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Hydrology and Earth System Sciences
Discussions

Interactive comment on "FAO-56 dual approach combined with multi-sensor remote sensing for regional evapotranspiration estimations" *by* R. Amri et al.

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The article fits within the scope of the HESSD journal by introducing an updated version of the FAO-56 model for evapotranspiration estimates using a combined use of two products from Earth Observation - soil moisture and vegetation greenness. The goal of the article is clear and is reflected by its name, little worse then is this reflected in the abstract. The innovativeness of the article, as it is now, is hidden in the shadow of acceptance of algorithms and coefficient from other publications (e.g. Erraki 2007, Merlin 2011). The authors should reassure that the innovative aspects of the article (e.g. the duality of the FAO model, the inclusion of the soil moisture products from

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active microwaveremote sensing in the model in combination with EO NDVI, or the alternations to the existing algorithms) are clearly explained in the article and highlighted in the abstract and in the conclusion and that these correspond throughout the entire article. For instance, you developed not used a dual FAO-56 model. You also implemented synergistically two independent EO datasets in the model. Importantly, some logical analyses are missing. In particular, I was expecting to see a comparison of the performance of FAO-56 model with and without inclusion of the EO products and a comparison of FAO-56 one layer and dual layer model. Comparison with ISBA is interesting but doesn't show the added value of including EO data and the added value of the duality of the FAO-56 model. Without the latter analyses, you can only hardly justify the dual layer FAO-56 model and thus the entire article. The discussion of the results is very week and needs a thoughtful improvement. You should give authors the hints why over or underestimation happen in regards to the structure and the forcing of the models. Justify, why did you perform the analyses between ERS soil moisture and SVAT soil moisture. Does this give more weight to the evaluation of your evapotranspiration model with SVAT? The English language needs a considerate improvement. Answer: We thank reviewer for these constructive comments. We will propose an improvement of results discussion. We will try to show clearly innovative aspects of the article, based on combination of FAO-dual approach and remote sensing, not only for vegetation characterization, as it was proposed by different studies (González-Piqueras, 2006, Erraki et al., 2007, Eraki et al., 2010), but also by considering satellite moisture products for soil evaporation estimation. Second, our second objective is to use FAO approach, proposed for irrigation management, to estimate regional evapotranspiration level, and then the interest of including remote sensing, for vegetation and also soil moisture, to consider temporal and spatial variations of these two inputs. At regional scale, it is not easy to consider validation with ground flux measurements. For this reason, we consider the comparison with ISBA SVAT model, operational tool of Meteo-France, to show the reasonable use of the simple approach based just on FAO and remote sensing to retrieve temporal variations of ET. We agree

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with reviewer, the English language will be improved with the help of English native. Improvements in the abstract are necessary: i) how did you consider the second vegetation class in your version of the FAO-56 model?, ii) include results of your study and iii) highlight innovativeness Yes, we agree with reviewer. Abstract will be modified. “The aim of the present..use of this simple tool with remote sensing”..note that your tool is simple only because is specifically made only for a certain geographical area and certain vegetation types. Yes, this tool is not particularly simple, but it has the potentiel to consider the spatial and temporal variations of land surface parameters. With the increasing of the number of operational products (for vegetation and soil) and long temporal series for different types of sensors (SPOT-VGT, MODIS, ERS, ASCAT/METOP, SMOS, ...), it becomes possible to consider the potential of these products as input in models like FAO, for different areas. Certainly, for semi arid areas with limited and dispersed vegetation cover, analysis could be less complicated than for other areas. You don’t have to explain the linear mixing theory (last paragraph on 8123 and first 5 sentences in 8124). The readers are expected to know the general concepts. Rather provide little more info on how you implemented it, especially how do you solve equation 1 and how do you consider time. Here we consider again the equation 1. Yi(t) =$\sum\_(j=1)^p$ πij×j(t)+εi(t)ãĂŮ Yi(t) is the average signal observed at pixel i and at time t,it is estimated from the NDVI time series of SPOT-VGT. j(t) is the signal assigned to class j at time t. this term is claculated for each class (we consider pure pixels) from the NDVI time series of SPOT-VGT. Therefore the unknown in this equation is the πij which is the area occupied by the class j in pixel i. You missed explaining several parameters in equation 4 and 5 (e.g. KS, FR, fc-0) Yes, we will add these parameters: ÆŠc is the fractional covers ; Fr is the percent cover per pixel for each class; The indices "o" and "c" denote respectively the classes cereals and olive groves.

Equation5: are you using completely identical coefficients to Erraki? Are you allowed to, are we talking about an identical site?/vegetation cover? The equation 5 proposed function of NDVI is proposed for cereal cover. We used the same equation established by Er-Raki et al., 2007 on the Tensift basin in Marrakech (Morocco) but with the ND-

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VImin and NDVImax values, retrieved in our site. The Tensift (Morroco) site considered in Er-Raki developments and Merguellil site present a high resemblance for cereal land use (climat, yields, irrigation,. . .). Which algorithm does ISBA use to determine evapotranspiration? Explain better the portion of the model related to evapotranspiration. In that paper, we compared our outputs with the outputs of ISBA-A-gs models, we will add more clarifications.

Scheme of water transfers ISBA model Where d1, d2, d2: the diffusion coefficients in the soil; The Model functionning contains : -Water and energy balance - Big Leaf model : one energy balance with soil/veg horizontal partition - Force restore for heat and water soil transfers (3 layers) - A-gs (Calvet et al, 1998): simulation of photosynthesis and its coupling with stomatal conductance (forced LAI) - Standard ECOCLIMAP parameters

Clearly describe what is SVAT and ISBA and their difference. SVAT models describe the exchanges between soil plant and atmosphere according to the physical processes occurring in each compartment. ISBA is the surface scheme based on the force restores method (Noilhan and Planton, 1989) which has been widely used coupling assimilation methods with remote sensing data (Calvet et al, 1998 ; Olioso et al, 2002a). So ISBA is a physical SVAT model.

Please comment on the possibility of transferring equation 5, 7 and 8 to other geographical regions. This appoach is tested on Merguellil site. Yes, the proposed approach could be applied to other regions with some adaptations. Proposed eqautions are developed for cereal or olive classes, but we can consider other proposed relationships for other classes. Page 8127, line 18 “Merlin et al. was adapted”, how was the original equation adapted. In other words, clearly explain your contribution to the equation. Here we mean that the Merlin approach is developed to estimate the soil evaporation coefficient. How did you implemented the ERS soil moisture in equation 7. Have you performed bias correction? Before application of these products, we propose their validation over the same site with continuous ground measurement acquired with thetaprobes (Amri et al., IEEE TGARS 2012). Soil moisture at saturation is identified

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from these ground measurements. In the equation 7, we consider volumetric soil moisture computed from soil surface moisture index proposed by Vienne university products (Pellarin et al., 2006, Amri et al., 2012).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8117, 2013.