HESS manuscript "hess-2017-47, Tree-, stand- and site-specific controls on landscape-scale patterns of transpiration" by Hassler et al.

Response to the comments of Referee #2

Thank you very much for your detailed comments. We address the individual points (put in italics) in the following.

The main results are that hydrometeorological conditions (evaporative demand and soil water supply) explain little variation in landscape-level sap flux patterns, compared to that explained by site-, stand- and tree-level factors.

It seems that our main point came not across clearly. Our main result is not that the hydrometeorological conditions don't explain much of the spatial patterns, but that apart from the obvious tree-specific predictors, the sites-specific predictors actually explain a considerable part. Which is of interest for hydrological modellers trying to improve spatially explicit transpiration estimates. We will adapt our conclusions to make that clearer.

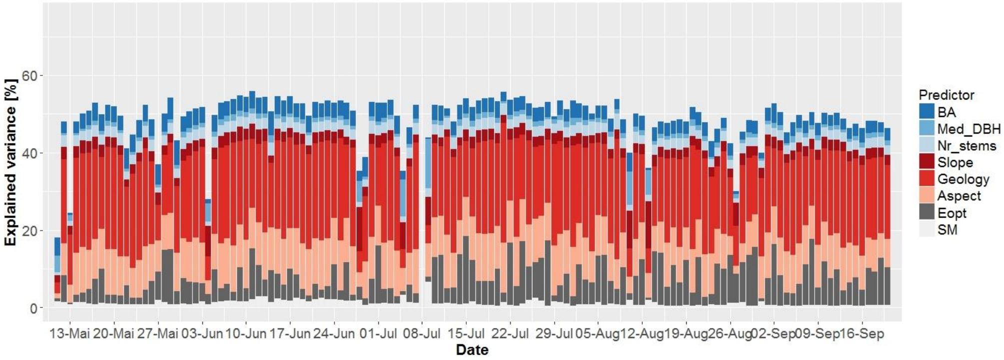
First, the authors present sap velocity (probably better named as sap flux or sap flow density, per unit sapwood area) not tree transpiration. While sapwood area-based sap flow density may be an interesting quantity in itself for more physiologically-oriented studies, where water transport characteristics are compared across species or ecological settings, it may have less interest from the hydrological point of view. A more natural approach would be to scale sap flux to whole-tree sap flow, using tree sapwood area and a reasonable integration of spatial variation of sap flux within the sapwood.

Thank you for the comment. This point was also made by referee #1, so this is a copy of the response to that comment.

We chose sap velocity as a response variable which is an equivalent to sap flux density (we stuck to the velocity term for reasons of consistency with the manufacturers equations but are happy to change it if it leads to misunderstandings). This was due to the reason that sap velocity was the actual measurement variable without further assumptions about allometric relationships of diameter and sapwood area, bark thickness etc., and because the sensor installation was not always ideal in this year with outermost thermistors in some cases possibly in the bark, so a maximum velocity is a more robust measurement than the upscaled water volume fluxes.

Additionally, we see that we could use published allometric relationships between diameter and sapwood area and a number of assumptions on bark thickness and radial variability to come up with estimations of sap flow volumes instead of velocities. However, these relationships would only tackle the tree-specific controls of the relationship between sap velocity and sap flow. In our dataset we also have the influence of the site- and stand-specific predictors, and to our knowledge there are no detailed studies incorporating these influences into published equations. Therefore we base our main analyses on sap velocity patterns as a proxy to identify possible influences on transpiration.

Nevertheless, we agree that for being directly helpful to hydrologists - whom we primarily consider as the interested audience for our results - we should at least attempt the upscaling to sap flow, even if we can only do so with equations for tree-specific controls and the associated unknown uncertainties. We ran the linear models again, leaving out Species, DBH and Height as they would be interrelated with the equations, resulting in the following figure which we will include in the manuscript. The figure still stresses the importance of Geology and Aspect of the site-specific predictors. Additionally, when the species and DBH effect is removed, potential evaporation becomes more important compared to the results for sap velocity.



Additional Figure (probably Figure 9): Explained variance of daily linear models of spatial sap flow patterns.

We will discuss these results in the light of the respective methodological limitations and mention that detailed single-predictor pre-studies might help to find better equations for upscaling, however, interactions would still not be included and could only be tackled with methods that directly measure transpired water volumes (e.g. eddy covariance estimates). However, we see that our main goal behind the study and our reasoning for using sap velocity did not come across clearly. We will change the introduction and methods sections appropriately to include more detailed descriptions and explanations and refer to them better when we discuss the results.

A related point is that, even if sensors measuring sap flux in three points along the tree's xylem depth were installed, so potentially accounting for some of the radial variation in sap flux, the authors chose only the point with the highest sap flux values (pg. 4, L. 33). In my opinion, they should integrate sap flux over the probe length and make some assumption about the variation of sap flux beyond the probe length and up to the sapwood-heartwood boundary.

Our main reason for selecting the maximum sap velocity of the three velocities we can obtain from the sensors' profiles was to have a robust estimate of sap velocity. As stated above, sensor installation was not always ideal in this year, so the maximum sap velocity seems the most reliable measure of something like a transpiration potential. Therefore we base our analyses mainly on this measure – ignoring that depending on the sapwood depth we will have different sap flow rates. We will revise the discussion to clarify the differences. For example, the upscaling to tree level will increase the difference between beech and oaks even more than looking at sap velocities.

Nevertheless we agree that a tentative upscaling and looking at the models of sap flow will be helpful, when the uncertainties associated with the upscaling are kept in mind. We stated how we will do and incorporate this in the response to the comment above. We will of course adapt the discussion accordingly to also address the differences between the results about sap velocity and the upscaled sap flow.

As for the modelling approach, I think that the contribution to explained variation by the different the predictors, will depend on the order in which these predictors are introduced in the model, something that is not stated in the methods. In other words, do results of the variable importance analysis change if hydrometeorological variables are introduced first, and then the rest of the factors?

You are right, if we were to simplify the models so that we had only the best for each day, the order of the predictors would be important and also the contributions would change slightly. In our case the order of the predictors is not important because the variable importance assessment calculates a mean of all possible orderings. We will revise this part (last paragraph on page 6 in the original manuscript) follows and hope it will be clearer then (changes in yellow):

“Although a step-wise simplification of the models using the Akaike information criterion led to a higher percentage of explained variance by the models, we refrained from using this simplification in order to keep the model structures similar for each day. This allows comparability of the temporal, day-to-day changes in predictor importance. For prediction purposes using the potentially best model would be more appropriate, however, in our exploratory analysis we focused on comparability. The relative importance of the predictors in explaining the observed sap velocity variance was assessed using the approach of Grömping (2007), made available in the R package relaimpo. Of the different methods to determine relative importance we used lmg, named after the original authors Lindeman, Merenda, and Gold. This method uses sequential sums of squares from the linear model, applies all possible orderings of regressors, and obtains an overall assessment by averaging over all orders which is deemed appropriate for causal interpretation and unknown importance of the different predictors (Grömping, 2007). The initial order of the predictors in the linear models is not important anymore for consideration of relative importance as orderings are shuffled anyways. “

Grömping, U., 2007. Estimators of relative importance in linear regression based on variance decomposition. American Statistician, 61(2): 139-147.

Also related to the models, the authors focus on the variance explained by the different predictors but they do not go into much depth in the direction of change in sap flux with the variation in the predictors (which is necessarily complex given the multiple variables involved).

We agree we do not go into much depth concerning the single-factor analyses. But as we know that we have a multivariate problem, we do not want to over-interpret these relations, but rather give a first general overview of the data.

The presentation of the results could also be improved. For instance, Fig. 4 could focus only on the most important variables (reduce the number of panels) and use conditioning symbols, shapes or colours to show multivariate relationships; one example, sap flux density vs dbh coded by species, geology or basal area categories.

This comment relates to the one before. We do not want to over-interpret univariate graphs, but give a data overview. Therefore we also think that having all panels in the figure is more informative than pre-selecting and further interpreting relations based on three-variate plots. After all that’s why we chose to use the multiple linear regression as an analysis tool.

Overall, the study does not seem to convey a clear message or a novel result. Some of the findings on the structural controls of sap flow across the landscape are not really that new (Adelman et al. 2008, Loranty et al., 2008, Angstmann et al. 2013, Tromp-van Meerveld & McDonnell, 2006, the last two studies cited in the manuscript).

We agree that the message could be clearer and we will improve on the phrasing in the revised manuscript, however, we do think we show a novel result. After all, there are only very few studies which actually compare the multiple influences on transpiration that exist in a landscape and try to quantify their importance to better inform spatially explicit transpiration estimates. Previous studies mostly consider only one additional factor to the well-studied tree-specific ones. Thank you for suggesting the two additional studies, we will include them in the introduction. However, they also do not provide a more general attempt at identifying the most important influences on transpiration patterns in our landscape. Adelman et al. (2008) suspect an effect of differences in water availability on a slope due to contrasts in species composition, but did not see the effect of slope position on transpiration, possibly because it was too late in the season and therefore to dry over the whole slope. And Loranty et al. (2008) find that species spatial patterns mainly control spatial patterns of transpiration, but did not see dependence of sap flux density on a moisture gradient along a slope. However, they also state that soil moisture was possibly not limiting for transpiration in their study because it was overall wet enough and for example the studied aspen is quite drought-tolerant.

Altogether we do see the dire need for more studies on the topic of influences on transpiration at the landscape scale, in different landscape settings, with different species, at best also with experiments targeting univariate effects, and last but not least independent transpiration estimates (eg. from eddy covariance towers) to test the hypotheses. Especially if hydrologists want to go beyond using the Penman-Monteith equations in spatially explicit models, estimates on influences that could be obtained from maps would help to improve models. We will revise the discussion and conclusions to accommodate these thoughts and novelty of our study.

Adelman, J.D., Ewers, B.E. and MacKay, D.S., 2008. Use of temporal patterns in vapor pressure deficit to explain spatial autocorrelation dynamics in tree transpiration. Tree physiology, 28, 647.

Loranty, M.M., Mackay, D.S., Ewers, B.E., Adelman, J.D. and Kruger, E.L., 2008. Environmental drivers of spatial variation in whole-tree transpiration in an aspen-dominated upland-to-wetland forest gradient. Water Resour. Res., 44.

Specific comments

P. 5., L. 6. What about the role of vapour pressure deficit in driving transpiration? Epot here seems to include a radiative term only. p. 7, L. 16 - 22. Please see my comment above on the possibility of showing bivariate plots with conditioning variables to show interactions between predictors.

We understood the comment above as a suggestion to show univariate response plots for sap velocity conditioned with a second predictor variable. This comment then refers to generally showing interrelations between predictors. We did not to that in this study but describe it when explaining the preparations for the modelling in the methods sections. We believe this sufficient and showing x-y-Plots of all predictor pairs would not contribute a lot to the focus of the study. However, if desired we can put the x-y-Plots in an appendix.

Concerning the role of vapour pressure (VPD) deficit: In an earlier version of the study we looked at temperature, radiation and VPD separately. However, we were reminded that a combined measure of atmospheric evaporative demand would be more suitable and we agree with that. The proposed measure by Renner et al (2016) is somewhat comparable to a Penman-Monteith approach, albeit based on thermodynamic principles. The simplicity of the equation and the necessity of only shortwave radiation makes it easier to use with the available measurements in the study area. In their paper Renner et al. (2016) tested for additional effects of VPD and wind speed on transpiration and the results did not show a distinct effect. These tests and the shown comparability, although slight underestimation, to Penman-Monteith suggests it is a good way of assessing evaporative demand for our purpose.

Renner, M., Hassler, S.K., Blume, T., Weiler, M., Hildebrandt, A., Guderle, M., Schymanski, S.J. and Kleidon, A., 2016. Dominant controls of transpiration along a hillslope transect inferred from ecohydrological measurements and thermodynamic limits. Hydrology and Earth System Sciences, 20: 2063-2083.

P. 9, L. 2 - 18. I don't fully agree with the explanation that soil moisture limitations are not detected because soil water availability is not exhaustively measured (over the entire soil profile or taking into account water in fractures). Transpiration shows a threshold response with declining soil moisture, *and even when deeper soil layers may be playing a role in supplying water you could still detect a (highly non-linear) relationship with most soi layers (e.g. Duursma et al 2008). Even if water was taken from deep layers, transpiration would still be related to soil water status in the upper layers (Warren et al., 2004).*

Thanks for pointing this out. We don't claim to have the true explanation for the lack of detected soil moisture limitations. It is possible that high moisture contents on rainy days with low transpiration are causing this effect. We will check on this by redoing the correlation analysis for soil moisture while excluding rainy days.

P. 10, L. 31-34. There are indeed some studies on this; see the Adelman et al 2008 study cited above on the spatial patterns of physiological regulation of transpiration. P. 10, L. 39-40. Could this lack of sensitivity for oak be caused by the inherent limitations of the heat ratio method in measuring high flows (see e.g. Vandegehuchte & Steppe, 2013).

Indeed there are some studies about spatial patterns of transpiration (albeit not many), but rather from a plant physiological point of view than from a hydrological one. As we are discussing the temporal dynamics of predictor importance in this paragraph we are referring to the dynamics of the multivariate predictors' effects which to our knowledge hasn't been studied at all so far. Maybe it makes things clearer if we put "multivariate controls in a landscape" instead of only "controls" in line 34.
We don't really see how a lack of sensitivity for oaks can be a sensor limitation as the sap velocities of the beech trees are generally higher than the oaks anyways. Did we misunderstand your comment here?

P. 11, L. 5-15. The authors should try to upscale sap flow density to sap flow using the three measuring points along the sapwood and using sapwood areas (measured or derived from allometry). Although they would need to make some assumptions on the circumferential variability, but nevertheless, I think it's worth doing the scaling.

We do have our reservations about using published allometric relation that are based on only the tree-specific influences, however, as stated above, we will include the linear models based on sap flow in the revised version of the manuscript for comparison and discuss the differences.

P. 11, L. 11-12. Other studies show sap flow well beyond the outermost ring in deciduous oaks (e.g. Poyatos et al., 2007).

Yes, we agree that oaks transpire not only in the outermost ring, we tried to stress this by putting the word "annuli" instead of "annulus" in the text of the original manuscript. But furthermore, the main interest here is the comparison to beech trees and they reportedly transpire up to greater depths (eg. measured 6-8 cm and estimated 10-12 cm in Gebauer et al. ,2008).

Gebauer, T., Horna, V. and Leuschner, C., 2008. Variability in radial sap flux density patterns and sapwood area among seven co-occurring temperate broad-leaved tree species. Tree physiology, 28: 1821-1830.