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

Anonymous Referee #1

Received and published: 1 August 2013

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 active microwave

remote 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.

See also following, more detailed, scientific and technical comments:

a) 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

b) "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

c) 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.

d) You missed explaining several parameters in equation 4 and 5 (e.g. KS, FR, fc-0)

e) Equation5: are you using completely identical coefficients to Erraki? Are you allowed to, are we talking about an identical site?/vegetation cover?

f) Which algorithm does ISBA use to determine evapotranspiration? Explain better the portion of the model related to evapotranspiration.

g) Clearly describe what is SVAT and ISBA and their difference.

h) Please comment on the possibility of transferring equation 5, 7 and 8 to other geographical regions.

i) 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.

j) How did you implemented the ERS soil moisture in equation 7. Have you performed bias correction?

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