

Interactive comment on “Remote-sensing algorithm for surface evapotranspiration considering landscape and statistical effects on mixed-pixels” by Z. Q. Peng et al.

Anonymous Referee #1

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General Comments:

This manuscript compares two different approaches to retrieve energy budget components (including sensible and latent heat flux) at the land surface using satellite data from the Chinese HJ-1B. One approach (IPUS) uses information aggregated to the 300m resolution as given by the thermal channel; the second approach (TSFA) uses a temperature sharpening approach, making use of a NDVI – TS relationship and down-scaling 300m Ts information to the 30m scale. Authors illustrate the differences between both approaches and demonstrate within a validation exercise the advantages and improved prediction capacities of the latter approach. I think this comparison and the results obtained are in principle worth publishing and will be of use for the read-

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ership of HESS. However, before a possible publication, author need to address and solve some significant concerns and questions that came up when working through the manuscript.

1. One of the major deficits of the manuscript is the following: The satellite data available are the 30m resolution data in the VIS/NIR spectral region and the 300m thermal information. As a “standard/normal” remote sensing user, I would try to make use of this available information. That means in a “reference application” (as it appears to me the IPUS scheme is meant to be) I would try to make use of the 30m data to derive NDVI and land use information (and all the other relevant parameters such as vegetation height, vegetation cover, roughness length etc., but also the simplification of individual fluxes for given LU type). Why are these parameters aggregated in the IPUS approach? Why don't use the high resolution information with an aggregated 300m Ts-signal. This should be compared to the TSFA approach in order to be able to evaluate the effect of purely temperature sharpening. Here actually the baseline situation is first worsened by aggregating information that is available in much higher resolution. In case the intention of the authors was to show what can happen when also in the VIS/NIR range only 300m resolution data were available, then all the (300m) average land surface parameters should have been derived from the aggregated reflectance information. So, I personally feel here are different aspects mixed and not properly separated.

2. The title of the manuscript suggests that the focus of the paper is on evapotranspiration – when looking through the manuscript and figures and tables, it seems to me that sensible heat flux is dominating the content and discussion. As a result, I would suggest to either change the title or put some more emphasis on ET in the presentation and discussion of results As a result of my evaluation I would suggest major revisions of the manuscript before a possible publication in HESS.

Specific Comments/Questions

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- In general, there are a very large number of abbreviations used in the manuscript – not all of them are intuitive and it is painful to always try and find the first position where they are explained. So I would suggest generating a list of abbreviations.

- Figure and table legends are not self-explaining throughout the manuscript and need extension!

P2L14-22: While this paragraph is ok in principle, we as hydrologist all know how important ET – so in order to come quicker to the point it should be omitted.

P6L17: Why choosing the 25% fractions having the lowest CV? Please explain in the text!

P9L11: How is Ld calculated in the scheme?

P12L21ff: It remains unclear how albedo is calculated

P14L11: briefly describe how this is expressed/described (Ref)

P16L2: What reliable methods? This needs to be more specific and with references

P17ff: In the section 4.1 surface parameter and fluxes derived are evaluated against measurements. In order to put those results into a general context I think a discussion and comparison in relation to other international Remote sensing/Flux measurement campaigns should be given.

P20L9-10: This statement about errors is not very specific!

P22L5: How do you justify a ground heat flux of 0 for buildings?

P23L22ff: This statement is actually a result of what is summarized under point 1 in the general conclusion.

P24L25ff: Why do you use these specific day for calculating the sensitivities! In fig. 12 the x-axis shows variations in %. This makes it difficult to follow the interpretations of the curves in the section.

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P29L5: Why do authors suddenly come up with the two source model – why didn't they use it initially?

P30: While the difference between Ts and Taero has been mentioned in the introduction, why isn't that problem discussed here!

P50: Table 13 – there is an error in the definition of the relative error (twice the same expression in the difference)

Minor Comments:

P3L4: Surface resistance is also needed for schemes classified under (1) because closure schemes need to calculate H where ra is required as well.

P3L20: Which models? All those listed in (1) - (5) or only those in (5)

P3L24-25: I do not understand "... inhomogeneity is a relative concept of homogeneity...!???"

P3L26: Density of what?

P4L4ff: I do not understand that sentence/statement!

P13L18: what is $\langle \dot{d}t \dot{N} \rangle$ in equation (15)?

P14L1: Sentence (... H Li et al ...) does not make sense

P14L7: What is 6SLUT? Reference!

P28L1: Sentence (... greatly decreased the heterogeneity) does not make sense

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