

***Interactive comment on* “Comparison of different evaporation estimates over the African continent” by P. Trambauer et al.**

Anonymous Referee #2

Received and published: 5 September 2013

General comments.

The research starts from the observation that few evapotranspiration intercomparison studies exist, none of them being focussed on the African continent. Indeed, the main novelty of the paper is to provide results for a large panel of regions and climates of the African continent. To reach this purpose, the authors suggest a detailed regions classification in Section 2.2. Furthermore, they consider 3 broad modelling/approaches classes (hydrology, land surface, remote sensing) and they select one or two models in each of them. However, the criteria used for model selection are not explained in the text. Each modelling approach is said ‘representative’ of its class but the meaning of this representativeness is not explained. Can the output of the selected models be considered as a benchmark of the output of a larger set of existing models belonging

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to each class? Previous studies have been done at the global scale and should be referred to give at least a partial information on the relative position of the selected models in their specific category (eg . multi model analysis and GEWEX LandFlux results). Furthermore the authors should specify and justify if they expect a larger ET variability between classes than within each class. It is likely that another model selection would have provided different results. Considering the ‘remote sensing’ class as specific looks arbitrary and meaningless in the sense that remote sensing input could be usable as input in any of the other two classes. Even remote sensing products are based on ET algorithms. Classification should avoid confusion resulting from criteria based on input data and methods.

The study doesn’t consider any validation against in-situ observations. As stated in Section 2.1, the models have mainly been validated for North America. The article mentions that nevertheless the MODIS ET product has in addition been compared in Asia but that it gives poor results compared to observations. As a consequence, model performance in Africa seems largely unknown and neither the models nor their mean behaviour can be considered as a reference. In this way, the comparisons done with EM can only be indicative of the dispersion resulting from the considered models.

These aspects should be better reflected in the introduction section.

The objective of the current paper is thus more to discriminate areas where a good consistence can be found between results of the selected models in contrast with regions where models output diverge. However, a simple intercomparison is useless if it doesn’t go further in the interpretation of the differences. The objective of the paper should then be extended in this direction. Indeed, the uncertainty in models output varies as a function to uncertainty in 1) model forcing; 2) ET modelling and parameterizations; 3) values of model parameters. The paper investigates part of the common input, but none of the meteorological variables, excepted the precipitation (point 1); it interprets superficially the impact of ET algorithms differences (point 2) and doesn’t consider at all parameters values (point 3). Of course, to provide a full answer to all

these aspects is challenging and a full in-depth study of all of them is probably out of the scope of a single paper. Anyway, the announced objective of the study stated in the introduction should be more ambitious than simply 'doing an intercomparison'. In particular, the content of the discussion section should be part of the results. Instead, the discussion section should state how the present work contributes to the problematic investigated by the international research community and what are the suggested directions for future work.

Specific comments.

Introduction: although not available on the studied 2000-2010 period, a new operational ET product is produced by the EUMETSAT LSA-SAF (Ghilain et al., 2011, HESS, 15, 771–786) and should be cited.

Overall results: Monthly mean results are investigated over a multi-annual period (2000-2010). Monthly standard deviation of daily product could possibly differ from on method to the other. Without displaying all the results systematically, could you comment on how they compare ?

Potential ET (PET) (section 3.1):

i- introduce PET definition, discuss concept(s) and possible differences among models;

ii- differences between 3 PET products (Fig 4): what are the most plausible estimations? Why so large differences ?

iii- comment on the possible particular input/parameters used to compute PET; specific parameters used as a function of vegetation types if any;

iv- If relevant for the discussion, add when possible a figure with annual or monthly standard deviation based on daily products over the same period and comment it.

v- MOD16 PET missing in dry and arid areas: is there a known justification ?

Actual ET (AET) (section 3.2):

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i- Focus on interpretation of the AET results. Discriminate as much as possible between impact of (1) input data, (2) modelling options/parameterizations, (3) parameters and make a link with the different considered regions when relevant;

ii- P. 8437, L 23-26: in view to provide an interpretation, mention clearly in which model versions water bodies are considered and how (by reference to the used algorithms);

iii- P. 8440 – 8443: regional analysis. Summarize the intercomparison results and focus on the interpretation. Move the detailed intercomparison description by region in Annex together with figures 8-9(-10).

Discussion/Conclusions:

i- see general comments;

ii- provide insight into the input of the research compared to previously available information mentioned in the introduction part (Vinukollu et al., 2011; Jiménez et al., 2011; Mueller et al., 2011)?

iii- can we rank the results as a function of class belonging (see introduction)?

iv- section 4.1.2 meaningless in the discussion section at the current stage: similar input (GLOBWB and PM PET) in PCR gives similar output (AET) !!! But reasons of high differences between GLOBWB/PM and MODIS PET should be elucidated because anticipated impact on models like PCR could be high.

Technical corrections.

General:

i- differentiate usage of 'model', 'data', 'product' in the text (e.g. P 8424, L4: revise sentence: (i) remote sensing, (ii) continental-scale hydrological models, (iii) land surface models are not 'complementary data').

ii- Some of numbers and words in the figure are too small. Check that character size

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used in figures allows text to be readable in the printed version.

iii- enlarge figure 8-10, considering other numbers of columns to improve space usage on the page;

iv- suppress Figure 10 (Taylor diagrams) if it doesn't add information compared to Figure 9;

Details: - P 8423, L12: cite FLUXNET;

- P 8426, L28: provide information about correction of precipitation from October 2009 ?

- P 8427, L13: information about quality (Droogers and Allen, 2002)?

- P 8427, L15-16: also likely less sensitivity to climatic input data, with possibly a reduction of dynamics and accuracy;

- P 8427, L20-22: a proper calibration of the Hargreaves relationship seems important. What about the parameter value selection - and what is its impact - in the current work ?

- P 8432, L20: 'several satellite-sensor products': which ?

- P 8433, L7-16: multiple references to 'Zomer et al. (2008)'; perhaps some of them are unnecessary;

- P 8433, L23-24: 'area-majority technique': provide details;

- P 8434, L 22: change the title 'Comparison of evaporation products' to 'Methods for comparison of evaporation products';

- P 8438, L 16: "We defined": how ?

- P 8439, L 23: 'assimilation increments': can you comment on its seasonal variation ?

- P 8440, L 12: MOD16 poor consistency: can you give an interpretation?

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- P 8458-8459: Combine Fig 4 and 5 in a single figure (can be with 3 columns and 2 rows labelled a to f).

Overall, the paper is well written and easily understandable. It could still be improved by reviewing phrasing, by avoiding some repetitions, and by looking to simplifications when possible.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8421, 2013.

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10, C4675–C4680, 2013

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