

Interactive comment on “Variational Assimilation of Land Surface Temperature within the ORCHIDEE Land Surface Model Version 1.2.6” by H. S. Benavides Pinjosovsky et al.

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Dear Reviewer,

Thank you for your review and for the interest in our work. I make list of answers regarding all your comments and questions

Specific Comments Section 1: P.2, L.31: could you precise what is a “specific deep land surface temperature” ? The sentence has been changed in the revised version because it was wrong and related to another paper not referenced here. The new sentence is now: "When assimilating LST into the model, the authors proved that the assimilation of LST can improve the model simulated heat and water fluxes. "

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P3, L.1: “or” should be replace with “of” Modification taken into account

P3, L3: remove “available” Modification taken into account

Section 2: P4., L.34-36: The SECHIBA version used has a “two-layer soil profile” meanwhile in appendix A (P.28, L.8-9) a “seven-layer soil profile” is mentioned for the THERMOSOIL subroutine. Please bring some precisions or corrections. A two-layer hydrology was used in this ORCHIDEE version. The seven layer discretization is for the resolution of the heat diffusion equation. We have changed the text in the paper to make it clearer

P.4.: L.1-12: could you precise why do you prefer the use of a brightness temperature in the interval [8-14] microns instead of the LST ? I certainly misunderstand the explanation. The use of this variable follows my previous thesis work (Benavides, 2014) when observations coming from a thermal infrared radiometer were used as observations (SMOSREX). This interval correspond to the radiometer filter used for these measurements.

L.6, Eq.1: the Stefan Boltzmann constant [σ] has been omitted in the first term of the equation. L.6, Eq.1: is LW_down estimated or measured in situ ? In this case, could you precise the spectral band associated and if a band factor has been applied to take into account that only a fraction of the radiation is measured in the spectral interval according to the Planck’s law at the difference of the Stefan-Boltzmann law. Precisions are thus required regarding the use of the Svendsen conversion function (Eq.2). We don’t understand your remark: in equation 1, we wrote the total radiation emitted by a soil surface and integrated on all the long wave spectra. The SB constant don’t appear on the left side of the equation. In our case, LW downward is measured by a large band radiometer and this is why we can use the Svendsen’s formula to estimate LST. The manuscript has been revised to clarify the notations and the confusions between LST and TB.

Table 3, P.18: “LST” is mentioned as observation but is it: LST, radiance or brightness

temperature in the [8-14] microns interval ? You should also indicate that it is a synthetic observation. I can assimilate LST or TB computed from a radiometer measurements. In my distributed version only LST observations are included. In the full SECHIBA-YAO version both measurements can be chosen.

P.4, L24: could you precise what is the type of the C3 crop for both sites and also give some details on the phenology or state of the plant development. As an example, LAI and canopy height could be added in Table 3 for PFT12. Vegetation in ORCHIDEE is characterized by using Plant Functional Type system of classification. Although PFT system describes to types of cultures (C3 and C4crop) it does not distinguish varieties of crops and only one crop type is currently active

Section 3: P.6, L.3, Eq.5: the cost function “f” should be replace with “J” in relation to Eq.4 Modification taken into account

P.6, L.7: I suppose that “y” should be replaced with “J”. I do not understand the reference to equation 2 which is the expression of the brightness temperature Reference to equation 2 misplaced. Modification taken into account

P.7, L.32: this empty line should be suppressed. Modification taken into account

P.8, L18-19: the sentence is unclear, please correct the syntax. The phrase will be replaced by: “When studying the subroutines, their complexity was reduced by breaking the different steps into simpler elements.”

P.8, L.32: “the second approach was used” I certainly miss something but you have not presented several approaches in this subsection. Misplaced reference: this sentence will be erased

Section 4: P.9, L.16: “The other parameters are multiplicative factors”. Why don’t you consider directly the parameters themselves: surface emissivity instead of k_{emis} , albedo instead of k_{albedo} , etc. ? Is it only due to a technical (or numerical) reason ? The idea is to have all parameters with the same value (all equal to 1) , in order to

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have directly the magnitude of the assimilation quality, and with the idea of having the possibility of comparing them

P.9, L.23: instead of “optimal value”, you certainly mean “initial value” ? What I meant is that prior to assimilation and to any perturbation, model parameters are always equal to 1

P.10, L.5-6 and Table 1 (P.16): the initial value of $m_{x_{eau}}$ (maximum water content) parameter is very low (150kg/m³). Why this choice ? This is the initial value generally used in sechiba before spinup.

What types of soil are considered? It is important to mention somewhere the soil description (classification or texture). Yes , you are true, the soil texture has been added in the text .

A low $m_{x_{eau}}$ value corresponds to dry or stressed surface conditions and will consequently increase the LST and overestimate it compared to in situ measurements. This remark is confirmed by the LE times series of figures 5&6 (see comments below) with quasi null absolute values. Is it done to increase the parameter sensitivity to LST in order to improve the results ? Yes , we agree, and this is the case in our experiments , we took dry conditions to be close to the initial value prescribed in Sechiba, but we could have chosen another value. This is at this stage only synthetic observations and twin experiments. The next step is the assimilation of actual observations which will be our future work.

P.10, L.26-29: in order to facilitate the interpretation of the results of Figure 4 and Table 2, you should precise earlier how the parameter sensitivity hierarchy is defined with both methodologies (finite differences and model gradients), i.e. based on the slope of the gradients. I didn't want to give much details on this because I think is out of the scope of the work: However I give a reference to my thesis (Benavides, 2014), where I give much details regarding this remark. However I clarified this point in the final manuscript

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P.11, L.12-18: you should homogenize your notations throughout the text, tables and figures (“true” = observation, “noise” = first guest or perturbed, “assim”= after assimilation) in order to clarify. Modification taken into account

P.12, section 4.4 “Results” and Tables 4 and 5: could you explain how a RMSD on LST reaching 5K is compatible with RMSD on surface fluxes lower than 2.5 W/m² for experiment 1? The same could be addressed for experiment 2 although RMSD on LST is lower and RMSD on LE higher (but even though relatively low in absolute value). Figures 5&6: times series of LE for bare soil and although for C3 crop have very low absolute values (less than 5W/m²). It is related to the low mxeau value (see previous comment) ? Are the synthetic observations times series realistic compared to real observations ? You should give more information on these points in order to argue your choices and to comment the physical behavior of the model. From a physical point of view, I am surprised by the fact that times series are similar for figures 5 (bare soil) and 6 (C3 crop). During the simulation period of 7 days, LST increases by about 10K meanwhile H flux decrease and LE flux stays quasi null how is it possible ? Times series of meteorological forcing and a description of the vegetation development should be helpful for the analysis. The experiments have been done in dry soil conditions , close to the initial value prescribed in Sechiba. We remind that we present here twin experiments, to present the tools developed and their potentialities. The dry soil conditions explain why there is not much difference between bare soil and C3crop with very low evapotranspiration rates. During this period, the ground heat flux increases and heat the soil, explaining the increase of the Surface temperature.

Section 5: P.13, L.1: “LST” should be replaced with “synthetic LST”. done

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/gmd-2016-64/gmd-2016-64-AC2-supplement.pdf>

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