

## **Summary**

This manuscript evaluates boreal summer season mean surface fluxes, soil moisture, land surface temperature (LST) and precipitation in two EC-Earth3 earth system model (ESM) simulations – one using the land-surface component only and one coupled to the atmospheric component. The objective of the work is to assess the quality of the land-surface component and to determine the impact of coupling on the simulation of surface climate.

## **Review**

In order to make the best use of ESMs for climate research, it is critical to understand how well they simulate the real Earth climate. The current work evaluates EC-Earth3 and the results would be informative for scientists looking to use that model to answer research questions. In particular the current work focuses on the impact of using the coupled configuration versus the land-only configuration on biases in the simulation of surface climate. This is an important consideration, as it is necessary to understand whether biases in the land-surface model propagate through the coupled model and how they may be enlarged or suppressed by the model representation of land-atmosphere interactions.

However for a paper with the title “the role of land-surface interactions for surface climate in the EC-Earth3 earth system model” and the aim of evaluating biases globally I find the usefulness of the current results limited due to the sole focus on boreal summer and the slim selection of observational datasets used for evaluating LST and precipitation.

Land-atmosphere coupling in the southern hemisphere is maximised in boreal winter, and I suspect the biases in surface climate will be also. An analysis for multiple seasons (at least June-August for the Northern hemisphere and December-February for the Southern hemisphere) is required. The analysis would also benefit from the inclusion of additional observational datasets including monthly mean satellite LST (infrared and microwave for comparison) and at least one additional precipitation dataset.

I would therefore recommend that the current manuscript be revised before being considered for publication. Please find my specific comments below.

## **Specific Comments**

### *Focus on boreal summer*

Line 300 is the first line in the main text that mentions the study focuses only on boreal summer (June – August). The study focus should be reflected in the abstract, introduction and possibly the title, with the justification provided at the start of the manuscript, not in the Discussion.

On that note, I am not convinced by the reasoning provided on Lines 587-593. If the objective is to understand the effect of atmospheric coupling on surface climate biases globally the work should consider the different seasons when the coupling is maximised in the different regions.

#### *Datasets – LST*

The author does not provide a justification for the exclusion of satellite LST products in the data section. Instead the author includes an inaccurate statement in the results section:

“A serious shortcoming of existing satellite-based data sets for land-surface temperature is that they depend on clear-sky conditions and, thus, do not incorporate periods with partly or fully cloudy conditions”.

This is actually only true for infrared satellite observations. Microwave satellite observations (for example AMSR2 skin temperature) do not depend on clear sky conditions. Furthermore the limitation of infrared satellite observations does not prevent a comparison when considering seasonal mean values. You could apply a mask to the MODIS Terra/Aqua monthly mean daytime LST products to exclude land points with less than a minimum number of valid observations (clear sky days) contributing to the value (for example 10 days in the season).

Therefore it is possible to include satellite LST products in the evaluation rather than rely solely on reanalysis.

One final query on this point, why did the authors use LST from ERA5 rather than ERA5-Land?

AMSR2/GCOM-W1 surface soil moisture (LPRM) L3 1 day 10 km x 10 km ascending V001 (contains microwave skin temperature)

DOI:10.5067/B0GHODHJLDA8

MODIS/Aqua Monthly mean Day-Time Land Surface Temperature at 1x1 degree V005

DOI:10.5067/2YCD3NSNDMRM

MODIS/Terra Monthly mean Day-Time Land Surface Temperature at 1x1 degree V005

DOI:10.5067/4SI45J6G6BW5

#### *Datasets – precipitation*

Were other datasets aside from GPCP considered? For example CHIRPS?

It would be more informative if more than one dataset were included for comparison.

The author needs to justify their choice of datasets in the data section.

<https://www.chc.ucsb.edu/data/chirps>

### *Abstract*

The abstract could be simplified to make it more clear and concise for the reader. For example I find this line from the Discussion makes the aims of the work immediately apparent:

“the intention is to assess a) the quality of the land-surface component and b) the effects of the coupling with the atmosphere”

Compared to the equivalent line from the abstract which is much longer and includes model details that could be reserved for later:

“The aim of this study is twofold, first to evaluate the quality of the simulation of surface climate by the land-surface component of the EC-Earth3 ESM, combining the HTESSEL land-surface model and the LPJ-GUESS dynamic vegetation model, and second to assess the role of the coupling of the land surface with the atmosphere for the simulation of the surface climate in EC-Earth3.”

Minor point on Line 21-22, I assume one of the instances of “underestimate” should in fact read “overestimate”?

### *Introduction*

In paragraph 2 the author states that EC-Earth3 represents a distinct step forwards, then proceeds to list the flaws with EC-Earth3. How do the biases in EC-Earth3 represent an improvement compared to the earlier version of EC-Earth?

There is a distinct lack of references in paragraphs 3-5 although many different processes are discussed. Here are a few examples that could be included in paragraph 3:

K. L. Findell, E. A. B. Eltahir, Atmospheric controls on soil moisture-boundary layer interactions. Part I: Framework development. *J. Hydrometeorol.* 4, 552–569 (2003)

R. A. Pielke, Influence of the spatial distribution of vegetation and soils on the prediction of cumulus convective rainfall. *Rev. Geophys.* 39, 151–177 (2001)

Bhowmick, M. and Parker, D.J. (2018) Analytical solution to a thermodynamic model for the sensitivity of afternoon deep convective initiation to the surface Bowen ratio. *Quarterly Journal of the Royal Meteorological Society*, **144**(716), 2216–2229. <https://doi.org/10.1002/qj.3340>.

Concerning paragraph 3, the concepts are a little disorganised. The paragraph is predominately discussing the surface impact on the atmosphere, yet ends with a sentence on

the atmospheric impact on the surface. Also the discussion on surface impacts on the atmosphere jumps between temperature and humidity/precipitation couplings rather than discussing each in turn.

### *Equations*

The flux equations, currently in the results section, should be included in the data section.