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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.

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Here we address comments by 2 anonymous referees and Editor Brian Amiro. We attach a pdf file indicating changes that have been made to the text.

Anonymous Referee #1

Comment 1.1

The simulation results suggest that resource limitation is the driving force behind the tree grass dynamics of northern Australia. This finding is in agreements with a number of recent Australian reviews and studies and adds to the international discussion

on savanna function. It would be interesting to consider the driving processes in this model (parameter sensitivity) and impact of various assumptions on the simulation outcomes. This could also allude to where the Australian setting differs from international savannas and how the global model could be achieved.

Response 1.1

We have included the following modification in the discussion : "Our findings support the view that Australian savannah tree cover is primarily controlled by availability of soil resources rather than disturbance. Simulated tree cover emerges from the balance between production – controlled by resource availability – and turnover, controlled both by resource availability and the frequency and intensity of disturbance. In our simulations for Australia, a high proportion of biomass loss was attributable to resource-limitation. Of the total biomass lost to mortality, 68 % was attributable to resource-limitation at the wet northern end of the NATT, increasing to 84 % at the arid southern end. The remaining minority of biomass turnover was attributable to disturbance loss, largely from fire. However, the approach of HAVANA-POP remains valid for regions in which savannah vegetation structure is controlled by the disturbance regime."

Comment 1.2

P16320 line 7. (and page 16318 Line 18). It is not clear in the text (without reading detail of Table 1) how the depth of the two layers was defined. Was the 20cm top layer cut-off defined from the soil description obtained from the soil maps, or is it related to the expected rooting depth of the native C4 tropical grasses? As grasses only have access to the top layer, an increase in this layer depth will affect grass growth. Likewise, the 4m depth of lower layer seems deeper than most soil descriptions from northern Australia (Soils Atlas) and this value will significantly influence tree biomass and population dynamics. Please state the basis for the depth cut-off as these are critical values and vary considerable in the literature with grasses reported from 20cm to > 1m. How would changing the depth of the bottom layer between 1 and 6 m influence the tree

population and biomass? These soil properties need to be justified in order to validate the output of the simulations.

Response 1.2

We agree that the model is sensitive to these depth parameters. We have clarified that these have been sourced from the literature by inserting the following text in Section 2.1: “The two state variables of the water balance model are soil water stores (W1, W2) [m-water] corresponding to upper and lower soil layers, the boundary between them corresponding to the approximate vertical extent of the grass root profile (Janos et al. 2008), and the total depth that of the tree roots (Hutley et al. 2000).” Further, we have included the appropriate references for the soil depths in Table 1.

Comment 1.3

P16324 line 13. It is not clear whether multiple tree and grass species were simulated or only the tree/grass functional groups. A description of the type species could be provided.

Response 1.3

We have clarified that species were not distinguished by inserting the following text in Section 2.2: “A single set of parameters was adopted for all trees and all grasses respectively: species were not distinguished. “

Comment 1.4

Page 16330 line 25. I do not understand how the regular exposure of the vegetation to fire results in curing of C4 grasses. Surely, exposure to fire will result in combustion of grasses and low soil water is the cause of C4 grass curing.

Response 1.4. We agree that this wording was wrong and we have omitted the phrase altogether.

Technical corrections

Comment 1.5

Page 16324 line 5. “by proportion a” should this be alpha as per equation?

Response 1.5

Done

Comment 1.6

Page 16330 line 22. Woodlands rather than wood-lands as per grasslands and shrub-lands.

Response 1.6

Done

Comment 1.7

Page 16331 line 22. Is DINGO presented elsewhere that can be cited? The following paragraphs could then be condensed.

Response 1.7

No: at the time of writing there was no citable reference for DINGO. DINGO developer, Jason Beringer, is a co-author on this paper.

Comment 1.8

Page 16333 line 15. (see Table 1 for calibrated parameters)

Response 1.8

The following sentence has been inserted “The parameters subject to calibration are shown in Table 1.”

Comment 1.9

Page 16339 line 10. Please define LSM.

Response 1.9

Now done at first usage.

Comment 1.10

Page 16348 Table 1. Lines for Ga, kE,w and KL,g could have source on single line to reduce size.

Response 1.10

Done

Comment 1.11

Page 16352 Figure 2. Consider adding “flux site abbreviations. . ..” to caption as per Figure 3.

Response 1.11

Done

Comment 1.12

Page 16354 Figure 5. Is there an extra space between y and -1?

Response 1.12

An issue of figure rendering: to be fixed in final figure submission.

Comment 1.13

Page 16354 Figure 5. Replace square brackets with round brackets for Williams et al 1996 for consistency.

Response 1.13

Done

âĂČ Anonymous Referee #2

Comment 2.1

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 treegrass 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.

Response 2.1

Although we haven’t performed a “no-fire” simulation, we used our results to quantify the relative roles of fire vs resource limitation on biomass turnover, and found that the effect of fire is relatively small and is not largely responsible for tree-grass partitioning along the transect. This has been emphasized in a modification to the discussion: “Our findings support the view that Australian savannah tree cover is primarily controlled by availability of soil resources rather than disturbance. Simulated tree cover emerges from the balance between production – controlled by resource availability – and turnover, controlled both by resource availability and the frequency and intensity of disturbance. In our simulations for Australia, a high proportion of biomass loss was attributable to resource-limitation. Of the total biomass lost to mortality, 68 % was attributable to resource-limitation at the wet northern end of the NATT, increasing to 84 % at the arid southern end. The remaining minority of biomass turnover was attributable to disturbance loss, largely from fire.”

Comment 2.2

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.

Response 2.2

The parameters are global and do not vary along the transect (see Response 1.3 above) , although the known dependence of specific leaf area with mean annual precipitation is incorporated (Equation 1.38). It is an interesting idea to vary parameters along the transect, but it is out of scope for this paper. Moreover, in the interests of parsimony it is desirable for parameters to be globally applicable.

Comment 2.3

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.

Response 2.3

We have clarified that the absolute size of the NSC pool is unknown , when we first describe it in Section 2: "Dynamics of vegetation carbon stores are governed by growth and turnover rates. Growth is constrained to be equal to net primary production (NPP, equal to gross primary production minus autotrophic respiration) in the long term, but

is temporally dependent on the size of an implicit NSC store, soil water availability and the deviation of the structural carbon store from an internally computed carbon carrying capacity, above which growth stops and NPP is stored away as NSC. No assumption is made as to the absolute size of the NSC store; rather it is the cumulative deviation due to the imbalance between NPP and growth that is maintained as a state variable of the model."

Further, we have included NSC in the state variables listed in Section 2.2: "The state variables of the vegetation model are carbon pools in leaves and fine roots of trees and grass, and the woody carbon pool (stem plus coarse roots) in trees, and the cumulative difference between NPP and growth, which equates to the deviations of NSC for trees and grasses from an (unknown) baseline value."

The change in NSC storage is shown in Figure 6b(i).

We avoid instances of the NSC pool becoming "too large" by constraining growth and NPP to be equal in the long term (Equation 1.21)

Comment 2.4

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?

Response 2.4

(i) No: the saturated volumetric water content parameter is not associated with wilting point or field capacity. (ii) The formulation for soil evaporation is given in Equation 1.12: it depends on the moisture content of the upper soil layer.

Comment 2.5

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

Response 2.5

We have inserted the following text in Section 2: “The time-step is one day.”

Comment 2.6

Is there a reproductive cost on GPP?

Response 2.6

No, although this could be added to the growth respiration cost (Equation 1.22).

Comment 2.7

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

Response 2.7

Please See Table 1. In Section 4, we have added the following text: “The parameters subject to calibration are shown in Table 1.” âĂČ B. Amiro (Editor)

Comment 3.1

Pg 16317, lines 3 and 4. The term “resource uptake surfaces” is not clear; I understand this as a modelling term, not something that species respond to.

Response 3.1

We explain what is meant by resource uptake surfaces: “Species respond by producing resource uptake surfaces quickly to optimise uptake of the most limiting resourceâĂŤleaves to capture light when soil water is abundant, fine roots to increase water uptake as supplies deplete”

Comment 3.2

Pg 16317, line 8. Repetitive to say “stored” then in “storage”.

Response 3.2 “in storage” has been removed to avoid repetition.

Comment 3.3

Pg 16318, lines 3 and 4. Define ESM as Earth Systems Model?

Response 3.3

Done

Comment 3.4

Pg 16318, line 8. Normally we would have Haverd et al 2013a cited before Haverd et al. 2013b.

Response 3.4

Not done: this is not easily fixed with reference software.

Comment 3.5

Pg 16324, line 5. Remove “a” after “proportion”.

Response 3.5

Done: See Response 1.5 above.

Comment 3.6

Pg 16327, Equations 26 and 27. I am having a hard time reconciling units in these two equations. It seems that the second term will have the same units in both equations, but the primary terms have saturation deficit and temperature, respectively?

Response 3.6

Thank-you for pointing out this mistake: an error in the typed equations only, not the model code. The denominator in the second term of Eq 1.26 has been corrected so it is down dimensionally consistent.

Comment 3.7

Pg 16329, line 14. Haverd et al 2012 is not in the reference list.

Response 3.7

Fixed.

Comment 3.8

Pg. 16331, line 20. I think that the Isaac reference will likely get published much behind the current paper, so is not useful here. Can it be removed?

Response 3.8

Done.

Comment 3.9

Pg 16332, line 26. fPAR is usually the fraction of PAR used by plants. Did you mean something different here?

Response 3.9

No, we didn't mean something different. The term "Vegetation fractional cover" has been removed

Comment 3.10

Pg 16334, line 4. Where did the values of 50 and 140 come from?

Response 3.10

We have revisited the divergent estimates of peak root carbon density, and adjusted the text as follows: "The ratio of fine root to leaf mass is a very rough estimate, as estimates of peak fine root mass in Northern Australian tropical savannas are divergent: 1800 g C m-2 (range 1050 – 4050 g C m-2) (Janos et al., 2008); 1300 g C m-2 (Chen et al., 2004); 70 g C m-2 (Chen et al., 2002) (assuming a specific root length of 10 m g-1 , and possibly a factor of 10 too low due to units conversion error (Janos et al., 2008))."

Comment 3.11

Pg 16334, line 21 and 22. The RËĘ2 values look about the same to me and no statistical test was performed for comparison; can you really say that some are larger and that RMSE is smaller?

Response 3.11

We agree, and have adjusted the text accordingly: "Even without being supplied external vegetation cover information, our new HAVANA model performed comparably to the benchmark (BIOS2) for monthly GPP and ET, based on the R2 values and RMSE scores (Figure 4). "

Comment 3.12

Pg 16337, line 8. Higgins is not in the reference list.

Response 3.12

Fixed

Comment 3.13

Pg 16337, line 20. You briefly mentioned grazing earlier, but it would help to add something here on the impact of both native and introduced grazers/browers. The model appears to not consider animals that could limit establishment of woody species, or contribute to the maintenance of grassland dynamics. For many parts of the world, these factors are important in savannas.

Response 3.13

We have inserted the following text at the end of the discussion: "We did not explicitly consider native or introduced grazers or browsers: however these represent a minor disturbance agent compared with fire in northern Australia (Murphy et al. 2015). The approach of HAVANA-POP remains valid for regions in which savannah vegetation

structure is controlled by the disturbance regime."

Comment 3.14

Pg 16338, line 7. Insert "of" after "implementation".

Response 3.14

Done

Comment 3.15

Pg 16342, line 15. "Acacia woodland" needs spaces.

Response 3.15

Done

Comment 3.16

Pg 16353, Figure 4. Label FAPAR as fPAR to be consistent with text? Interactive comment on Biogeosciences Discuss., 12, 16313, 2015.

Response 3.16 We suggest making this modification at the time of submitting final figures.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C8654/2015/bgd-12-C8654-2015-supplement.pdf

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