Dear Editor,

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

Thank you in advance,

Regards,

Hector Benavides

Many of your replies simply state "Go read my PhD thesis". Nobody will bother reading a whole PhD thesis to find particular information. You need to provide these clarifications. It is not appropriate in a scientific paper to refer to a PhD thesis as much as you do.

-Clear explanations of the different references to my PhD thesis are now presented in the manuscript and I no more refer to the thesis

Benoit Coudert asked for some further details on the properties of C3 Crops in ORCHIDEE, e.g., LAI, rooting depth, height etc. I think this information should be provided.

-Plant functional types are useful to distinguish the different soil type. In the present case we used the agricultural C3 grass type whose parameters are given in the text. They are:

$V_{cmax,\ opt}$ (optimal maximum rubisco-limited potential photosynthetic capacity)=90 mol/$m^{-2}s^{-1}$

T_{opt} (Optimum photosyntixc temperature)= 27,5+0,25Tl °C

Tl (Function of multiannual mean temperature for C3 grasses)

$_{max}$(maximum LAI beyond whitch there is no allocation of biomass to leave)=6

z_{root} (exponential depth scale for root length profile)= 0,25m

$_{leaf}$ (prescribed leaf albedo)=0,18

h (prescribed height of vegetation) =0,4m

Ac (Critical leaf senescence)=150 days

Ts (weekly temperature beyond which leaves are shed if seasonal temperature trend is negative)=10°C

Hs (weekly moisture stress beyond which leaves are shed)=0,2

I don't understand your reply to the comment about the multiplicative factors. How can you have all parameters (albedo, emissitivity etc) all equal to one? This does not make any sense to me. Were you referring to the multiplicative factor, rather than the actual parameter? If so, what's the point of having a multiplicative factor of one? I don't follow the logic here.

The parameters are divided into two groups: inner parameters and multiplying factors (Table 1). The first group corresponds to physical parameters. The second group collects parameters weighting some physical processes of SECHIBA. In the initial model, the weighting parameters have the value of one indicating that no weights are used, thus the effect of the assimilation is to allow a local adaptation of these weighting factors.

In order to compare the restitution of each parameter the physical parameters have been normalized. The controlled parameters are scaled by their prior values, so we control nondimentional parameters and a value of one indicates that the variable has been correctly reconstructed. Therefore the reconstructions of all the parameters can be compared.

In response to Abdelaziz Kallel, about the "Gradient Algorithm", "estimation of control parameters", and the third one, clarifications need to be made within the manuscript.

A paragraph about the gradient descent has been added in the text section 3.1
-For the gradient algorithm, a more depth explanation is proposed at the end of section 3.2
Finally, YAO includes routines devoted to classical assimilation scenario (incremental form) and is interfaced with the M1QN3 minimizer (Gilbert and Lemaréchal, 1989). As they metioned, the routine M1QN3 has been designed to minimize functions depending on a very large number of variables, no subject to constraints. The algorithm implements a quasi-Newton technique (L-BFGS) with a dynamically updated scalar or diagonal preconditioner. It uses line-search to enforce global convergence; more precisely, the step-size is determined by the Fletcher-Lemaréchal algorithm and realizes the Wolfe conditions.

For question 3
The assimilations used no background and a matrix R that is the identity. This was added in the text.
A new experiment has been run that add noise on the observations. For each experiment the conditions of the first guess are mentioned in the text.

in equation 4 , as we have no knowledge about the Matrix R we take the identity matrix indicating that no weight is used during the assimilation process. In the experiments y is just a scalar.

Page 2, paragraph starting with "Variable data assimilation" – This is a rather long paragraph, I suggest breaking it into two.
-Consideration taken into account

Page 12, section 4.4, lines 8 to 10 should be one paragraph.
-Modification taken into account

Your results section is very short. The paper does not have a discussion section at all??? You need to relate your work back to the rest of the literature. You have not done this at all in the paper, which I find very odd for a scientific paper.

-The result section, as well as the different experiments show in the paper were review in order to account for more variability in the experiment parameters : assimilation period, sites, noise added, paraemters, etc.

You state that there is little difference in H and LE because there was no precipitation during the simulation period. You therefore must show results during periods where there is high precipitation. A simulation period of one week is much too short. This must be extended.

-A more adapted results to the experiment are known shown

You state that your results can be explained by "The complexity of the model" – This is much too broad and general. I expect a discussion of the results to be much more in depth.

-consideration taken into account

It is critical that model evaluation covers a long enough period to sample seasonality, and a range of sites covering a large number of PFTs. You only show results over a one-week period, at one site, and simply refer to your PhD thesis for the Kruger site. This is not appropriate. This paper needs a lot more work.

-This aspect is worked on the new version of the manuscript. Other experiments are added and more information regarding the theoretical aspects of my thesis work are mentioned directly on the manuscript