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Geoscientific Model Development Discussions

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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The article addresses a 4D-VAR assimilation experiment of synthetic LST with YAO software in the SECHIBA module of the ORCHIDEE Land Surface Model developed at the "Institut Pierre Simon Laplace (IPSL)". The authors focus on the presentation of SECHIBA-YAO model and methodology with the stake to deliver an assimilation toolbox for end users. Twin experiments are conducted with synthetic data (for assimilation) in order to determine the most sensitive parameters of the model through 3 experiments on bare soil and C3 crop. Their study aims to demonstrate i) the ability of the assimilation methodology and ii) the potential of the LST in the retrieval of the 10 parameters and initial conditions to control leading consequently to the improvement

of surface fluxes and LST estimates.

General Comments

The paper is clearly organized and quite easy to follow. I have appreciated the global approach. Nevertheless, some precisions and complements are expected in the results interpretation. The authors should also precise which variable is really used for the assimilation: brightness temperature in the [8-14] microns interval, surface radiance or LST ? Why one or the other is finally chosen ? (See specific comments). The good results of the methodology have to be nuanced by the choice of the dataset used to manage the study and also by the fact that synthetic data are used (see specific comments below). I encourage the authors to carry on with their study with the difficult but interesting transition to the use of real remote sensed data. It is also expected that the authors take care to place their work among the different studies dealing with the control of LSM or SVAT models with surface temperature for water budget or surface fluxes estimates purposes.

Some Specific Comments

Section 1: P.2, L.31: could you precise what is a "specific deep land surface temperature" ? P3, L.1: "or" should be replace with "of" P3, L3: remove "available"

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.

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.

L.6, Eq.1: the Stefan Boltzmann constant [sigma] 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).

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.

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.

Section 3: P.6, L.3, Eq.5: the cost function "f" should be replace with "J" in relation to Eq.4 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. . .

P.7, L.32: this empty line should be suppressed.

P.8, L18-19: the sentence is unclear, please correct the syntax.

P.8, L.32: "the second approach was used". . . I certainly miss something but you have not presented several approaches in this subsection.

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 kemis, albedo instead of kalbedo,. . .etc. ? Is it only due to a technical (or numerical) reason ?

P.9, L.23: instead of "optimal value", you certainly mean "initial value" ?

P.10, L.5-6 and Table 1 (P.16): the initial value of mxeau (maximum water content) parameter is very low (150kg/m3). Why this choice ? What types of soil are considered ? It is important to mention somewhere the soil description (classification or texture). A low mxeau 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 ?

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.

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.

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.

Section 5: P.13, L.1: "LST" should be replaced with "synthetic LST".

Conclusion:

A correctly revised manuscript answering the questions and bringing the required precisions and corrections might certainly be of interest to the readership of GMD.

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