Authors Response to Reviewer 1 comments

This paper from Chebbi et al. deals with the modeling of fluxes exchange in a semi-arid olive orchard. The literature on the topic is quite rich, but the research topic is still relevant due to the lack of a "good-for-all" solution available in the literature at the moment. Specifically, the paper seems to aim at exploring the use of two different modeling framework, namely the single- and the two- path approaches.

1. My main concern with the paper is the lack of clarity in the logical reasoning behind the evaluation protocol adopted here. Firstly, the author evaluates the models in term of transpiration, but it is not clear which scheme they tested. Single? Two? Does it matter at this point? The authors never clarify; hence it is not possible for a reader to understand if their conclusions are reasonable. Then they come up with an empirical calibration (again, not clear related to which scheme), without accounting for possible other sources of error, and only after all of that they discuss a comparison of the two schemes, where it is not clear how the previous calibration and correction play a role. Maybe this approach is logical if the reasoning behind is clarified in the text, but at the present the flow of the paper results really odd and difficult to follow and comment. I suggest to strongly revisit the text to make clearer to the readers why the comparison procedure was structure in this way (maybe with the support of a flow chart). At the present state, it is difficult to me to give a fair evaluation of the results without the needed context.

   Response:

   We agree with the reviewer that both aspects (lack of proper representation of transpiration and 1P versus 2P representation) are treated one after the other but are mentioned all through it. We would separate both issues more clearly. When dealing with the model evaluation in terms of transpiration, all results correspond to the 1P scheme but are also valid for the 2P one, we would stress this fact. This 1P default configuration with default setting and using parameters derived from ground measurement was thus used in the first part as the reference to tackle model outputs inconsistency. Then, the model was slightly revised to address these issues for both versions (the increase of the fraction cover and the water supply assumptions) before intercomparing them. Once the evapotranspiration partition is correctly reproduced over the entire system, the comparison between the two versions of the surface scheme (1P vs 2P) was evaluated.
   In the revised version of the manuscript, the paragraph below would be added: "in this study, the model was run using parameters determined from observations or from the literature. First, the different ISBA outputs from 1P configuration, applied as a benchmark, were compared with observed data, including an analysis of an inconsistency when dealing with the observed vegetation fraction cover of 7 % as the weighting of the evaporation E and transpiration T components. These findings hold also true for the 2P configuration. Then, the model was slightly revised to address those inconsistencies, principally the evapotranspiration partition. In particular, we looked at how the effective area that transpires can be increased to match the observed T. an additional water supply was added to match the observed transpiration after significant rainfall amounts. Finally, the second issue deals with the choice between the patch (or uncoupled) approach and the layer (or coupled) approach and which is the configuration that better reproduce the water and energy exchanges, with respect to the vegetation sparseness and structure of this discontinuous canopy. These two configurations could finally be assessed and compared when the limitations (i.e., effective fraction cover and the water supply) arising from the first issue were cleared".

2. The introduction is too long in my opinion, and it needed to be streamlined. For instance, between lines 140 and 160 too many concept are cramped, with the result to be confusing and also to mix-up different concepts that are not meant to be used in the same modeling framework (e.g., clumping factor and fcbased partitioning).

Response:

In a revised manuscript, this paragraph would be condensed accordingly " There are two main concerns in our case: 1/ the very low fraction cover in the study site equal to 7 %, which can be regarded as bare soil, results in low fraction of net radiation available to the vegetation if the partitioning is based on this horizontal projection fraction. It seems also that "big leaf" potential evapotranspiration derived from most SVAT models, which use the vegetation fraction cover as weighting factor for the turbulent fluxes partition, do not allow to achieve a sufficient order of magnitude compared to the observed one. For example, to simulate a transpiration value equivalent to the maximum of 3 mm per day recorded during the wet period over the same study site (Chebbi et al., 2018), a potential amount of 3/(fc=0.07)=42 mm day-1 would be required. The observed transpiration was checked in Chebbi et al. (2018) through comparison to the difference between the observed evapotranspiration and evaporation. Moreover, the order of magnitude of our observed transpiration rate falls in the range documented in the literature (Moreno et al., 1996; Tognetti et al., 2006). Indeed, Santos et al. (2018)reported mean transpiration of 1.5 mm per day with maximum values observed in the summer under deficit irrigation treatment over 10 years old olive trees with a spacing of (4.2×8 m) in southern Alentejo, Portugal. Similarly, Moriondo et al. (2019) validated their model (dedicated to the simulation of growth and development of olive trees) against a set of data collected over a rainfed olive grove in Italy with ground cover of 0.19. In their research, it was also found that the simulated as well as the observed transpirations reach 3 mm per day in July. Therefore, there is a clear deficiency in the modeled potential transpiration rate to represent the contribution of transpiration to the whole area in the case of fraction cover partitioning. The area average transpiration is clearly stemming from a larger contributing surface than what can be classically computed from a turbid medium with clump LAI of woody trees (roughly 3) weighted by the fraction cover, and must be calculated by aggregating a larger leaf-atmosphere interacting layer."

3. L198-206. Such details are not needed here, since a reader not familiar with the model cannot understand the content of this paragraph at this point of the text.

Response:

The following paragraph would be removed in a revised manuscript. "The model was applied using parameters determined from observations or from the literature. First, the different ISBA outputs were compared with observed data, including an analysis of an inconsistency when dealing with the observed vegetation fraction cover of 7 % as the weighting of the evaporation E and transpiration T components. Then, the model was slightly revised to address some inconsistencies, principally the evapotranspiration partition. In particular, we look at how the effective area that transpires can be increased to match the observed T. Finally, the second issue deals with the choice between the patch (or uncoupled) approach and the layer (or coupled) approach and which is the configuration that better reproduce the water and energy exchanges, with respect to the vegetation sparseness and structure of this discontinuous canopy."

4. Methods The authors should focus on the key features relevant for this study and that distinguish the 1P from the 2P. This brief, rather generic, description of the model is not useful for a reader not familiar with the model. For instance, in the 2P approach the concept of clumping factor is not relevant (since the vegetation fully cover is patch, and likely assumed to be spherically random), so the reference in the introduction to clumping factor is confusing when discussing the partitioning. The concept is relevant for the 1P, but it is not clear if accounted or not.

Response:

The clumping factor is used here only as the mean to relate LAI for the vegetation patch in the 2P configuration (CLAI, which corresponds to the field estimated LAI of the tree crown) and the 1P one (area average LAI). This would be clarified in the revision. The description of the model would be condensed as suggested in the revision and details about model description would be moved to the Annex1.

The paragraph L297-302 would be modified accordingly:

"While all the other parameters remain equal, the LAI (Leaf Area Index) is the parameter that varies between both simulations. Indeed, for the 1P configuration, the field scale LAI (=0.24 m²/m²) includes the area of soil which is not covered by vegetation and is expressed as:

$$LAI = veg \times CLAI + (1-veg) \times 0 \quad (1)$$

Where the CLAI, used for the 2P configuration, is the clump LAI (i.e., the ratio between the leaf area and the area of the soil below the tree) and is thus equal to LAI/veg (=3 m²/m²)."

5. The value of LAI reported in Table 1 (3.2) refers to the projection of the tree crown (e.g., m2 of leafs over m2 of soil covered by the projection of the tree) and it is only used in P2 (I assume), whereas the "field scale" LAI is a much different value discussed successively in Eq. (1). This is a rather key point, that is not well explained in the text. In Eq. (1) you call LAI the LAI used in 1P, as a function of the LAI used in 2P (which was previously called LAI as well), and then defining CLAI as LAI/veg. A reader may then read LAI = 3 and CLAI= 46, which is not the case, I guess. Additionally, in this discussion is never mentioned if a clumping factor is used and how it is defined. Since it is often mentioned in the introduction, it would be important to clearly state.

Response:

See response 4.

The LAI values would be clarified in Table 1 as follow:

CLAI	3 m²/m² of soil for 1P	Observations

The clumping factor included in TSEB model was mentioned in the introduction but not used in our study.

5. The minimum stomatal resistance is another key parameter, much discussed in the literature on olive trees. Many people can argue that errors in this parameter are much more likely than in the interpretation of LAI. Again, this need a lot of justification to be completed ignored here and in the discussion.

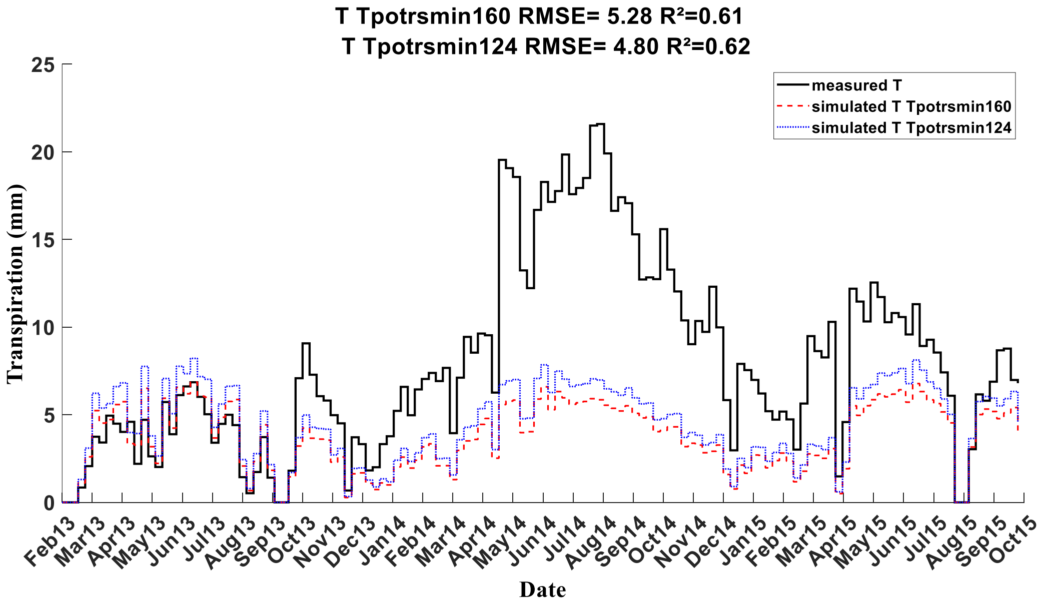
Response:

The justification of the minimum stomatal resistance value would be further justified from the following literature review.

rs (s/m)		age	site	article
	124.4	18 year old	cordoba spain	(Moriana et al., 2002)
range from	423.0	13 year old	seville spain	(Rodriguez-Dominguez et al., 2019)
to	141.0	13 year old	seville spain	(Rodriguez-Dominguez et al., 2019)
	169.2	8 year old	seville spain	(Hernandez-Santana et al., 2016)
	166.7	26 year old	seville spain	(Fernández et al., 1997)
	162.7	40-year-old	spain	(Torres-Ruiz et al., 2011)
	211.5	7 year old	southern italy	(Giorio et al., 1999)
	235.0	30 ans	south east of Tunis	(Dbara et al., 2016)
	282.0	10 ans	central italy	(Marino et al., 2014)

The default value of Rsmin proposed by the model falls within the range of the mean rsmin value found in the literature. The lower limit of rsmin is about 124 m/s as found in the study of Moriana et al. (2002).

The figure below shows the potential transpiration deriving from the reference 1P configuration for two values of rsmin (the minimum value reported in the literature to our knowledge for olive trees 124 m s^{-1} and the value used for this study 160 m s^{-1}.



6. L248. Please use here the term 2P and stick to 1P vs. 2P for the rest of the paper (as you stated later on, L285 but failed to apply in some circumstances). The continuous interchanged usage of path/sources make difficult for the readers to follow the rest of the text (especially because the 1P is a two-source and the 2P is parallel single sources).

Response:

Would be edited as suggested. The terminology (1P/2P) would be thus used all along the manuscript to enhance clarity and readability.

7. Results L320-325. Is this discussion really necessary? At the best, I would frame this part as a benchmark bottom minimum in term of model performance.

<u>Response:</u>

The following paragraph would be removed in the revised manuscript "Taking into consideration the negligible LAI value, we first evaluate the model in a bare soil condition as a lower limit in terms of model performance and investigated whether this assumption can provide a fairly realistic simulation of the energy fluxes. The model was not able to track the seasonal dynamics particularly for the latent heat flux which shows peaks after rainfall event followed by sharp decrease when the soil is dry (not shown). This temporal pattern of LE was inconsistent with field observations. In addition, the RMSE between the observed total fluxes over the orchard and the simulated fluxes from the sole bare soil patch were significant about 31.46, 73.24, 58.23 and 44.12 W m-^{2} for Rn, G, LE and H, respectively."

8. L327. From here on it start the confusion, since no clarification on which version of the model is discussed in these figures. It is not possible to me to have a full analysis of these results if no context is given.

<u>Response:</u>

In the revised version of the manuscript, the following paragraph would be added accordingly: "The simulation, here, is a 1P configuration used as reference to evaluate the model performance and the energy fluxes deriving from this simulation are shown in the figure 2." we would also clarify in figures captions which simulation was used.

9. L11. I would suggest to replace sustainability with resistance.

<u>Response:</u>

As suggested, sustainability would be replaced with resistance.

10. L13. Even if generally low, cover fraction reaches values definitely higher than that, without discussing intensive olive orchards.

<u>Response:</u>

The sentence would be corrected as follow: "(i.e., rainfed olive trees that have a vegetation fraction cover ranging from 2 to 15 %)"

11. L15. I would suggest to replace decipher with separate/extract.

<u>Response:</u>

Decipher would be replaced with separate in the revision.

12. L72-73. Please correct the reference format.

<u>Responses:</u>

The reference format would be corrected.

13. L183. The reference here to climate change is, in my opinion, out of place.

Response:

“and above all the evolution of this functioning under predicted climate changes” this part of the sentence would be removed.

14. L193. This reasoning adopted to justify the use of ISBA (not needed in my opinion), is week, since almost all the models can “test future scenarios based on future climate forcing”. Again, I would stick to the actual goal of the presented research, without involving climate change, which is not the focus of this study.

Response:

This paragraph would be modified as follow: “The choice of this complete physical model for the present study can be justified by its ability to test the two configurations (coupled and uncoupled) and the different soil water transfer schemes (i.e., force-restore and multilayer diffusion) within the same modelling environment.”

15. Fig. 1. I would suggest to invert the two panels, since 1b is referred to before 1a.

Response:

The two panels would be inverted.

16. Table 1. Please clarify that you are talking about SOIL layers here.

Response:

Would be edited as suggested.

References

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