Interactive comment on "**Inter-comparison of four remote sensing based surface energy balance methods to retrieve surface evapotranspiration and water stress of irrigated fields in semi-arid climate**" MS No.: hess-2012-545. by J. Chirouze et al.

1. General comment

the Authors present an extensive work (reinforced by meaningful experiment data) aimed to assess and compare four remote sensing-based methods for surface evapotranspiration and water stress estimation from crops under semi-arid conditions. I think that Authors address relevant scientific questions within the scope of HESS and therefore the paper could be taken into account for the final publication.

Despite the topics presented are actual and the experimental data are important, the paper should be improved according to the points that I synthesized below in the sections "manuscript organization" and "concepts to be clarified". Smaller comments regarding other specific questions are given below in the section "specific comments and technical corrections ".

2. Manuscript organization

I found this paper difficult to read especially in the first three paragraphs (1. Context and objectives; 2. State of the art; 3 Material and Methods). On the other hand the part of manuscript describing Results is well organized with the minor exception of some graph that could be improved in format and style and some unclear explanation about Water Stress parameter (pag. 921).

The main problem of the first three paragraphs is an excessive fragmentation of concepts that could create confusion and misunderstanding for the reader.

The Authors used too many sub-paragraphs inducing confusion; some fundamental topics (state of art, objectives, description of models, etc.) result scattered in many parts of the test. An example of this confused organization of the first part of manuscript is the lack of the goal of research in the "Context and objectives" paragraph (pagg. 898-899) that are, in contrast, explained in the "State of art" paragraph (pag. 902, lines 8-14).

Therefore, I suggest, as preliminary correction, a re-organization of manuscript (essentially the first part) using a "more classical" sequence and nomenclature of paragraphs; contextually, I will try also to suggest how re-join "all the pieces of puzzle" to remove the above mentioned fragmentation of concepts.

To do this, in the following table I defined a new list of paragraphs that Authors could use, the order of topic to be include and the parts of original manuscript to be used for this. In the next section (Concepts to be clarified) I reported integrations that should be also included.

New paragraphs	Topic and/or subparagraph	Original Manuscript
1. Introducion (only one paragraph !!)	a) Main subject and reason of its importance; b) Little state of art; c) Aim of work;	Paragraphs 1 and 2.. .. with some corrections..
2. Material and methods	2.1 Theoretical backgroung (briefly description of RS approaches used for ET estimation) (description of SVAT/ICARE approach and rationale discussion about the choice of its use as reference) 2.2 Site and data description. 2.3 Data preprocessing	Paragraph 2 (eq. 1). Paragraphs 3.3.1, 3.3.2, 3.3.3 (description of main SEB equations that are communal and details in cases of differences). Paragraph 3.3.4 Paragraph 3.1 Paragraph 3.2
3. Results	3.1 Analysis of "ancillary" data (albedo and surface temperature). 3.2 Analysis of terms of surface energy balance and their spatial variability; 3.3 Assessment of water stress and its spatial distribution;	Paragraphs 4.1 and 4.2 Paragraphs 4.3, 4.4, 4.5; Paragraph 4.7 Paragraphs 4.6 and 5.3
4. Discussion	Performances inter-comparison and model's structure and improvements	Paragraphs 5.1 and 5.2
5. Summary and conclusion		Paragraph 6

3. Concepts to be clarified

3.1 About the use of ICARE/SVAT model as reference

The performances of the four RS models were evaluated through a comparison with a set of micro-meteorological EC system and also using the spatially distributed outputs of ICARE/SVAT model. The latter, as described by Authors (pag. 915 lines 24,25 and pag. 916 lines 1-5), is …"*a classical dual-source SVAT model that solves the water balance of the surface*"…..using "a two layers force restore model" ..that.. "*simulates the evolution of soil moisture and temperature for each soil layer (shallow and root zone). Thus, as a dynamic model, it is given initial conditions in surface and root zone temperature and moisture levels; therefore the surface temperature is not an input but an output*".

In my opinion the "water balance" sub-model should be better described because in ICARE/SVAT the outputs of energy balance sub-model, i.e. evapotranspiration (used

as reference for model's comparison) strongly depends on water balance sub-model, through the "force and restore" approach. I think that the SVAT approach (ICARE) can be usefully used for an inter-comparison exercise, but its characteristics should be clearly explained.

On the basis of this argument, I think that the Author should better explain in the revised introduction of paper (state of art) the characteristics of ICARE/SVAT and the reason of its use as reference.

Moreover in the "Material and Methods" the Authors should also describe and detail data and parameters used in WB sub-model of ICARE model (hydraulic soil parameters, depths of soil layers, initial conditions in surface and root zone, conditions at the bottom of soil profile, root uptake functions, etc,).

3.2 About Remote Sensing methods and imagery data-set

Another question that the Authors should clarify is the definition of the "main families" of Remote sensing method for ET estimation. The Authors used the following classification:

1. Contextual methods (pag. 899, lines 21-24) …" *all approaches based on the simultaneous presence, at the time of acquisition, of hot/dry and cold/wet pixels within the satellite image, for a sufficiently large range of vegetation covers or surface states*".
2. Single-pixel models and (pag. 899, lines 3-4) … "*methods*" .. that "*solve an energy budget for each pixel independently from the others*".

In my opinion this type of classification is not properly appropriate because, for example, in the group 1 can be included SEBS model which uses (as described by Authors at pag. 909, line 5) the concept of "hot/dry" and "cold/wet" pixels as boundary conditions. Moreover, following the classification proposed by Authors, in the group 2 could be partially considered the S-SEBI model, that computes for "each pixel, independently from the others", the evapotraspiration term by means of a simplified relationship for Evaporative Fraction calculation.

Therefore the Author should use a different way to classify the method for ET estimation from Remote Sensing data. I can suggest to use this type of classification (or similar):

1. Simplified energy balance index methods (for example S-SEBI and similar, that are all methods based on an analysis of the relationship between albedo or NDVI and surface temperature to obtain a simplified equation for the calculation of Evaporative Fraction or ET).
2. Direct energy balance methods (for example SEBAL, TSEB, SEBS, that are all methods based on the direct estimation of the latent heat flux, λET, as residual term from the surface energy balance equation). Within this family a distinction has to be done between:
    2.1- Single Source approaches (as SEBS or SEBAL), where soil and vegetation are considered as a combined sole source;

2.2 Two Source approaches (TSEB), where soil and vegetation are treated separately.

Furthermore, a part the type of classification, considering that the Authors compared SEBS (single source) and TSEB (two source) models, in the next version of the paper they should explain the main differences between "single-source" and "two-source" approaches to estimate sensible heat flux H that is term with the largest uncertainty in estimating λET.

The Authors used both ASTER and FORMOSAT-2 imagery data-set. ASTER data were used to exploit its Thermal band and FORMOSAT-2 for VIS-NIR bands. Indeed, ASTER provides also VIS-NIR and SWIR bands useful to compute albedo, so I don't understand the need to use FORMOSAT-2 data. Moreover using the greater number of VIS-NIR and SWIR ASTER bands the computation of albedo would have been improved respect to the method used by Author (eq. 2).

3.3 <u>About definition and estimation of Water Stress (Pag. 621 L:12-26).</u>

The Authors used as Water Stress index the term: $1 - \lambda E/\lambda E_{max}$, where λE and λE_{max} are actual and potential latent heat flux from a plant, respectively (pag. 921, L:10-12). This definition is not properly correct, because the actual Stress of a plant should be related only to the transpiration term removing the evaporation term. Moreover the term λE_{max} was computed using ICARE/SVAT model with the option of continuous irrigation. Sincerely, I don't well understand this choice and I have the doubt that the comparison between the Water Stress indexes derived from models (Fig. 11) could be not properly homogeneous. About this problem my question is if a more homogeneous comparison could be conducted using only TSEB and ICARE (the only model that, being dual-source, are able to retrieve actual transpiration) and using reference ET_0 in place of $\lambda E_{max.}$

4. Specific comment and technical corrections

P:897, L:1-4. "*Remotely sensed surface temperature can provide a good proxy for water stress level and is therefore particularly useful to estimate spatially distributed evapotranspiration*". It is on the contrary: RS can provide a good proxy for ET estimation and is therefore useful to quantify water stress stress.

P:897, L:5. Clarify the term "equilibrium temperature".

P:897, L:7-11. Reorganize following my previous comment (3.1).

P:898, L:1-4. Add some references about "water use" data.

P:899, L:11-18. Clarify the nature of SVAT and the reason of its use as reference for Remote Sensing approaches. Clarify also the data-assimilation concept .

P:899, L:17. Check the reference "Schuurmans et al., 2003 (..or Schuumans ?).

P:899, L:21. Reorganize following my previous comment (3.1).

P:900, L:28. "Choi et al., 2009" is not reported in the Reference list.

P:901, L:2. Invert years in "Su et al., 2007,2005"

P:901, L:2. *"..in most cases, those studies.."* … *"and are limited to two or three inter-compared models".* This comment is not useful, after all the Authors inter-compared four models !!.

P:901, L:15-20. Reorganize following my previous comment (3.2).

P:902, L:8-14. These are the objectives of work. Move at the end of new introduction.

P:907, Eq. (2). Clarify following mu previous comment (4).

P: 908,909,910,911. In my opinion these are the pages where the description of models could create confusion. On the basis of previous comments about the classification of Remote Sensing method (3.2), I suggest to explain how the models compute Evapotranspiration using this order:

1. Description of surface balance equation to obtain the instantaneous λET as residual term ($\lambda ET = Rn - H - G$) and definition of Evaporative Fraction, EF.
2. Description of methods to compute Rn and G (as at pag. 908) that are common in TSEB and SEBS;
3. Description and discussion of the differences in H estimation between TSEB and SEBS (also following my previous comment 3.2). To do this, I think that it is useful to describe, at first, the general equation for H ($H=\rho cp\Delta T/Ra$ and its modifications in case of two-source approach, H=Hc+Hs); then, all terms used to describe wind and temperature profiles according Monin-Obukhov could be described in a synthesized form (Is it necessary to shown eqs. 7, 8, 9, 10, 11?)
4. Describe the simplified methods (S-SEBI and VIT).

P:909, L:15. Clarify the concept of "potential temperature".

P:916, L:15-20. *"As a complex physical model, ICARE use a large set of input parameters describing the different properties of the surface (soil and vegetation). Those parameters need to be calibrated in order to obtain consistent results*"…." we *chose to run the model in its most standardized version, with literature or measured values, when they are available, except for the soil resistance to evaporation*".

This part is crucial (see my previous comment 3.1). As the ICARE is used a reference the Authors should be better detail this part of work.

P:920, L:19. The comment about the kB^{-1} parameter is not clear, ..it is "*too bi*g" respect what ?

P:920, L:21. The comment about the about the overestimations of H should be more detailed.

P:921, L:13. "for a p." ?

P:921, L:13-24. Really, I did not understand this part. Please, clarify (See my previous comment, 3.3).

P:928, L:18. "Shuttleworth and Wallace (1985" is not reported in References list.

P:941, Table 3. Specify unit for H.

P:953, Fig. 5. Insert labels and units in x and y axis and remove title.

P:954, Fig. 6. Insert labels and units in x and y axis and remove title.

P:955, Fig. 7. The size of figures is too small. Insert labels and units in x and y axis and remove title. Insert the same tics in both axis.

P:956, Fig. 8. The resolution of captured figure seems too small. Insert labels and units in x and y axis and remove title. Insert the same tics in both axis.

P:957, Fig. 9. Insert labels and units in x and y axis and remove title. Insert the same tics in both axis.

P:958, Fig. 10. Insert labels and units in x and y axis.

P:959, Fig. 11. Insert labels and units in x and y axis.

P:960, Fig. 12. The size of figures is too small.

P:961, Fig. 13. The size of figures is too small.

P:962, Fig. 14. The size of figures is too small.

P:963, Fig. 13. The size of figures is too small. Insert the same tics in both axis.