

We thank the reviewer for his detailed reading of the paper and his helpful comments.

1. Page 10, line 16: The symbol " σ " is already used at line 1 of page 10. Therefore, this is slightly confusing.

We agree that this notation can bring confusion and we will change it in the revised version of the manuscript.

2. Page 12, line 20: The authors assume that the second (site-constrained) a priori estimation is the "real" probability distribution when estimating model uncertainty. However, for example, when predicting the productivity of sugar cane under future climatic conditions, what is "real" priori probability would depend on the assumption of the future scenarios. In other word, if we assume that all possible crop cultivars can be used anywhere in the world in a future scenario, the "pessimistic" priori probability maybe more "realistic". I think that the meaning of "real" in this paper should be defined.

We thank the reviewer for this very interesting comment about the significance of the different parameters prior ranges. The word "real" must indeed be used with caution when describing model analyses. Here we use the words 'real' and 'realistic' for the observations-constrained uncertainty analysis as a contrast to the simplest approach that is only based on literature search for parameters ranges. On page 13, lines 5 to 9 we tried to stress that our method is only an attempt at reducing the range of overestimated parameters and at providing a reduced range, somewhat limited to current growing conditions. We agree that this should be discussed in more details and we offer to:

- modify page 12 line 20: "The second (site-constrained) a priori estimation is a refinement of the uncertainty estimation based on the idea that a probability distribution of the parameters that is more realistic of the current growing conditions can be approached by the distribution of optimal parameters over all the current possible case studies (sites, weather, management)"
- add the following lines on page 13 after line 9:

"This observations-constrained range is highly dependent on growing conditions. When the model is applied to the context of climate change, these ranges may then be out of their domain of significance and the first wider estimate of prior parameters distribution, based on literature, must be preferred."

3. Page 14, line 7: A period is needed between "...Marivoet, 1990)" and "The larger...". This will be changed.

4. Page 14, line 12: Which spatial resolution was used, 0.5 or 0.7?

Indeed there was an inconsistency in the description of the resolution of the runs. We will fix it to a consistent 0.7° resolution.

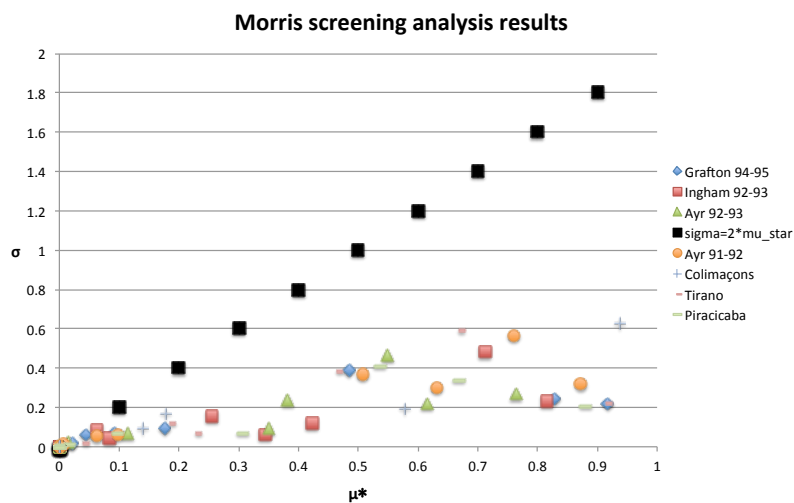
5. Page 15, line 13: In the screening analysis, the parameters that have large non-linearities are eliminated from the final parameter set, and the uncertainty analyses are conducted with the parameter set. This treatment would be reasonable when discussing about the parameter sensitivity. However, I cannot understand why it can be assumed that the parameters that have large non-linearities do not have large influence on the uncertainty of the model. I think that the authors should discuss the effect of eliminating the parameters that have large non-linearities on the evaluations of uncertainties of model output.

We thank the reviewer for noticing this confusing and partly inaccurate statement that must be clarified in the text. The $\sigma > 2\mu^*$ limit that we put for the parameters to be considered for ranking is a conservative limit that cannot be interpreted only as presence of non-linearities or interactions. We consider that parameters above the line $\sigma = 2\mu^*$ would actually reveal

something different about the model than what we are looking for here, i.e. a highly non-linear behavior of the model. Parameters below this line can also involve non-linearities (high σ) but are sensitive parameters that have some direct effect on the model output and which uncertainty might therefore be related to uncertainty in the output. In our results, as shown in the figure below, the parameters with significant values for μ^* are well below this line anyway (the highest ones are close to the $\sigma=1\mu^*$ line) and only parameters with a small mean elementary effect compared to the standard deviation of the elementary effects fall under this condition.

In the manuscript we propose to modify page 15 line 13:

“We first made sure that no parameter with a significant value for μ^* was above the line $\sigma=2\mu^*$ which would imply that non-linearities and/or interactions would be so strong that the uncertainty propagation from the parameter to the model output could not be clearly established. None of our parameters selected for their significant values for μ^* was above this line. We then ranked the parameters based on their μ^* index,...”.



6. Page 24, line 6-8: The relationship between PRCC and temperature explained here is different from that described in Fig. 11.

We apologize for this mistake; the description was attributed to the wrong parameter subplot. The parameter Tmin is the one that has high correlation indices with the simulated sugarcane biomass for low temperature pixels. This will be corrected in the revised version of the manuscript.