Hydrol. Earth Syst. Sci. Discuss., 4, S286–S294, 2007
www.hydrol-earth-syst-sci-discuss.net/4/S286/2007/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Analysis of effective resistance calculation methods and their effect on modelling evapotranspiration in two different patches of vegetation in semi-arid SE Spain" *by* A. Were et al.

A. Were et al.

Received and published: 15 May 2007

We would like to thank the two referees of this manuscript for their enriching and interesting comments. We acknowledge their suggestions that for sure will help to improve this manuscript for its submission to HESS.

Here are in detail our analyses and replies to the different comments made by both referees.

ANALYSIS OF THE COMMENTS FROM REFEREE # 1:

Referee #1: General comment: This paper deals with the issue of estimating latent heat flux under heterogeneous conditions. This is indeed a very important issue for several disciplines i.e. hydrology, meteorology, agrometeorology. The authors tested 3 empirical approaches for defining effective or area-averaged resistances. Several papers have addressed this same issue for the past 20 years (Shuttleworth et al. 1997, Lhomme et al. 1994 among other) which have not cited in the references. I therefore suggest that the authors test other approaches, especially those based on the "matching-rule" of the surface fluxes.

REPLY: In this manuscript we dealt with the issue of aggregating heterogeneity to obtain parameters accounting for sub-grid heterogeneity, and applying them in models for estimating energy fluxes at grid-scale. Aggregation of parameters has been normally focused on the aggregation of patch-scale parameters (where patch-scale is the sub-grid scale) to obtain effective parameters at a scale where different patches are included (being this the grid-scale), which is the case of many meso-scale models and GCMs. Shuttleworth et al., 1997, presented an excellent paper where they analysed empirical and theoretical approaches to estimate effective parameters to be used in meso-scale models through aggregation of patch-scale resistances. In their aggregation rules they included the blending-height theory to reconcile the empirical and theoretical approaches. However, in this manuscript we have applied simple aggregation rules for soil and plant resistances (being this our sub-grid scale) to obtain the effective resistances at the patch scale (the grid-scale in our case) and afterwards we have applied them in a simple Penman-Monteith model to estimate latent-heat flux, in contrast with sparse-vegetation models. Parameterisations enabled us to obtain empirical equations for estimating the soil and plant resistances, and through simple empirical aggregation rules we obtained the area-averaged effective resistances of the patch. For this purpose, we used simple equations for aggregating soil and plant resistances, like the ones indicated by Blyth et al. (1993), instead of more complex aggregation methods like the ones used by Shuttleworth et al. (1997) or Lhomme et al. (1994), because in the latter case it would be more logical to use a sparse-vegetation model. In any case, we find the work of Shuttleworth et al. (1997) very interesting and we will

HESSD

4, S286–S294, 2007

Interactive Comment

consider it for future works on aggregation of heterogeneity for estimating latent heat flux.

Referee #1: Specific comments:

1) Specific comment, Referee # 1: A- The authors should address the issue of the footprint of the eddy covariance system using a footprint model.

REPLY: To answer this comment, we should point to a typo error in the manuscript. As it can be seen in Fig. 1, the size of the herbaceous patch is 104 m2 (100 m x 100 m), instead of the 100 m2 (10m x 10m) indicated in the manuscript. This may explain the concern of both referees (see general comment of Referee # 2) for the fetch, or footprint of the eddy covariance measurements. Considering the size of both patches, and the height of the eddy covariance systems, the authors considered that the fetch was sufficient for both patches.

2) Specific comment, Referee # 1: B- The number of soil heat flux plates is not enough to capture the variability of the heat flux encountered in this type of environment.

REPLY: The number of soil heat flux plates was two in each patch. In the case of the R. sphaerocarpa patch one plate was located on soil under a R. sphaerocarpa plant, and another on bare soil to consider the variability between soil under plant and bare soil. Even though we know that it would be better to have more heat flux plates, we did not have that possibility. We tried to compensate the lack in heat flux plates with a higher number of soil moisture measurements, used to calculate St (the heat stored in the soil above the heat flux plate), with 6 sensors on the herbaceous patch and 12 sensors in the R. sphaerocarpa patch (6 under plant and 6 in bare soil). The authors were aware of the high variability of the soil heat flux in this type of environment, but we used a similar experimental scheme used in successful previous works carried out on the same experimental field site and vegetation (for instance Domingo et al, 1999; Brenner & Incoll, 1997).

HESSD

4, S286–S294, 2007

Interactive Comment

3) Specific comment, Referee # 1: C- No detail is given how they scale up the measured resistances to the patch scale.

REPLY: In the manuscript, specification of how the measurements of the different resistances and conductances were made and averaged to obtain the different resistances of soil and plant of the patch (that were later used in the parameterisation of these resistances) was not done in order to simplify the Material and Methods section. However, below are detailed the measurements and averaging of the soil and plant resistances made in this work. Soil surface and aerodynamic resistances, as well as plant surface resistances, were measured in different positions, and then averaged to obtain the soil and plant resistances of the patch. In the case of soil surface resistances, they were measured with lysimeters, 6 in the case of the herbaceous patch, and 12 (6 under plant and 6 in bare soil) in the R. sphaerocarpa patch. The value of the soil resistance of the patch was an average of the measured values obtained in each lysimeter. In the case of soil aerodynamic resistances, they were measured with pairs of heated sensors according to McInnes et al. (1994, 1996). In the herbaceous patch there were 3 pairs of sensors, while in the R. sphaerocarpa patch there were 4 pairs of sensors placed in a gradient from under plant to bare soil (according to Domingo et al., 1999). The measured values obtained with each pair of sensors were averaged to obtain the aerodynamic resistances of soil, soil under plant and bare soil. In the case of the plant resistances, they were estimated from measurements of leaf conductance. In the case of the herbaceous patch, measurements were made in three leaves of three different species of herbaceous plants (which differed during the measuring period). The measurements made in each leaf were averaged to obtain a value of leaf conductance for each species, and these values were averaged to obtain the leaf conductance of the herbaceous plants of the patch. In the case of the R. sphaerocarpa patch, a similar procedure was followed (see Brenner and Incoll, 1997 for more details). Therefore, the scaling-up of the measured resistances was carried out by averaging the resistances measured with the different replicates for each case.

ANALYSIS OF THE COMMENTS FROM REFEREE # 2:

Referee #2: General comment: A question that is relevant to the Hydrology community, i.e. parameterisation in Penman-Monteith evapotranspiration modeling, was assessed using state-of-the-art measurement methodology (as far as I can judge) for each single term, and, according to the cited literature, state-of-the-art aggregation methods to be compared (which I cannot really judge). The manuscript is innovative in that it thoroughly examines the ability of these different aggregation methods to regain real evapotranspiration for a particular environment representing difficult aggregation conditions, i.e. scattered vegetation with almost bare interspace.

1) General Comment, Referee # 2 There is only one major point of worry to me (any other criticism is in the specific comments/technical corrections and will be easily met). This is the footprint (fetch) of the Eddy Covariance measurement on the herbaceous patch. If it was really only 100 m2 = 10 m x 10 m, this is far too small (about 100 m x 100 m would probably just be enough). Good energy balance closure is not a sufficient indication that these measurements were "correct" (in that they represented the patch). There are simple (probably sufficient for your terrain) freely available footprint models as by Schmid (1997, Agricultural and Forest Meteorology 87, 179-200, http://www.indiana.edu/˜climate/SAM/SAM_FSAM.html) or Kormann and Meixner (2001, Boundary-Layer Meteorology 99, 207-224). With such a model, the contribution of the herbaceous patch to the measured turbulent flux above the ground can be estimated (it will be less than 50%, I think). Comparing estimated evapotranspiration to a flux average of R. sphaerocarpa and herbaceous weighted by their modelled contribution would be the most straightforward way to test parameter aggregation a posteriori (a priori, the best way would be a bigger patch or an advection correction measurement set-up). Another interesting (but simplifying) possibility would be to regard all eddy covariance measurements as R. sphaerocarpa measurements (however, this way a herbaceous patch reference measurement for atmospheric aerodynamic resistance would be completely missing). Maybe you have other suggestions to solve it,

HESSD

4, S286–S294, 2007

Interactive Comment

but it cannot be left like this regarding the 2.5 high vapour flux over such a small patch as its evapotranspiration.

REPLY: The referee's comment is based on the fact that in the manuscript it is stated that the size of the herbaceous patch is of 100 m2. As it can be seen in Fig.1, this is a typo error of the manuscript, being the real area of the herbaceous patch of 104 m2 (100 m x 100 m, as recommended by the referee). With this size the measurements of vapour flux at 2.5 m high are representative of the herbaceous patch. A footprint analysis was not performed as we considered that the Eddy covariance system of both patches had a sufficient fetch to ensure that their measurements were representative of the patch.

Referee #2 specific comments:

1) Specific Comment, Referee # 2 p 245 l 14 ff: The sentence beginning "This may be due to..." is unsatisfying both from a linguistic and a topical point of view. Try to explain in more sentences more clearly what you want to say, or if you are very unsure, don't try to explain in the abstract at all.

REPLY: We agree with this comment and therefore this sentence will be removed from the abstract and left only for the discussion in the manuscript that will be submitted to HESS.

2) Specific Comment, Referee # 2 p 251 l 4: Is it really 100 m2 = 10 m x 10 m? See "General comments".

REPLY: As already referred in the Reply of the General comments, this is a typo error of the manuscript and must be replaced by 104 m2.

3) Specific Comment, Referee # 2 p 251 l 15: To me, as well as maybe to other readers, the term "Biomass is picked in spring" is unclear even though it may be a correct technical term. Was there some kind of harvest?

REPLY: What the authors wanted to indicated in this sentence was that in the herba-

HESSD

4, S286–S294, 2007

Interactive Comment

ceous patch the maximum biomass was reached in spring. The sentence will be rewritten to become clearer.

4) Specific Comment, Referee # 2 p 252 l 252: Calling z0+d the "mean flow height" is unfamiliar to me. It seems inappropriate as theoretically this is just where flow becomes 0.

REPLY: The term mean surface flow height referred to z0 + d is used by Brenner and Incoll (1997), and Domingo et al. (1999). Shuttleworth and Wallace (1985) define z0 + d as the height where the effective source, at which the mean canopy air stream conditions are assumed to apply, occurs. According to this definition other authors like Verhoef and Allen (1998 and 2000) refer to the height z0 + d as the mean canopy source height. As the term used in this manuscript may be confusing, although it has been used before, we will change it to mean canopy source height.

5) Specific Comment, Referee # 2 p 258, 1st paragraph: What about data where the wind direction was inappropriate for measuring the patch (e.g. North for the herbaceous patch)? Why are so few data left?

REPLY: It was considered that as the predominant wind direction (see Fig. 1) corresponded to 85% of the data, filtering according to the wind direction was not needed. Moreover, although the Eddy covariance systems were located at the North of the patches, they were around 20 meters away from the boundary of the patch, and centred on the East-West direction. Despite the fact that the data used in this work were enough for the analyses done, it may not seem so because no data filling was done. Only days where all instruments worked and data from all the energy balance components were available, without filling any data gaps, were selected.

6) Specific Comment, Referee # 2 p. 261, l. 22 ff: The slope (b) of regression is difficult to interpret as an indicator of "goodness" if R2“0.95. It will often be lower than 1 simply because of the fact that uncertainty in the correlation is replaced by y estimates closer to the average. You will see what I mean if you change x and y, and probably find that

1/b (x on y) > b (y on x). A more meaningful slope for such cases is explained by Webster 1997, European Journal of soil science 48, 557-566. As this would probably go to far here, I simply suggest to put less stress on b.

7) Specific Comment, Referee # 2 p. 262, l. 5 ff: This way to measure the "goodness" of estimates is indeed much better for your purpose than the regression (see comment above). Its results should be given priority in the discussion and conclusion. It would be interesting, however, to check if the same ranking results from the (percentage) root of the mean squared differences. This will give big differences a greater ("bad") weight and be closer to the regression philosophy.

8) Specific Comment, Referee # 2 p. 264, l. 6 ff: See comment above, the second half of the sentence seems more meaningful to me than the first, especially if it also withstands the RMS (root of mean squares) criterion.

REPLY: As the above three comments are related, we will answer them as a whole. We agree that the information obtained with the regression analysis is poor due to the low R2 obtained and the high intercept. However, we considered it adequate as a first approximation to compare measured and estimated values of evapotranspiration. According to the referee's comment, we think that in the corrected manuscript the analysis of parameter b should be eliminated, leaving only Figures 8 and 9 as they give a visual information on the relation between the different estimates and the measured values. As the referee indicates, the MPE (mean percentage error) or the MAPE (mean absolute percentage error) are more reliable for assessing the “goodness” of the estimates, as can be seen in the discussion. Also to corroborate these results we think that for an improvement of the manuscript for its submission to HESS, an analysis of the RMSE (root mean square error) will be also done.

1) Technical corrections, Referee # 2 All technical corrections indicated by the Referee will be corrected in the manuscript before its submission to HESS.

References:

Brenner, A. J. and Incoll, L. D.: The effect of clumpling and stomatal response on evaporation from sparsely vegetated shrublands. Agric. and Forest Meteorol., 84:187-205, 1997.

Domingo, F., Villagarcía, L., Brenner, A. J. and Puigdefábregas, J.: Evapotranspiration model for semi-arid shrub-lands tested against data from SE Spain. Agric. and Forest Meteorol., 95:67-84, 1999.

Lhomme, J.-P., Chehbouni, A. and Monteny, B.: Effective parameters of surface energy balance in heterogeneous landscape. Bound.-Layer Meteorol., 71:297-309, 1994.

McInnes, K. J., Heilman, J. L., and Lascano, R. J.: Aerodynamic conductances at the soil surface in a vineyard. Agric. and Forest Meteorol., 79:29-37, 1996.

McInnes, K. J., Heilman, J. L. and Savage, M. J.: Aerodynamic conductances along a bare ridge-furrow tilled soil surface. Agric. and Forest Meteorol., 68:119-131, 1994.

Shuttleworth, W. J. and Wallace, J. S.: Evaporation from sparse crops, an energy combination theory. Quart.J.R.Meteorol.Soc., 3:839-855, 1985.

Shuttleworth, W. J., Yang, Z-L. and Arain, M.A.: Aggregation rules for surface parameters in global models. Hydrol. Earth Sys. Sci., 2:217-226, 1997.

Verhoef, A. and Allen, S. J..: The relative importance of surface and aeordynamic resistances in a multi-source energy-CO2 model. Physics and Chemistry of the Earth, 23:459-463, 1998.

Verhoef, A. and Allen, S. J.: A SVAT scheme describing energy and CO2 fluxes for multi-component vegetation: calibration and test for a Sahelian savannah. Ecological Modelling, 127:245-267, 2000.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 243, 2007.

HESSD

4, S286–S294, 2007

Interactive Comment