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Hydrology and Earth System Sciences
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.

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The authors would like to thank the anonymous reviewer for the comments and suggestions, these will make for an improved manuscript.

We have provided below our response combined with the comments provided by Reviewer 1 to make it easier to follow. Additionally, since numerous changes were made to the manuscript, a revised version is provided in the supplement files. Please find below the responses to the general issues raised along with the more specific comments made by the referee.

Reviewer 1 comments:

"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 utilized 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 required 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 summarized 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."

Authors' Response:

Overall response: The introduction section has been modified to more directly state the scientific objectives and how these will be achieved (see L38 to L98). More direct links of the influence of the sensitive analysis to TSEB-2S were added (see L289-290; L441-443). As well, the entire manuscript was revised with more direct and clear language. Additionally, to make the manuscript easier to follow (and more concise) the analysis comparing the two different wind profiles was eliminated, since little difference was found when using the two different wind profile schemes and it may distract from the overall objectives of the paper. Please find below the responses to the more specific comments made by the reviewer.

Reviewer 1 comments:

"(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."

Authors' Response:

Response to point 1: The introductory section was modified following comments about clarity and to better articulate the scientific objectives and the reasoning of the methods that were applied (see L38 to L98). These included a greater emphasis on the difficulties in applying TSEB in ecosystem with complex/multiple vegetation structures (L76-79), justifying our hypothesis to apply the model considering two distinct phonological/modeling periods (L85-88) and the objectives were stated with more clarity (L90-98).

We clarify that the 'list of variables' at the beginning of the manuscript consists of a combination of inputs, outputs and intermediate variables within the model, where many cannot be changed/adapted. As such, the 33 variables within this list are not all parameters (there are only 11 parameters in TSEB) and this table was added to help the reader easily find the symbols and acronyms pertaining to important processes and variables related to TSEB. To limit confusion around this, the table title was changed to be 'list of acronyms'. In addition, a table was added with all inputs and parameters which more clearly describe the input/parameter and its role in the modeling procedure (see table 1).

Reviewer 1 comments:

"(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."

Authors' Response:

Response to point 2: Indeed, the parameter selection was not directly specified. The two global SAs were conducted with two different modeling structures: one using the Goudriaan (1977) wind model and the other with the Massman et al. (2017) wind model. In addition to the original 11 parameters, the Massman et al. (2017) scheme requires an additional 3 parameters (i.e. Cd, Xisoil and hmax) and hence why the SA using Massman2017 is done with a total of 14 parameters. As mentioned, since the use of different wind profiles resulted in very little change in results, this analysis was eliminated and only the TSEB with the Goudriaan (1977) wind scheme is now used for the analysis. As stated, the 33 variables listed in the 'table of variables′ are not all parameters (i.e. mixture of inputs, parameters, outputs etc). The selection of parameters for the sensitivity analysis was based on analyzing all parameters within TSEB which are used in the sub-modules of radiation transfer between canopy and soil (i.e. fc, wc, Xlad), roughness and resistance schemes (i.e. hc, z0soil, lw, c, C', b), the initial canopy transpiration estimate (fg and alpha_pt) and, if applied, the Massman et al. (2017) wind attenuation scheme (i.e. Cd, Xisoil and hmax). A line was added to explicitly state this (L342-344) and information on which sub-module the parameter influences was added in table 3. In addition, the assumption that the SA method has about independence of parameter factors was directly stated (L325-327)

Reviewer 1 comments:

"(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 nonsummer, 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."

Authors' Response:

Response to point 3: The default model was parameterized in an attempt to represent a mix between tree and grass vegetation, as observed in this ecosystem. Since trees were found to have a high contribution to H (i.e. as discussed in El-Madany et al., 2018) due to their lower aerodynamic resistance as compared to the grass layer, we decided to parameterize the resistance with tree characteristics (where the most influential parameter in this regard is hc), while parameterizing vegetation cover characteristics (e.g.. fg and fc) as grass since this layer represents roughly 80% of the surface cover. These assumptions further highlight the difficulty in parameterizing and applying the TSEB default model in this type of ecosystem. This is because multiple, and very different, vegetation types are present, making it hard to adequately specify a unique parameter set for the single vegetation layer assumed in the modeling structure, which must try to characterized the heterogeneity present (i.e. mix between tree and grass). This is the central problem this manuscript is trying to address. Indeed, as the reviewer points out, an hc=8m is probably too high to characterize the tree-grass mixtures so it was lowered to the weighted average of both tree and grass layers (i.e. 0.8*0.5m+0.2*8m = 2m) (see new table 2) but this will ultimately add more resistance and similar results (underestimation of H) will be achieved (Fig. 7).

Similarly, the end-member simulations were incorporated to add context and provide'boundary conditions' when we parameterize the model completely ignoring one of the vegetation layers. In addition, the end-member scenarios (an assumed vegetation cover for a given land classification) are typically used in global modeling applications, therefore these simulations also highlight the problem when TGEs are wrongly classified in global land cover maps, which is common for these type of ecosystems

(e.g. Jung et al, 2006). A line was added to highlight this (L374-376). Thus, the three 'scenarios' consists of describing the model as 1) clumped tree cover (TSEB_tree), 2) homogeneous grass cover (TSEB_grass) and 3) mixed tree and grass cover (TSEB-DF). However, as shown, none of these scenarios can adequately model turbulent fluxes. This way the configuration of TSEB-2S was designed in an attempt to solve this problem in a relatively simple manner by assuming a dominant vegetation cover exists depending on the seasonal period due the dynamic phenology observed in the ecosystem, allowing for a different parameter set depending on the seasonal period. The TSEB-2S configuration is better described in section 2.4.4 with additional figures added (Fig. 3 and 4) showing the different LAI time series used.

Indeed, we agree that adding a variable fg based on field data distracts in evaluating if the improved results were related to changes in this parameter or to the new modeling structure of TSEB-2S. Therefore, TSEB-2S was revised to implement a constant fg (i.e. using the same fg configuration as the default model). See section 2.4.4 and table 4. In addition, see L442-453 for more details on the changes to parameter values in TSEB-2S. As can be seen, in figure 9, model performance decreases very slightly as compared to using a variable fg as in the previous version of the manuscript, but the error statistics are largely within the same range and simulations still improve substantially when using TSEB-2S compared to default TSEB-DF (see Fig. 7 vs Fig. 9).

Reviewer 1 comments:

"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."

Authors' Response:

Response to point 4: As stated in point 3, we are no longer using a variable in-situ based fg input in TSEB-2S, the fg is maintained constant just like TSEB-DF. Regarding validation, indeed the towers are located relatively close to each other but they have gone through a nutrient manipulation experiment, which was shown to cause differences in surface biophysical properties and energy partitioning between the three tower footprints (El-Madany et al., 2018), therefore showing some level of spatial variability. As well, the model was applied for different years with substantial inter-annual variability (i.e. wetter and more productive 2016, dry and less productive 2017) so it was interesting to test if the new model structure was still able to produce reliable results considering the different seasonal and phenological conditions of the different years. In addition, while measurements between towers are correlated, we argue that they are independent model runs because the model simulations use independent inputs (i.e. LAI, LST) and were evaluated against independent EC measurements of the tower in question. Additional statements regarding this were added in L116-124 and L424-428.

Response to specific comments in supplement pdf provided by Reviewer 1

L1: Please consider specifying that it is daytime energy fluxes. And specifying which energy fluxes are assessed

- Response: title was changed to 'Adapting the thermal-based two-source energy balance model to estimate daytime turbulent energy fluxes in a complex tree-grass ecosystem'

L19-22-31-40-53 etc: Loaded statistical term. I suggest simply deleting these words, as they are not needed. Please try to avoid subjective terms. Accurate? With some values would be better wording here.

- Response: all 'significant' terms without statistical backing were edited. As well, subjective terms were changed and avoided throughout the text.

L67: Why is there so much focus on how much things increase or develop? It seems like it is not really the real issue to me. Also, 'over the past years' is rather meaningless and actually detracts from the impact of the whole sentence. Is this sentence even needed?

- Response: yes, this focus was avoided and the sentence was taken out. Similarly, edits were applied throughout the introduction.

L74: needs some references

- Response: Reference was added (L61).

L78: Citing two papers does not support your statement of 'widely' argued. I suggest avoiding grand, sweeping, subjective statements. Perhaps just say: Local SA techniques are unsuitable for complex non-linear models since there are often strong and significant parameter interactions.

It is so much more direct and then having two citations seems to be enough.

Perhaps go through the entire manuscript and eliminate as much of this type of subjective wording in favour of very direct, clear wording.

- Response: The sentence was edited and, as suggested, the whole manuscript was edited to be more clear and direct.

L95-97: This paragraph about the objectives needs re-writing. Please make it clearer why these things were specifically studied . It is close, but not really good enough. It shouldn't take a lot of work, but it is really important to describe why specifically these things are studied.

- Response: As mentioned in the general response to point 1, the objective statements in the introductory paragraph were re-written to better state the goals and rationale of the manuscript in a clear and direct manner. These include more directly stating the problematic of the study, justifying the methods and better articulating the objectives of the study. As well, the study was reviewed and simplified to eliminate the analysis of the different wind profile sub-models within TSEB, as the added value of this analysis was minimal and distracted from the overall goal of the manuscript. See L38-98.

L164: AC is lower case in the figure but upper case in the main text of the manuscript

- Response: AC was changed to lower case in the main text (L171) and eq.4 to 6

L245-246: I assume a good deal of the inputs required were derived by inversion of certain equations. It might be important to specify which inputs were directly measured and which ones were inverted based on which measured values

- Response: The meteorological inputs necessary to run TSEB are described in a revised version of table 1 along with the instrumentation used to measure them. These meteorological forcing were directly measured by the tower systems (no inversion). LST was obtained from eq. 13. Note that LE and H, described in table 1, are not inputs but serve to evaluate model performance. Table 1 was modified to clarify the description and purpose of the parameter/variable.

L297: Table 3 is referenced before Table 2.

- Response: The position of table 3 is changed to place it before table 2 of the original

manuscript.

L311: Please go through methods/results/conclusions and change all present tense to past tense, it really makes the manuscript easier to read and understand. Present tense in the methods makes it unclear what was done exactly in the paper compared to what is possible to do or what others have done. Present tense in the results/discussion/conclusion makes things sound more universal rather than specifically pertaining to this study...

- Response: The manuscript was revised as suggested and changed to the past tense where appropriate.

L326: I was unsure what the $\sim$ represents, it would be good if it was explicitly explained

- Response: This was a typo. The $\sim$ was added to the variable and explicitly explained (L334)

L335-336: It is really important to provide details on how the parameters used in this analysis were chosen and how others were excluded. I don't find in this section or Table 2, which 3 parameters are not used for TSEB_G-DF. Maybe it is listed somewhere. I do notice there are 11 parameters in Table 3, so that must be it. Is it stated directly? I don't think it is up to this point. Also, Table 3 is referenced in the text before Table 2, which doesn't help the reader follow what is happening.

- Response: As stated in the response to point 2, there are 11 parameters within TSEB. The differences in the number of parameters were due to there being two different wind profile schemes tested within TSEB in the previous version of the manuscript (one of the wind schemes needed an additional 3 parameters). In the revised version of the manuscript, only one wind profile scheme (Goudriaan, 1977) was used. Table 1 was revised to clarify the parameters and inputs used. Details on the parameters used in the SA were added and stated more directly (see L342-345) and table 3 was edited to include the sub-module that each parameter influences.

L347: how were these values decided upon and what makes them realistic or meaningful in this local sensitivity analysis?

- Response: Values are related to typical uncertainties expected from remote sensing products of LST (i.e. Sobrino et al. ,2006) and LAI (the double of the uncertainty of the LAI empirical model shown in appendix A). This is added and directly stated in the text (see L351-354).

L429: what if you did the assessment in degrees C? then the change in LST would be considered to be $\sim$10% rather than 1%. Also, what if you compared Ts-Ta rather than just Ts? This may change what % a 3 degree variation in LST means yet again. I'm not super convinced by this logic.

- Response: Initially, the percent change was added to make the uncertainty between LAI and LST more directly comparable. However, this may be irrelevant so only the unit change assessments were kept (see section 3.1.2), which is typically how uncertainty is evaluated for remote sensing products. The analysis is done in K for the LST because we wanted to quantify the uncertainty related to the input itself, which is normally a product retrieved in K.

L469-470: Please make it clear that you mean the improvement is seen between Figure 6 and Figure 7. Also the way it is written it makes it seem like the wind attenuation scheme is responsible for the improvement, I realise now that TSEB-2S indicates this, but readers are not as intimate and in-tune with your nomenclature, so don't make them work so hard. Say it directly. include that the difference is between fig 6 and 7 and that it is due to not having the default parameters or whatever.

- Response: The line was changed to explain more directly (see L478-479)

L517: I'm not really seeing this. It captures some of the big events, but also adds in a lot of variability that isn't there a lot of the time. It even seems to very poorly represent the drying down effect in the first figure part. I think you'd be better off not trying to

'sell' just the good things. If you want to describe temporal variability, then report the RMSD and relative error of the time series by day or month rather than just overall global summaries. You might be better off not doing this analysis, actually. The third figure plot does OK with the gross patterns and even the trend, maybe...

- Response: The line mostly referred to being able to capture the specific rain events during the summer period, even though consistent and important errors are observed. The line was changed to better articulate this (L505-507). Additionally, rainfall data were added to Fig. 11.

L545-548: It would be great if you provided details and some more context/info

- Response: Details about the error bounds of other energy balance studies were added to better compare/contextualize results presented in this manuscript (see L537-552)

L573-574: In this study or in Nieto?

- Response: This was referring to Nieto's study. The sentence was taken away since this was discussing an issue related the different wind profile models used. This analysis was taken out of the manuscript for clarity purposes.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-354/hess-2019-354-AC1-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-354, 2019.