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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Tree-, stand- and site-specific controls on landscape-scale patterns of transpiration" *by* Sibylle K. Hassler et al.

Anonymous Referee #1

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Review of manuscript hess-2017-47

GENERAL COMMENTS

Hassler et al. present an analysis of sap flow velocity in a set of 61 oak and beech trees in the same catchment, focusing on the spatial and temporal correlations between sap flow velocity and various possible biotic and abiotic controls. They find that a combination of controls is necessary to explain the variability in sap flow velocity, and that the controls on temporal variability are different than those on spatial variability. They also find that the explanatory power of the controls varies with potential evaporation.

I commend the authors on their work, which I think provides the ingredients for an excellent paper; however, substantial revisions to the manuscript will be needed to realize that paper. The general difficulties that would need to be overcome on revision

are:

1) The paper tries to be about transpiration (starting with the title) and yet the authors explicitly state that they do not calculate transpiration because they do not have the information necessary to reliably estimate it from sap flow velocity. They cannot have it both ways. If they cannot make even a rough estimate of transpiration (or even sap flux density), then they cannot conclude anything about it, and they cannot phrase the paper as if they can. They must either make a quantitative estimate of transpiration with uncertainty (however large) and then see what they can and cannot conclude about it, or else restrict their discussion to sap velocity instead (which would be quite limiting).

2) I think the statistical analyses do not quite get us to the reported conclusions. The authors show that a many-variable model can explain about 70% of the variability in sap velocity. But with enough variables, a model can "explain" almost any variability, without necessarily being meaningful or being able to predict variability in a new dataset. To make claims about predictive power (as the authors do), they would need to test the model's predictive power by dividing their dataset into "training data" and "testing data", as is commonly done with models. If the model successfully predicts the variability in the testing data, then a claim can be made. This approach could strengthen the paper. The focus on proportion of variability explained is also somewhat limiting and even misleading. For example, the authors highlight in the abstract that "the temporal dynamics of the explanatory power of the tree-specific characteristics, especially species, are correlated to the temporal dynamics of potential evaporation". Potential evaporation is strongly correlated with transpiration, so this finding isn't really a finding to me; it's just saying that when transpiration is small, noise dominates the variability and so the proportion of variability explained isn't a good metric to use to evaluate a model. Instead evaluating how well the fitted model can predict testing data over a range of conditions would avoid this problem.

3) The discussion is weak, tending to repeat the published literature or the present findings without addressing or even recognizing the key questions that the present

findings raise. Perhaps as a consequence, the paper does not sufficiently digest the results into informative, clear conclusions, which is to say that I was left asking: what did the authors really discover? What did they want me to take away from this paper? In my view, the main candidate for a discovery in the present draft is the finding that several factors were all important controls on sap flow velocity âĂŤ but that is a somewhat vague and superficial finding, and not really a surprise, I don't think. I am sure the authors and the readers can learn more from this work. And in turn, the implications of the conclusions are not well articulated. That is, I was not convinced of why I should care about the study's conclusions. I suspect that once more substantial conclusions are expressed, then more concrete implications will follow.

4) The writing is good but the ideas could be made easier to follow. For example, it's hard to wrap one's head around a heading like "Temporal dynamics of predictor importance for explaining the daily spatial sap velocity patterns". This heading refers to the dynamics of a statistic that is itself a summary of dynamics. Moreover, "for explaining the" adds confusion because it is largely redundant with "predictor", and the term "daily spatial sap velocity patterns" is an ambiguous way to condense "spatial patterns in daily mean sap velocity" âĂŤ that is, if I have understood the authors' intended meaning correctly. So this is a section about temporal patterns in the ability of the model to predict spatial patterns in a temporal average of sap velocity. That is quite a convoluted idea. Is it really the best way to look at the data? If so, great care must be taken to guide the reader through it.

I hope the authors will revise the manuscript in order that it might reach its full potential.

SPECIFIC COMMENTS

p1, l28: Soil only affects transpiration via plant-physiological characteristics. It also seems strange to single out soil but not the atmosphere here.

p2, l33-5: As stated, this doesn't make sense to me. If canopy transpiration was varying due to length of the growing season, then the effect would surely be seen in the sap

flux densities of individual trees, which are also affected by the growing season length. Perhaps the contrast was between total annual transpiration and instantaneous summertime transpiration, rather than between tree and canopy scales?

p3, l1-6: This paragraph seems out of place. It reads like you're moving on to a new topic, but in fact you are reiterating the idea of site-specific characteristics influencing sap flow, which you were talking about on the previous page.

p4, l39: The driving gradient for transpiration is often phrased as an aspect of atmospheric conditions, as here, but in fact, what's more important than the atmospheric water vapor pressure (i.e. the end of the vapor pressure gradient, which typically doesn't vary much over the course of the day) is the temperature of the leaves and associated saturation vapor pressure therein (i.e. the start of the vapor pressure gradient, which varies a lot from day to night and is the reason why transpiration is typically negligible at night). So it would probably be most accurate to say that the main environmental limitation to transpiration (and therefore sap flow) is the solar heating of the leaves.

p5, l34-6: This seems backwards. Slopes less than 5 degrees, called "Plain" would be a category in aspect, not in slope position; and less steep parts of slopes would be a category in slope position, not aspect.

p6, l34-5: I am not familiar with this method, and so I do not understand the idea here or the meaning of Fig. 2. Please provide at least a reference to let unfamiliar readers understand what you are doing here.

p7, l8-9: This result confirms what I said in my comment above: the most important control on sap velocity is solar heating of the leaves (the only real variable in your E_pot equation is solar input).

p7, l31: The "few days" look like 2 months to me.

p8, l21: I could not figure out what you meant by this until I looked at the figure. It was not clear what was being cumulated, which variance you were talking about, or what

the contributions were to. Statistical analyses often involve technical details and jargon that make them difficult for the average reader to follow unless extra care is taken to describe them clearly (at the expense of brevity). For example, you might refer to "the proportion of variance explained by all the tree-specific predictors taken together, all the stand-specific predictors taken together, and all the site-specific predictors taken together".

p8, l22-4: I actually have the impression that if you scaled the site- and stand-specific lines up to have the same mean as the tree-specific line, then all the lines would be seen to vary similarly.

p9, l23-31: This is really just restatement of results. You are missing the opportunity for discussion and analysis here. For example, you repeat the observation that Epot doesn't explain much spatial variation but fail to connect the dots and say that it's not surprising that Epot drives temporal variation but not spatial variation given that Epot itself varies a lot temporally but not so much spatially.

p9, l31-5: Here you are getting into discussion, but I think you are missing the real point because you are writing as if your measurements were of transpiration or sap flux density instead of sap velocity. Of course big trees will transpire more, but they also have bigger trunks with more sap "bandwidth". A question that I think you should be asking here, and to which your data might speak, is how transpiration and sap velocity scale with tree size. That relates to allometry: how does the canopy size scale with DBH and sap "bandwidth"?

p9, l36: Was this a statistically significant effect?

p9, l37-8: You are glossing over the difference between sap velocity and sap flux (or transpiration) here. I do not think you should conflate them; rather I think you should use the discussion to explicitly consider how they might relate.

p10, l12-6: Here I feel like I am reading the same few points from the literature over and

over, slightly rephrased: e.g. water availability depends on the type of ground. I knew that before reading your article. What I am looking for as a reader of your discussion is to learn something from your findings. How do these points from the literature help me learn something from your findings?

p10, l17-8: This is, at first glance at least, very surprising and contradicts the positive temporal correlation of Epot and sap velocity. You should comment on that. The sampling effect of oak vs beech is something it seems you could (and should) check statistically with your data.

p10, l33-4: The key point that I have not seen you address is how much the drivers themselves vary. That is surely the reason why Epot doesn't have much spatial explanatory power, as I noted above. I wonder whether the explanatory power of each predictor just depends on how much that predictor actually varies.

p10, l34-40: Again, this paragraph is mostly just repeating findings. In a discussion, I'm looking for the "why?", at least some informative speculation. The fact that species explains a lot of spatial variation on some days and little spatial variation on other days was surprising to me. Why is that? Is it that conditions on some days are favorable to all species while conditions on other days favor one species over another?

p11, l3-4: It sounds like you're saying that when Epot is low, there isn't much sap velocity, and the variability in sap velocity is just noise. That is probably true, and it is a reason why proportion of explained variance alone is not a great way to assess the effect of a potential predictor on sap velocity.

p11, l9-17: This kind of discussion needs to happen earlier, as each topic is discussed.

TECHNICAL CORRECTIONS

Please see the attached, annotated PDF file (edits might show best in Acrobat).

Please also note the supplement to this comment:

http://www.hydrol-earth-syst-sci-discuss.net/hess-2017-47/hess-2017-47-RC1-supplement.pdf

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