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Biogeosciences
Discussions

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

H. Verbeeck et al.

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We would like to thank the three reviewers for their very detailed and useful comments. Our answers to their comments are given below:

Answers to comments of Reviewer # 1

We agree with the first general comment of reviewer 1 that a direct validation of the modelled storage water is desirable. Unfortunately, storage water use can only be measured indirectly and no such independent measurements (e.g. stem diameter variations with Linear Variable Displacement Transducers) of storage water use are available for our study. Moreover, tree diameter is not modelled in the current model and can thus not be used as a validation of the model. Nevertheless, the model tests in a companion paper (Verbeeck et al. 2007), that is now accepted for publication in

Tree Physiology, make us confident enough to use the model to analyse the drivers of storage water use. In the companion paper, half hourly sap flow measurements and simulations are compared for a period of several months and estimates of daily storage water use based on measured and modelled sap flow are compared.

We agree that the title suggested in the second general comment reflects better the content of the paper. In the revised manuscript we will change the title in: “Model analysis of the effect of atmospheric drivers on storage water use in Scots pine”.

In this paper our model is applied to get more insight in storage water use. An overall evaluation of our model, the model assumptions and a comparison with other existing modelling approaches is to our opinion beyond the scope of this paper. Nevertheless, we agree to add a short discussion dealing with the shortcomings of the current approach. We will use the suggested publications as a guideline to write this discussion.

We will consider all specific remarks of reviewer 1 in the revised manuscript. Nevertheless, we would like to give a response on some of the remarks:

1. We observed that our modelling approach does not suggest a higher storage water use under conditions of atmospheric drought. The daily minimum tree water content was indeed lower in periods of high VPD. But the daily contribution of storage water to transpiration was not higher, according to our results. The reserves were not depleted after the first day of high VPD, due to refilling during the night.

3. We will add some comments on the uncertainty on the sap flow measurements. Some uncertainty in estimating sap flow in deep xylem could be caused by non-parallel needle installation.

5 and 6. The accuracy of our conclusions concerning the time-lags between onset of transpiration in the leaves and onset of sap flow is indeed restricted by the time step of the meteodata: 30 minutes. This means that, based on the measurements, we are not able to determine time lags smaller than 30 minutes. In our study transpiration

rate was always higher than sap flow during day and sap flow was always higher than transpiration during night. This means that the system never has stationary conditions. The moment when a pine tree switches between storage use and storage refilling can only be estimated with an accuracy of a half hour.

8. “a” is defined by Dewar (2002) as the “effective ABA sequestration rate”. We can agree with the reviewer that the units of this parameter (mol H2O /m^2s) do not correspond to an ABA sequestration rate sensu stricto. We will add the word “effective” to the definition of this parameter according to the original definition of Dewar (2002). “a” can be considered as a sequestration rate (mol ABA /m^2s) devided by an ABA concentration (mol ABA /mol H2O), which results in the units “mol H2O/m^2s” for a.

11. The Penman-Monteith equation is applied on the sunlit and shaded fraction of each canopy layer. We will rewrite this paragraph to make this clear.

13. Without soil water stress (no cavitation), we can assume a constant xylem flow resistance. In case of soil water stress this assumption is no longer correct, because the xylem flow resistance will increase due to cavitation. We will add some comments dealing with this assumption in the revised manuscript.

14. “delta W” is the integration of dW over the time step. Maybe it is less confusing if we write all equations in integrated form.

15. In parallel with comment 13 we will add some discussion on the assumption of a constant C (with reference to Scholz et al. 2007).

16. In the revised manuscript we will emphasize that the calculated minimum water content is not the absolute minimum, but the minimum of the considered period. We use the minimum of the considered period as a baseline to quantify other parameters (e.g. the maximum available tree water content during the considered period).

17. We did use constant parameters, because we could not find seasonality in the calibration for every two weeks. We will state this more clearly in the revised manuscript.

BGD

4, S482–S488, 2007

Interactive Comment

EGU

We used the simplex algorithm for calibration.

19. See specific remarks 5 and 6 of Reviewer 1.

20. We will add this point to the discussion in the revised manuscript. We presume that enhanced storage water use was probably not yet necessary under these conditions, but our analysis can not distinguish if it was “not necessary” or “not possible”.

23. The value for STOmax (Table 1) for tree number 26 was wrong. The correct value is 0.46 kg/day.

25. We will change the units for stomatal conductance in the figure. The figure shows the stomatal conductance for an average square meter of (sunlit or shaded) leaf area at the top of the canopy.

Answers to comments of Reviewer #2

We agree with the general comment of reviewer 2 that we can improve our paper by adding some discussion on the limitations of our model in the revised manuscript. This is in agreement with several comments of Reviewer 1.

1. We will adapt the formulation of the mentioned sentences (and others) according to this remark.

2. We agree that storage water use is mainly driven by transpiration and that other meteorological variables have an indirect effect on storage water use. We will mention this in the revised manuscript. We also agree that refilling and night transpiration are crucial processes. We observed sap flow during the whole night. This sap flow contributes as well to refilling as to night transpiration (Verbeeck et al. 2007), nevertheless without measurements of leaf transpiration during the night we can not quantify this exactly. It might indeed be interesting to investigate how storage use and refilling affects the transpiration of the next day. But we don't see how we could analyse this with the available data and models.

BGD

4, S482–S488, 2007

Interactive Comment

3. We don't agree that stem water potential would not be able to vary under a constant flow resistance. We think it is rather obvious that stem water potential can change with changing soil water potential and changing leaf transpiration. As already mentioned, we will discuss the assumption of the simple electrical analogon in the revised version of the manuscript.

We will consider all "other" remarks of reviewer 2 in the revised manuscript. Nevertheless, we would like to give a response on some of the remarks:

1. There were 14 sample trees all together. All of them were measured by multi-point sensors (6 measuring point each along the needle) during short-term period (several days) in order to study radial and circumferential variability. For that, 4 sensors per tree were used situated at 4 cardinal points (24 measuring point per tree). Moreover, each multi-point sensor was moved along the xylem radius and measuring points were doubled in this way (Nadezhdina et al. 2002, 2007). Then, during long-term period sample trees were measured by single-point sensors continuously and sap flow was scaled up to the tree level with knowledge of radial and circumferential variability according to the procedure described by Nadezhdina et al. (2002).

2. A table with parameter values and their seasonal variability will be added. Equations 4, 5 and 6 are intermediate equations and are not validated as such.

3. We know it is quite obvious that our values fall within such a large range found in literature. However, we think it is important to inform the reader about this large range that can be found in literature.

4. To our opinion, the atmospheric conditions do not force the stomata to close. The atmospheric demand for water is low (low VPD). Stomata do not risk loosing too much water. Incoming radiation is indeed relatively low, but photosynthesis can still be quite considerable due to diffuse radiation.

5. As far as we know we did not include a residual analysis in the manuscript. We will

BGD

4, S482–S488, 2007

Interactive Comment

do such an analysis and include any relevant results in the revised manuscript.

Answers to comments of Reviewer #3

We do not agree with the first general comment of reviewer 3. To our opinion the use of storage water is also substantial and important under circumstances without water stress, to avoid cavitation when canopy transpiration is higher than stem sap flow.

We agree that it would be interesting to write a paper on soil moisture effects. Unfortunately our dataset does not contain a period with real soil water stress.

Specific comments:

We did not include a residual analysis in the manuscript. We will do such an analysis and include any relevant results in the revised manuscript.

Our other paper dealing with the validation of our model is now accepted for publication in Tree Physiology (Verbeeck et al., 2007)

Concerning validation with stem water content measurements: see answer on first general comment of reviewer 1.

It is not clear for us what should be improved in Figure 9.

References

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BGD

4, S482–S488, 2007

Interactive Comment

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

BGD

4, S482–S488, 2007