seedling in small containers at the beginning of the season but a lower concentration at the end (as in Figure 4).

P. 18. Line 401-403. The authors for some reason never refer to the concept of Carbon Use Efficiency (CUE), but I suggest it would be useful here to explain the results. Substantially what they are saying is that CUE = (1 – (Rm,tot+Rg)/GPP) is higher in free seedling and it is reduced by sink limitations. Maybe, a figure showing the temporal evolution of CUE for the various treatments would be also an interesting piece of information.

P.19. Line 416. I am not sure if parameters were changed one at a time resetting the previous parameter to the original value or if effects are added up (which seems more the case from the presentation of results). If parameters are really changed "one at a time", this does not allow to reproduce all the interactions among parameters and can inflate the role of certain parameters. Therefore the total effect of Table 4 (54.8 gC plant-1) does not necessarily correspond with the real total effect, which is not reported for comparison. If the parameters are switched on sequentially then you will obtain the total effect but the importance of certain parameters will not be clearly separated, since it will depend on the adopted sequence of switchers. For instance, the role of "k" would be likely smaller when the interplay with the other parameters is considered. Now, I am not asking to running simulations with interactions among parameters since they would be an extremely high number (going factorial) and they will not add much to the overall discussion on the model results. However, this simplification and the specific method needs to be stated explicitly and the difference in the total effect between the simulations changing parameters "one at a time" and the total observed effect needs to be mentioned, since it can provide an idea of the importance of the interaction among parameters.

P.20 Line 445-451. Table 4 and Figure 5. Following my previous comment, I wonder if it is not better to show the effect of each parameter independently rather than the sum of the effects of the parameters on the final biomass. I think it would be better to show the effect of each parameter by itself on the baseline rather than what is shown now. In any case, a clearer explanation of what is shown would be necessary.

P. 23. Line 471. I think with regards to the importance of the storage pool in models, Fatichi et al 2016 would fit well here. Fatichi S., C. Pappas, V. Y. Ivanov (2016). Modelling plant-water interactions: an ecohydrological overview from the cell to the global scale. WIREs Water, 3(3), 327-368, doi: 10.1002/wat2.1125

P.25. Line 550-552. Another way to say the same concept is that CUE is higher in free seedlings.

P. 26. Line 586. Karst et al 2016 is not in the reference list.

P. 27. Line 605-610. I agree with the authors, but there is still an important challenge of dealing with sink limitations in ecosystems encompassing tall-trees and heterogeneous vegetation types and for which observations for data-assimilation may not be available.

P. 29. Line 675-677. I thank the authors for referencing to my work, but this article is completely irrelevant for the current paper and indeed is just quoted by mistake.

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