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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimation of Mediterranean crops evapotranspiration by means of remote-sensing based models" *by* M. Minacapilli et al.

Anonymous Referee #2

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First of all, I would like to congratulate the authors with the extensive work that they have performed. Setting up a spatially distributed hydrological model and applying two different surface energy balance model is a time consuming effort that requires the integration of different work fields. I believe this work is new and scientifically challenging and it is relevant in the scope of the HESS journal.

After the introduction the authors describe extensively the differences between the one-source and two-source energy balance models, SEBAL and TSEB. This is followed by a short description of the SWAP work to estimate ET spatially and reference is made to earlier work in D'Urso (2001) and Minacapilli (2008). Finally the quality of the SWAP

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Interactive Discussion

Discussion Paper

model is discussed and the two energy balance models are compared with each other and with the SWAP results. The article is well written and well organized, but should be improved according to a few points that are presented below in the section "manuscript organization". Smaller comments regarding the language and other edits are given below in the "specific comments". The title reflects the contents of the paper, but I would suggest to change the title to "Estimation of actual evapotranspiration of Mediterranean perennial crops by means of remote sensing based surface energy balance models". The abstract covers the research methodology. I would suggest to include one paragraph some major outcomes or conclusions. The presentation of the images, especially the maps, is excellent.

I have **three major concerns** regarding the scientific methods and assumptions that are used, and the conclusions that are drawn from the work.

First of all, what is the goal of the research? I understand that two energy balance have been applied using airborne remote sensing, that the SWAP model was applied in the same area using some spatially distributed data. Is the goal a quantified spatial comparison of the outputs of both models? This has not been explained in the introduction and the goals of the research/manuscript must be described in the introduction.

Secondly, my major concern when reading this paper is the fact that the spatial SWAP results are used as reference to compare the outcomes of SEBAL and TSEB. To define the upper boundary of the model, an approach has been chosen that relates the LAI derived from the satellite image to a certain Kc value. However, I cannot follow how this is performed. Sufficient attention has been given on how the LAI is calculated, but then reference is made to papers by D'Urso (2001) and Minacapilli (2008). I have checked the 2008 paper, but also here no details are provided how the LAI is linked to Kc. The 2008 paper describes a much larger area and only data for grapes is presented here. Moreover, on P14, L19-24 I find exact copies of the text from the 2008 paper in this manuscript. I believe the choice of Kc play an important role to determine the upper limits of the ET and the final SWAP ET map in Fig. 5. This should be explained

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extensively in the update of the manuscript.

Next, SWAP is validated using three points where soil moisture was measured; in an olive field, citrus field and a vineyard. (why are the results of only two sites two sites shown in Fig. 4?). Since the SWAP results are used as reference I would like to know more about its accuracy, but here reference is made to a PhD thesis that is not available to me (Bianda, 2007). Moreover, I am not convinced that, if measured soil moisture is in correspondence with SWAP estimations, the estimated/calculated ET values are correct on those locations, let alone spatially distributed over the entire area.

E.g. if we consider the citrus field in Fig. 5 (SWAP, 15x15 meter) and the same field in Fig 9A (SEBAL, 15x15) and 9B (SEBS, 15x15 meter), it appears that the distribution of SWAP ET is rather homogenous, whereas the energy balance models show similar patterns of a higher ET in the southern part of the field. From Fig. 1 it appears (just visually) that soil types (or their top layers) appear to vary a lot inside the field. Also topology could explain the ET patterns found with the energy balance models. Other explanations are also possible, such as irrigation water distribution across a field. Again, I am not convinced that the SWAP model catches the spatial patterns of ET well since the parameterization may not be catched, but I cannot check this since no details are provided on e.g. the soil sampling and interpolation between the points. For future research you may consider calibrating the SWAP model using remote sensing outputs of the ET (see e.g. Immerzeel et al., 2008 that you also refer to).

Thirdly, in section 4.2 the TSEB and SEBAL models are compared and very strong conclusions are drawn. I believe the conclusions are not supported by the findings in this research and they also cannot be supported by the data that were generated in this research. Moreover, the results are based on the comparison of the observations and calculations during one single day (May 16, 2005). Starting on Page 5, L25 the authors mention that the SWAP calculations should be taken as reference since no flux instrumentation was installed, since there was a high level of agricultural fragmentation. Firstly, in the study are only three types of vegetation present, I would not call this a high level of fragmentation. Secondly, while mentioning this, is it acceptable to use SWAP, calibrated on soil moisture measured on three locations and upscaled using an LAI image, as a reference?

On P17, L3-4, the authors conclude from Fig. 7 that SEBAL underestimates ET compared to TSEB. What is shown here is a comparison between two models, and it can easily be argued the other way around that TSEB overestimates daily ET. No reference or information is provided that supports their conclusion. Similarly, the authors argue that SEBAL overestimates of the sensible heat flux (H). This can easily be argued the other way around since no independent measurements of the energy balance were made that support this finding. The authors refer to three conference papers where this effect of overestimation of H by SEBAL was witnessed before. I have no access to these papers and moreover two of the three papers probably concern the same research since the same authors are involved (Minacapilli, D'Urso). The authors are clearly in favor of the two-source models, but from this manuscript I do not find any evidence that could support this preference.

Some observations regarding the maps of ET in Fig. 5 (SWAP), Fig. 9a (SEBAL), and Fig. 9b (TSEB):

1. ET in olive field O1, appears rather homogenous for SWAP and TSEB, SEBAL, however, shows significant lower ET in the Western part, roughly between citrus fields, C1 and C2. Is there an explanation for this?

2. Both TSEB and SEBAL depict lower ET values in the two vineyards, compared to SWAT. Can this be due to the way the upper limit of SWAP is determined (Eq. 21)?

3. Also in both citrus fields, the ET calculated by SWAP is high compared to the results of the energy balance models.

4. What explains the difference in ET between both citrus fields. Is there an age difference, planting distance?

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5. The ET in citrus field C2 is largely between 0 and 0.25 mm in the SEBAL calculations. This seems to be too low unless there is severe water stress on this day.

Regarding Fig. 11:

1. The standard deviations of the energy balance models are rather similar, the SWAP deviations are smaller. What does this say about the three models? Does SWAP catch the spatial variations that are present or do the energy models overestimate? What is the influence of scale?

Manuscript organization

1. Introduction: there is too many details/equations on the TSEB and SEBAL models in the introduction. The explanation of the differences between the two models on page 4, L8 to page 5, L9 should be included in section 2.

1. Introduction: define the goals of the research and shortly how the article will continue in reaching these goals. That should guide me through reading the rest of the paper.

2. Models description: the part on P10, L1 to L13 describes similarities of the two models. You could leave this part out and shortly summarize in one paragraph where the major difference between SEBAL and TSEB are.

3. Case study and data collection. I would rename this to "Study area and data collection"

3. Case study and data collection. On P13, L2 reference is made to Figs. 2a and 2b. These are results and I would suggests you discuss these figures in section 4.

4. Results and discussion. Since the authors mention several times that SWAP ET is the reference, I think you start by comparing ET from the energy models with SWAP. So section 4.2 should start with P17, L18-20 (which should be elaborated) and continue with a qualitative comparison of the energy balance models.

Specific comments

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P2, L2, L12, etc. there is no need to capitalize remote sensing, surface energy balance or agro-hydrological. Change to small caps throughout the document.

P2, L19-21. I do not see the link between Mediterranean vegetation and the reason to focus on the differences between two models? Is it not relevant for vegetation in moderate climates? P2, L22. "To evidence"; is not a verb, change to "proved". Change throughout the document since it was used more often.

P3, L12 van Dam, should be with capital V: Van Dam

P3, L16-17 One reference to Liu should be sufficient since it involves the same research

P5, L16-18. I do not understand the meanings of "remote acquisition" and "intensive data". Rephrase.

P7, L10 Please provide a better reference other than Liang (2004) for the NDVI

P7, L11 Do not mention soil heat flux again, just G0.

P7, L1 there is two time reference made to Brutsaert (1982)

P9, L20 is reference made to 1999a or 1999b?

P10, L17 Kroes an Van Dam is not in the reference list

P11, L17 Canopy parameters such AS crop...

P11, L20 spatial distributions is without "s", change throughout the text

P13, L27 remove "intensive"

P14, L25 crop height is without "t";

P15, L18 why is it a preliminary validation?

P16, L2 "has been carried out in correspondence of", this is not correct English. I suggest you leave out "correspondence of"

P18, L26 MD is not explained before. I have assumed it is the absolute difference between SWAP ET and SEBAL ET?

P22, L10 Kroes et al. is not used in the text

P24, L23 Warrick is not referred to in the text

P24, L28 Wosten is not referred to in the text

Table 1. reference should be Van Genuchten, with capital V. Also at other location in the text

Table 3. It would be useful to present average for the individual fields here since especially Citrus fields C1 and C2 show different ET pattern.

Figure 1. The quality is too low, it is difficult to read the text inside the fields. What is the geographic system? UTM?

Figure 3. You should use the same scales. The scale in Figure B is easier to read

Figure 4. Where is the citrus crop?

Figure 11. I would propose again to make average for the different fields instead of the crops altogether.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1, 2009.