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Hydrology and Earth System Sciences Discussions

Interactive comment on "The impact of in-canopy wind profile formulations on heat flux estimation using the remote sensing-based two-source model for an open orchard canopy in southern Italy" *by* C. Cammalleri et al.

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Response to Anonymous Referee #1

First of all, we would like to thank Referee #1 for the detailed and helpful comments on the submitted manuscript.

1) Title a little bit too long.

We have shortened the title in the new version of the manuscript, as: "The impact of

in-canopy wind profile formulations on heat flux estimation in an open orchard using the remote sensing-based two-source model".

2) Introduction P4692 lines 5-10: 'flux observations. . . are used to evaluate three different in canopy wind profile algorithms. . . ' In fact, the flux observations were used to validated the surface fluxes simulated by 3 different TSEB versions, varying only by the in canopy wind profile representation. There are no measurements allowing to really test what in canopy wind profile model is better for the study case.

We have rewritten the sentence in order to emphasize that the validation was carried out to indirectly assess which wind profile formulation allows better estimation of the heat fluxes.

3) P4693 line 3: eq 3 , why there is no ground heat flux for vegetation (Gc)?

G0 is the ground heat flux, which represents the heat flux conducted to/from the soil surface layer. The heat storage in the canopy layer is considered negligible due to the relatively low height of the vegetation. A clarification was added to the manuscript.

4) P4693: Justification Eq 3,4 it would be better to give a review of all parameters used in the equations with their value used in this study (maybe in annex).

We have added a table that describes the main parameters we used.

5) P4694 line 3: 'modelled by the approach proposed by Brutsaert (1982)' have you test other approach? This formula can be arguable for some situations.

We generally found a reasonable agreement between the values modelled using Brutsaert (1982) and the values derived from the measurements collected by a 4-component net radiometer (measuring incoming and outgoing longwave and shortwave radiation) installed in the study area. This is also confirmed by the good agreement between the modelled and measured net radiation in the same study site, as reported in Cammalleri et al. (Journal of Hydrology, n° 392 pp. 70-82, 2010).

6) P4694 Line 4: emissivity varies according to soil moisture. . . have you kept these values constant for all surfaces?

The emissivity is not constant for each pixels (because it is a function of canopy coverage), but the dependence of e_s upon soil moisture is not taken into account. This choice, widely adopted in remote sensing applications, is justified by the difficulty to take into account this variability and by the small effect on the daytime Rn assessment (see Cammalleri et al., Journal of Hydrology, n° 392 pp. 70-82, 2010).

7) Line 10: eq 7: coefficients defined for specific conditions or low crops? Application to sparse crops or to orchards problematic?

The coefficients are defined for bare soil, and do not take into account the canopy coverage because the effects of vegetation are included in the Rn,s computation.

8) P4695 eq 10: means that you must know the mean row spacing. Is it possible everywhere for a regional application? What is the variability of this parameter?

Information on row spacing is required, and standard values can be estimated by means of land use map, as added in the text. Furthermore, the variability of this parameter at higher spatial resolution is small.

9) P4696 eq 14: coef c =0.0025, defined for maize. the cultivated crops can comprise a lot of very different crops. Have you test the sensitivity of rs?

We agree that the influence of this parameter could affect the TSEB outputs. This parameter varies from 0.0011 for a smooth soil surface to 0.0038 for a rough surface. A value midway between these two extremes of 0.0025 was selected as a reasonable choice based on previous applications in the literature. Furthermore, since the main goal of this work is the study of in-canopy wind profile formulation effects on model output, we used this fixed value.

10) P4696: You describe rs but not ra and rx, why? (give some information in annex).

We have added the ra and rx description in the new version manuscript.

11) P4697 eq 15-16: what parameters are measured or fixed constant (add a table with 3 columns (Goudriaan, Massman, Lalic) sensitivity of these parameters (calibration performed more than validation).

Values of the wind parameters for this study have been included by means of a summary table (Table 1). The values adopted come from those recommended by the corresponding references cited in the paper.

12) P4698 eq 18: coef Cd can vary.

We agree, but we choose to adopt the recommended values suggested by Lalic et al. (2003) and Goudriaan (1977).

13) P4701: give more information about the ground measurements LAI, Ts, canopy height: how many point measurement per field?

We have added a Table that summarizes the in situ measurements (# of sample, calibration, etc.).

14) P4702: line 10: 3 soil heat plates used, is it enough to take into account the spatial variability at field scale? Accuracy?

There were 3 heat flux plates at the SAS installation and 2 at the EC site within the olive orchard field. Differences in G between the SAS and EC installations is relatively low (see Table 3).

15) P4703 line 5: 'EC flux closure was enforced by assigning energy residuals to latent heat flux' It can be a problem if your G estimations are wrong (soil heat flux measurements are often problematic particularly for sparse crops and if you have heterogeneous soils).

The energy balance closure was in general satisfactory (Closure Ratio = 0.87). The closure error is similar to what has been reported in the literature. We examined the variability in G (which are relatively low compared to other fluxes) and no particular bias has been found.

16) Table 1: what comments on the high values for RE % for G? add in caption (values computed for all day long?) it would be more interesting to see mean values for the 7 studied dates.

The analysis has been performed using the daytime data. We added the mean G values for the 7 studied dates on Table 3. RE % for G are high because to the small magnitude of G; in fact in this case the RE % statistical index is not a good descriptor, however the absolute indices (MAD and RMSD) indicate that the accuracy of G measurements is satisfactory due to the small contribute to the energy budget. We added a comment in the text.

17) P4703 line 26: what is the spectral range for the FLIR thermal camera?

The FLIR thermal camera ranges from 7.5 to 13 um. This information is reported in the new version of the text.

18) P4704: Why do you not use radiosoundings to other models to correct the images from atmospheric effects?

The calibrations with using contextual in situ measures are more accurate in case of high resolution images, returning R2 values always greater than 0.98 (not reported in the manuscript for the sake of brevity).

19) P4704: Why have you chosen the Clevers method to estimate LAI? There are other robuster methods which can be applied to your data.

We are satisfied using the Clevers method since we used the in situ measurements to calibrate the Clevers formula. This a good compromise between the simplistic VI-LAI relationship and the complex in-canopy radiative transfer models (Minacapilli et al., HESS 13, pp. 1061-1074, 2009).

20) Line 25: 'the canopy heights have been retrieved by means of local calibrated LAI based polynomial empirical relationship as suggested by Anderson' Could you give more information on this method (initially suitable for soybean or corn), please. This point is important. What is the variability obtained? Give values or a table for example.

This approach has been used because the homogeneity in crop type (only arboreus without herbaceous). In these conditions a simple LAI-hc 2nd order polynomial provided satisfactory results validated using independent measurements (R2 = 0.94). We have added this info in a new Table.

21) P4705 line 11: how are aggregated thermal data?

We averaged the original radiance values and then we retrieved the temperatures using the Planck law. We have added clarification on this step in the new text.

22) P4707 Line 13: On the Table 3, it seems that some dates present standard deviations higher than 2, that is not negligible! Have you test the impact of this variability on your main outputs?

The focus of the analysis is not the negligibility of temperature standard deviation but the representativity of a small olive subplot on the whole olive field. This representativity is demonstrated by the similarity of the single plot and whole field standard deviation. Of course the magnitude of the standard deviation is related to the spatial resolution, but this is not relevant in this study.

23) P4707 line 24 figure 7: only displays the mean values. These values were computed for areas comprising numerous pixels. It would be interesting to add the variability observed for the 3 simulations on the figure 7 (idem for fig 10). P4707: Add standard errors in table 4 and fig 8.

We added the standard deviation on Figures 7 and 10. The choice of standard deviation instead of standard error is justified by the very small magnitude of the SE (few W m-2), which made harder the analysis of the variability.

24) P4708 line 18: you have not mentioned before that ïĄą* can change for the same field during the year. How have you defined this variation for your study case? Give the values in a table.

The temporal variability of alpha_* (which is related to the roughness sub-layer of the underlying vegetative surfaces) should change during the year only for crops that have a significant growth. In the case of olive trees this can be considered negligible. To avoid possible misunderstanding, the sentence was simplified.

25) P4709 lines 13-23: The comments must be more nuanced because there are no measurements on these crops (vineyards and citrus fields). A table with the main parameters chosen for these crops can be given here. P4711 line 2: 'the analysis suggests. . . ' the main results were obtained for olive orchards so the conclusion must be reviewed.

We agree on the fact that the validation (in terms of heat fluxes) can be realized only in the olive field. Despite that, also the model vs. model comparison in the other areas introduce interesting information. We have restructured the discussion and conclusion parts to remove misunderstanding on the validation of the models in the fields where no measurements are available.

26) P4711 Line 16: the role of wind direction. Could you develop more this point (and add arrows on fig 2 to show this point).

The role of wind direction will be analysed in future studies. The wind direction is not reported in Fig. 2 because the olive orchard does not have a row structure. Only vineyards have this structure.

27) Discussion on the application to other sites, data availability, accuracy?

We have added some considerations about these aspects at the end of the conclusions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 4687, 2010.