Biogeosciences Discuss., 4, S139–S144, 2007
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Biogeosciences Discussions

Interactive comment on "Atmospheric drivers of storage water use in Scots pine" *by* H. Verbeeck et al.

Anonymous Referee #1

Received and published: 19 March 2007

General Points:

In this paper the authors make use of May to October 2000 xylem sapflow data from a humid, temperate Scots pine forest to calibrate a resistance-capacitance model of water flow and storage. Unfortunately the dataset includes only sapflow data and atmospheric conditions. Hence there is no direct validation of the modeled storage water use. Interpretation with respect to the effects of atmospheric drivers on storage water use therefore depends mainly on the reader's confidence in the model predictions. For example estimates of the available storage size, or the seasonal variability (from dendrometer band readings) could have been used to generate such confidence. Futile to ask six years after the measurements were performed?

Overall, this paper is well laid-out and the presentation is mostly clear and concise. The title should reflect the fact that “storage water use” is merely modeled, not measured (for example “Model analysis of the effect of atmospheric drivers on storage water use in Scots pine”?). The abstract covers the findings of the paper, and in general the length of the sections is sufficient. Graphical representation and use of scientific literature is adequate. The figure legends could be improved with more information, and Table 1&2 should be reorganized (see comments below).

In the discussion section I was missing an evaluation of the simplifying assumptions of the model here (“electrical analogue approach”?) as compared for example to other simple (e.g. Fisher et al. 2006 PCE 29, 151-65) or much more complicated models of tree hydraulic resistances and capacitances (for example, Früh & Kurth 1999 Journal of Theoretical Biology 201, 251-270; Buckley et al. 2003 PCE 26, 1767-85; Bohrer et al. 2005 Water Resources Research ISI:000233164200002). Together with many minor glitches (unit mismatches in equations, the repeated use of one variable name for variables of different meaning, the confusion between leaf and canopy level around eq. 3), the lack of validation data for storage water use, and several more points worth of discussion (half-hourly meteorological data recording for studying eventual shorter time lags, definition of maximum available stem water storage, 15-day recalibration of the model, etc. see below), my overall assessment is accept with major revision.

Specific Remarks

1. Page 2, line 15-16 In how far can you exclude that they did not rely more on stored water during periods of high VPD, because it had already been depleted during the first day of relatively high VPD? According to Fig. 10 minimum tree water content and maximum VPD correlate.

2. Page 3, line 21-23 In this respect, one could refer to publications that helped elucidate the interplay of hydraulic versus (atmospheric) drought effects (Bond-Lamberty & coworkers)

BGD
4, S139–S144, 2007

3. Page 6, line 2-3 State the measuring resolution for the border between sapwood and heartwood (mm, cm)? Can't you say something on the uncertainty of your sapflow estimates (sapflow scales with sapwood area)?

4. Page 6, line 17 In this respect, you might want to cite Ford CR et al. Tree Physiol 2004.

5. Page 6, line 24 Is a time resolution of 15 min averages enough to separate the differences between onset of transpiration in the leaves and onset of sapflow in the trunk or to infer trunk storage water changes?

6. Page 7, line 11 Is a half-hourly time resolution for the meteorological driving variables enough to perform the analysis you set out?

7. Page 8, line 10 There is a unit mismatch in the denominator. If VPD is squared , Do should have kPaˆ2 unit, not just kPa as stated. The Tree Phys submission (Verbeeck et al. 2007) which contains the model description is more correct in this respect.

8. Page 8, line 22 If "E" is transpiration rate in mol H2O m-2 s-1, I would not call "a" an "ABA sequestration rate", which should have units of μmol ABA m-2 s-1. Yet from the equation E and a must have the same unit (neither is stated in the manuscript). According to the Tree Phys submission (Verbeeck et al. 2007) both have the unit "mol H2O m-2 s-1", which needs some explanation, when "a" is really "ABA sequestration rate".

9. Page 9, line 1 Replace "en" by "and".

10. Page 9, line 4-5 What is meant by "via a system of equations"?

11. Page 9, line 6-11 " actual transpiration at leaf level" to "where E is the canopy transpiration" This needs clarification. On which "level" have you applied the Penman-Monteith equation? On leaf or on canopy level? From what you wrote in line 18-19 ("sunlit and shaded leaf area of each canopy layer is used to scale up from transpiration at leaf level to the entire canopy"), the statements before should have been "leaf level".

BGD

4, S139–S144, 2007

However, your definitions of E, gb, gw indicate that the equation 3 was applied for the entire canopy, and would explain why you did not have to explain how you derived the VPD or Rn at the (how many?) canopy layers of line 19. But if so, then all your statements on scaling from leaf to canopy level are wrong. Rewrite line 6 to 19!

12. Page 9, line 11 & Page 10, line 8 Now you give the third definition of a variable named "E" (counting the ones used in equation 2, 3 and 4). When I take the definition and unit for E in Eq. 2 from the Tree Phys submission (Verbeeck et al. 2007), they all have different units, and eventually different meanings. To reduce overall confusion in a revision, use different symbols for variables with different meaning.

13. Page 10, line 10-11 Discuss the effects of the assumption of a constant xylem flow resistance from the root to the leaves on your results.

14. Page 11, eq. 8, 9 What did you try to imply by using "delta W" in eq.8 and "dW" in eq. 9?

15. Page 11, line 11-12 Discuss the effects of the assumption of a constant tree capacitance on your results. See e.g. Scholz et al. 2007, Biophysical properties and functional significance of stem water storage tissues in Neotropical savanna trees. Plant Cell and Environment

16. Page 11, line 19-23 I thought the period you assessed was a period without severe drought. How come you can define the maximum available tree water content the way you do here? Would it eventually not be much larger when the minimum water content could drop further down in a period of extended soil drought? Is it reasonable to assume this smaller maximum water content anyway? Discuss the effects of setting this baseline on your results. Steppe et al. 2004 is missing in the reference list.

17. Page 12, line 23-25 and Page 13, line 1-6 Good point for a scientific discussion: compare calibrated parameters with literature values for a1, C, and Rx. You state that you did not find seasonalities in your parameter estimates, recalibrated every two

BGD
4, S139–S144, 2007

weeks? Would it not make more sense to use constant parameters instead? On a minor note, what kind of calibration algorithm has been used?

18. Page 14, line 22-23 “In contrast” to what? “different” with respect to what?

19. Page 18, line 8 Again, is the time resolution of your sapflow and meteorological data enough to allow such time-lag analyses?

20. Page 18, line 9-11 “storage water use did not increase with atmospheric drought” From the results in Table 1 and 2 I take that the trees (with exception of no. 26) used on average a forth of the their “available” water per day during periods with only atmospheric drought (that is not during soil drought). I would add a discussion on their ability to refill the storage during less severe conditions (e.g. no cavitations, reversibility of cavitations?), see for example Bucci et al. Tree Physiology 2004, 1119-27. Is it because enhanced storage water use is just not possible, or because it is not yet necessary under those conditions?

21. Page 18, line 20 Replace “er” by “et”

22. Page 25, 26 Table 1&2 I would move the columns STOmax and CONmax to Table 2 and delete the column of DBH from Table 2 (already in Table 1). How come that tree no. 26 is able to use more stored water (STOmax=4.6 kg/day) than its maximum available tree water content allows (AWmax = 2 kg)? Is it because of the problematic definition of Wmax (see 16.)?

23. Page 29, line 4 “four typical days were selected” for which reason?

24. Page 30, line 2-3 add appropriately “(left bars)“ and “(right bars)” after the dates

25. Page 33&34, panels 7 from top Why is “stomatal conductance” given in m s-1 here, but the equation 1 (for stomatal conductance) gives stomatal conductance in “mol CO2 m-2 s-1” ? How is stomatal conductance “for pine #23” defined? Is it scaled with leaf area of this pine tree, or does it refer to an average squaremeter (sunlit or shaded) leaf area? Be more explicit.

BGD

4, S139–S144, 2007

26. Page 36, line 4 “trough” must say “through”

Interactive comment on Biogeosciences Discuss., 4, 615, 2007.