In this new version, the authors made important clarifications that have improved the article considerably. Nevertheless, there are a few other issues in this new version that require attention:

- The abstract and other parts of the manuscript still make major claims of priority. I understand that other studies in the past didn't work with CMIP6 and gridded observational products, but it still feels disrespectful regarding the work that other colleagues have done in the past. The abstract does not have the level of detail for a casual reader to understand what is really novel here and why this claim of originality.

The abstract has been rewritten with more details added

- The description of the data analysis in three different steps (section 2.5) is helpful, but it still lacks detail. For example, step 3 is still ambiguous regarding on how those medians were calculated. By pooling all the residuals of all models or of each individual model? The most appropriate way, which is followed by most authors in other analyses, is to present equations with the respective indices for gridcells and models. I recommend the authors to add the specific equations here, otherwise their description will still suffer from ambiguities.

We added equations detailing the calculation procedure.

- The new text on the moisture dependent functions is helpful, but there is still a problem in the interpretation of the model of Davidson et al. (2014). The authors seem to imply that this model cannot be implemented in ESMs because most of them are based on first order kinetics while Davidson's is based on Michaelis-Menten kinetics. This is a misunderstanding. Davidson's model is a function for a rate modifier, is not a system of differential equations with interacting substrates that would lead to nonlinear kinetics. The rate modifier function is expressed as what you would called Michaelis-Menten kinetics, but others would simply call a logistic function. It is only used to obtain a rate modifier, and it can be incorporated in ESMs as any other rate modifier. It is true that that oxygen and soil moisture dynamics must be modeled explicitly in the ESM, but that's different problem.

This part was removed and we only mention the oxygen limitation in the new version.

- The conclusion section makes the case for the improvement of models, but there's no word on improving the observation-based data products. Are we still assuming that they are perfect and we only need to improve the models? Your analysis showed that at least one data product may be biased at high temperatures. I think we still need to improve these products considerably.

We fully agree with this point but since we are not observation derived products producers it is more difficult for us to provide useful point on this aspect. Nevertheless we added some information in the conclusion.

- Table 1 is a welcome addition to the article. Can you add an extra row with the proportion of pixels within the median. The absolute number of pixels are difficult to grasp without an idea of the total number of pixels.

Table 1 was modified with the proportion instead of the total number of grid cells.

The new version, and in particular the new added text has a number of typos and small errors. Please read your manuscript more carefully before final submission. Some of the issues are:
- Ln 56, 'propose a way of improvement'?
- Ln 153, comma is missing? Consider rewriting.
- Ln 217, residues?
- Ln 271. 'constraint ... by constraining? Revise.
- Ln 313. 'mechanically'?

Our co-author Phil Martin who's a Native speaker checked the English of the last version of the manuscript.