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Discussions

Interactive comment on "Adapting the thermal-based two-source energy balance model to estimate energy fluxes in a complex tree-grass ecosystem" *by* Vicente Burchard-Levine et al.

Anonymous Referee #1

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The rationale of the paper is that estimating evapotranspiration from the thermal based two source resistance energy balance modelling (TSEB) approach in tree-grass ecosystems is particularly problematic. The authors investigate whether results from the default TSEB parameter set can be improved upon by dividing the year into two seasons, one in which tree characteristics dominate and one where grass characteristics dominate; and therefore (a modified version of) the tree or grass parameter set is utilised during those appropriate times. Latent and sensible heat flux from the 2 season version of TSEB (TSEB-2s) is compared to that produced from either the 'default' parameter defined TSEB or the tree and grass endmember parameter defined TSEB. The authors make use of three eddy covariance flux towers that measure the data re-

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Discussion paper

quired to define the parameters of TSEB and also are used for independent validation. A global sensitivity analysis was performed on a subset of 14 or 11 selected parameters using the Sobal sensitivity method, which tested the models sensitivity directly to the parameters changing and also summarised their indirect interactions. A local sensitivity assessment was also performed on two of the main input variables, being LAI and LST at midday. The effects of implementing two different wind attenuation variants was also assessed.

I feel that there is a publishable paper in the work that has been done, but not as the paper stands. The scientific objective needs to be clearer and justified, and the experimental design needs to be clear and convincing. The paper has so much in it that it is hard to follow. There are interesting signs from the sensitivity analysis, but clear links to the sensitivity analysis to the adjustments made to TSEB-2s were not explicit. They were connected, but too loosely. I include some of my more major concerns below. I have also provided many comments in the attached pdf, please see and address these as well.

(1) The scientific objective paragraph (∼L85-L97) needs clarity/re-writing. The details around the objectives are not clearly justified as to why specifically they make up the current experimental design. For example, why two phenological modelling periods, why two modelling structures and why are they based on wind attenuation formulations, why is the secondary goal about LE partitioning (do you just mean it is the second goal, or do you mean it is not as important to study or that it is not studied in as much detail). The objective(s) don't seem really convincing or structured and this really needs to be fixed. Food for thought: when running/improving a complex and highly (probably over) parameterised model, it would be good to come up with a practical component to the objective. It is my view that for a model that makes use of 33 variables/parameters it is unlikely that all variables/parameters will be realistic or known hardly anywhere, let alone be realistic across vast areas/time periods. I wonder if the objective can provide some insight into something useful for scientists that will not be running TSEB?

It probably already does, but may need to be expressed in those sorts of words.

(2) I have some concern about the sensitivity analysis (SA). The two SAs were performed with 11 parameters and 14 parameters. The table caption of Table 2 indicates that they are the selected parameters used for the TSEB global SA, but there are 14 of them. So which ones were left out for the 11 parameter set? Was it the last 3? Table 3 has 11 parameters, which are the same as the first 11 in Table 2, so it would seem that my guess was right. It doesn't help my process of working it out that Table 3 is referenced before Table 2. Why make me work so hard?

Regardless, the more important point is that I don't find how these 11 and 14 parameters were selected from the possible list of 33 at the start of the paper. I suppose that it has to do with what it says around L296 that parameters related to vegetation resistance and roughness were configured. Again, I had to search and re-search for this. Plus it doesn't specifically say that is the criteria for selecting the parameters nor specifically why 3 were left off. I apologise if it does, but I have been going back and forth and I've gotten a bit lost now...

Furthermore, what is the effect of leaving parameters/variables out of a sensitivity analysis? I would like to be re-assured that the authors have considered this and there is justification for it. So, it seems important to provide information about how the subset of parameters were selected and what the influence of leaving some (most actually) out of the assessment has. The way that the equation looks to me is that it assumes all the variance due to adjusting the model parameters is captured, then partitioned. Well, if you aren't assessing all of the parameters, then you are not capturing all the variance. It might be OK, but I feel like it needs specific addressing.

Finally on this point, the SA would likely have assumptions regarding independence and or normality. There is no indication whether this was considered or if it matters.

(3) probably most importantly, I have concerns regarding a number of issues dealing with the comparison of the new TSEB-2s results compared to the so-called default

TSEB results or the end-member tree or grass TSEB results. So figure 5 shows the LE and H results from the default TSEB and they don't look so great. The default parameters used for the results shown in figure 5 are given in Table 3. I note that the default values assume that fraction green cover (fg) = 0.7 all year, which is the same value used for the grass end-member model for that same parameter. Also in the default parameter set, the canopy height (hc) = 8 m, which is the height of the canopy used for the tree end-member model. Also noteworthy is that neither of the grass or tree end-member models results look good either as seen in figure 8; the grass endmember model underestimates H, but overestimates LE while the tree endmember model overestimates H and underestimates H. Figure 7 and 8 form the basis for the benchmark to which TSEB-2s is compared. Not a particularly hard benchmark to beat.

Well Table 4 shows us that for much of the year, the TSEB-2s makes use of measured fg, so this choice kind of confounds the comparison right from the start. Does the TSEB-2s model do better because it splits the year into two separate seasons or simply (primarily) because it uses a varying and measured parameter instead of a static one? It begs the question of whether the TSEB-2s needs the two season split or simply a better estimate of fg. The other thing is that Table 4 shows us that for the non-summer, a canopy height of 0.5 m is used. So using a hc=0.5 for most of the year rather than hc=8 m probably makes a difference. Inspecting Figure 1 and reading the study site section again verifies that the site is only about 20% tree. So, it seems like a questionable choice to use an 8 m canopy height as a default parameter for your benchmark analysis to represent a site having 20% trees. Pictures of the trees in Figure 1 make it seem even if the site was fully forested, a canopy height of 8 m might be too high! So, this begs the question of whether simply reducing the hc parameter to something more realistic in the default set of parameters might improve the benchmark results seen in Figure 5. So, it kind of feels like a bad set of parameters might have been used to derive the benchmark results. Then field data and a few better choices were made in the two-season model, providing much improvement. I'm not sure it is a reasonable or fair comparison. It might be just about as good if you chose a reasonable canopy height parameter (and any remaining improvement might be due to using dynamic measured fg). I could be wrong, but it certainly is not convincing. Comparison with endmember model results that assume the whole site is grass or tree adds some context, but does not do anything to convince me that the comparison is sound.

Finally, the 'independent' validation doesn't seem too independent. Maybe I am wrong, but it seems like the independent validation still uses ground measured fg. So, again, improved results at other sites compared to default TSEB is not surprising if that is indeed the case. Furthermore, if ground measured fg is used in the independent validation, it really invalidates any conclusions that are being drawn about how transferable the method is across time, I think (spatial and temporal evaluation of section 3.3.1). And as far as testing its spatial transferability, the other sites are all within a few kilometres of each other. That is not really overly convincing either. I think this experimental design needs re-thinking. The point about splitting TSEB into two seasons is simply not convincing at the moment.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-354/hess-2019-354-RC1-supplement.pdf

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