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Discussions

Interactive comment on "Modelling of water and energy exchanges over a sparse olive orchard in semi-arid areas" *by* Wafa Chebbi et al.

Anonymous Referee #1

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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. My main concern with the paper is the lack of clarity in the logical reasoning behind the evaluation protocol adopted here. Firstly the author evaluate 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 scheme, 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 more clear 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.

Major comments Introduction 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 fc-based partitioning). 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. 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. 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. The minimum stomatal resistance is another key parameter, much discussed in the literature on olive trees. Many people can argue that errors in this parameters 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. 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). 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. 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. Minor comments L11. I would suggest to replace sustainability with resistance. L13. Even if generally low, cover fraction reaches values definitely higher than that, without discussing intensive olive orchards. L15. I would suggest to replace decipher with separate/extract. L72-73. Please correct the reference format. L183. The reference here to climate change is, in my opinion, out of place. 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. Fig. 1. I would suggest to invert the two panels, since 1b is referred to before 1a. Table 1. Please clarify that you are talking about SOIL layers here.

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