

Interactive comment on “Improving evapotranspiration in land surface models by using biophysical parameters derived from MSG/SEVIRI satellite” by N. Ghilain et al.

Anonymous Referee #1

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Review of

Improving evapotranspiration in land surface models by using biophysical parameters derived from MSG/SEVIRI satellite

by Ghilain et al.

General comments:

This is a very interesting paper on the use of satellite-derived LAI estimates into land surface models. LAI products derived from METEOSAT are considered, together with

C5348

an innovative method to downscale mean LAI values to biome LAI values. As this method is based on land cover maps, the impact of the mapping uncertainty is investigated. Finally, the impact of using different LAI products on simulated surface fluxes is shown. The use of in situ flux tower data permits showing the added value of refining LAI. The English is rather poor sometimes and some copy editing work is needed. While Section 1 (Introduction) is well written, the organization of the remaining Sections is not classical. Indeed, perhaps because this paper is mainly dedicated to the description of methods, the authors have chosen to embed results and discussions in each thematic method section. This is somewhat perturbing and gives the impression that several studies are described in the same paper. The results using the heating-rate estimates are not useful. The discussion Section is not in line with the content of the paper. The paper is long (18 figures), and reorganizing the paper using a more classical structure (introduction, material and methods, results, discussion, conclusion) would probably help the authors focusing their work more.

Recommendation: Major revisions.

Particular comments:

Title and throughout the text: please replace “biophysical parameters” by “biophysical variables” when time-varying quantities are considered.

P. 9122, L. 1: What do Nafr and Safr mean ?

P. 9122, L. 8: To what extent can raw ERA-Interim data be trusted? For example, Szczypta et al. 2011 (HESS) have found that precipitation can be largely underestimated.

P. 9123, L. 5: “ecosystems”?

P. 9124, L. 18: Why using this value of 2.5 and not another value ? If a parameter was tuned, this should be clearly mentioned. More information about the error on the climatology should be given. Is it lower on average for METEOSAT than for MODIS ?

C5349

P. 9125, L. 6: The LAI linear interpolation at wintertime is shown in Fig. 2 for an evergreen coniferous forest. What is the impact of this hypothesis for other biomes (e.g. deciduous forests) ?

P. 9126, L. 14: How are these weighting factors determined ?

P. 9129, L. 11: Why are the sites of Fig. 4 randomly chosen ? It seems that many sites correspond to areas seen by METEOSAT at a high incidence angle.

P. 9129, L. 12, L. 15: Do you mean "the 3 land cover maps" ?

P. 9129, L. 16: The caption of Fig. 5 should indicate the "neighbourhood size".

P. 9131, L. 15-16: Do you mean that with option 1, no further LAI correction is made ? This should be clarified.

P. 9131, L. 20: The caption of Fig. 8 should be complete for understanding and indicate the meaning of a "Hg pixel".

P. 9136, L. 11: Why is Table 4 presenting these sites in particular ? Have they been chosen because the use of the LSA LAI improved the model LE simulations ?

P. 9138, L. 1-22 and Figs 17-18: These new results are based on a new product (LST), not described before. Heating rates are probably influenced by soil moisture, and also by many other factors. Deriving conclusions on soil moisture from a few images is far-fetched. All these results seem out of the scope of this paper, they are not properly analyzed and discussed. Therefore, they should be removed.

P. 9138, Section 7: This Discussion Section is very technical, and most of it should be removed. Advertising future space missions has little to do with the scientific questions addressed by the paper. This Section should be revised completely, using the Discussion material disseminated in the other Sections of the discussion version of the paper.

C5350

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C5351