

## Reply to Reviewer 1, August 2023

Thank you for the detailed review and the suggestions to improve the manuscript! In the following I will respond to the comments in blue. Not all responses are full-fledged, but rather indicate how to implement the required changes in the revised manuscript. I will start revising the manuscript accordingly after the editor's decision to proceed further.

### General Comment

The manuscript entitled "The role of land-surface interactions for surface climate in the EC-Earth3 earth system model" by Wilhelm May evaluates two simulations against multiple reference data sets. For the first simulation, EC-Earth3 is forced offline with reanalysis data. For the second simulation, the land surface and atmosphere are dynamically coupled, but land surface characteristics are prescribed from the offline run. Comparing offline and constrained online simulations shows to what extent biases are related to the land surface component versus the coupling of the atmosphere. The first simulation has an overall warm and dry bias with some regional differences, a negative bias in net radiation and sensible heat flux, and a positive bias for latent heat flux. Dynamic coupling leads to stronger biases, and the sign of those biases can differ from the ones in the offline run. The study describes regional differences using multiple reference data sets in great detail. The author concludes that atmospheric coupling has a large impact on temperature, net radiation and sensible heat flux, and a modest impact on soil moisture and latent heat flux. The study concludes that further model development is required for reducing model biases.

The manuscript is very well written and I expect that the results are of much value to EC-Earth3 model developers. However, the manuscript also remains very descriptive. The Discussion section elaborates only on the study's limitations. It remains unclear how well the results compare to previous studies, the relevance of the findings for the broader land surface modelling community, and possible venues for model improvements. I, therefore, recommend that the manuscript may be considered for publication in ESD after major revision. Please find my detailed comments below.

I acknowledge the main concern that the study is very EC-Earth specific and that the results need to be discussed in relation to the aspects mentioned here. I am not sure about previous studies that have followed this approach, but I will check the scientific literature once more. Maybe I can learn something from overview papers on the models used for CMIP. I think I can include a discussion of the relevance of the results for both land-surface and atmospheric modelling and speculate about how the finding can support model development.

### Detailed Comments

L 7 The effect of the land surface on the global climate system is not limited to CO<sub>2</sub> alone. The ice and snow albedo feedback for instance affects the global climate as well. Please rewrite.

Yes. I will also check the Introduction for completeness.

L 65 The description of how changes in latent heat flux can potentially affect precipitation is vague, please rewrite.

Yes. My intention was to be short here. Will add more details.

L 76 Please add references.

Yes.

L 88 Typo, replace *ad* with *and*.

Correct.

L 176 Explain why you use prescribed data from the offline simulation in your coupled simulation.

In short, that is for eliminating the contributions of errors in the representation of the land surface to the biases induced by errors in the atmospheric model.

L 222 So your offline simulation is based on a spinup that was driven with GSWP3 and a historical run that uses ERA5 data? Why did you not use ERA5 data for both, the historical and the spinup? Please justify.

At the time when I performed the offline simulation, ERA5 was not yet available before 1979. That is why I had to use a “realistic” initial state of the land-surface component, particularly in the case of LPJ-GUESS, from an offline simulation with a meteorological forcing that went back earlier in time to begin with. To avoid the impacts of discrepancies between ERA5 and GSWP3, I first ran the offline model for 20 years forced with the climatological meteorological forcing from ERA5 ahead of the offline simulation with the varying forcing. I can add this reasoning to the text, but I am not sure how detailed it should be.

L 246 Replace *merged* with *spatially upscaled*.

Fine.

L 297 Please gather all results under a Results section.

I guess you refer to Figure 17 here, which I had added (in the Results section, actually) because I think the Taylor diagram is well suited to summarize (some of) the results of the study in a comprehensive way. I am not sure how to deal with the high number of figures (see also my response below), but this figure could be a candidate for omission.

L 307 Please add the value of the global mean bias in the text.

Yes, this improves the readability. Will also add other values from Table 2 to the text where relevant.

L 370 The equation applies to the upwelling, not downwelling LW radiation. Please adjust the direction of the arrow accordingly.

Yes, that is right, but probably not necessary anymore when omitting this part.

L 371 Add that ground heat flux is close to zero when averaged over longer periods, such as a month, which is why you may omit it in your equation.

Yes, but probably not necessary anymore when omitting this part.

L 358-377 This paragraph provides very basic information that you can expect the reader to know. I think you can skip this part.

I agree. I could mention the components that are included here (net radiation, sensible and latent heat flux, evaporative fraction) in Section 3.1 add that the relationship between net radiation and the energy fluxes.

L 402 Provide the definition of evaporative fraction further up where you first use the term.

Yes, I think I will add this to Section 3.1.

L 587 This paragraph explains why you focus on JJA. Please provide this justification earlier in the methods section.

Yes, I should have done that somewhere in the beginning. The other reviewer suggests that I consider the DJF season for the Southern Hemisphere, as the coupling between the land surface and the atmosphere there is stronger than for JJA. This justification would hold for both hemispheres.

L 570 The Discussion section only elaborates on the study's limitation. Please also elaborate on how well your results compare to previous studies, the relevance of your findings for the wider land surface modeling community, and possible venues for model improvements.

Yes, I can see that the study is very EC-Earth specific and that the results need to be discussed in relation to the aspects mentioned here. I am not sure about previous studies that have followed this approach, but I will check the scientific literature once more. Maybe I can learn something from overview papers on the models used for CMIP. I think I can include a discussion of the relevance of the results for both land-surface and atmospheric modelling and speculate about how the finding can support model development.

L 605 I would omit subsections in the Summary and conclusions section.

Yes, I can see this looks a bit awkward, given the shortness of the sub-sections.

Code and Data availability: You write that code and data can be made available by the author on request. I don't think this approach satisfies the ESD data policy, which states that data and other information underpinning the research findings are findable, accessible, interoperable, and reusable (FAIR) not only for humans but also for machines.

I need to check up on this. I had just looked at what had been done in other publications in Earth System Dynamics, where code and data were made available on request in some of them. The code (cdo and Fortan) used for the analysis, I think, is standard and it might not be necessary to make it available through a platform like github. The (monthly mean) data from the simulations, on the other hand, I might need to make available be on a suitable platform.

## Figures

- Figures 1a, 3a, 5a, 7a, and 9a: Please replace the divergent with a continuous color legend, as you are not showing the difference but the absolute values. Also, please ensure all legends include their units.

I presume, this comment includes Figures 11a and 13a as well. Yes, I can see the point (not so much for soil moisture and precipitation, though). But for the energy fluxes there are slightly negative values, which need to be treated somewhat differently. For the latter, I presume you refer to the evaporative fraction. In this case, the unit is standard unit, which I did not add to the legend.

- Figures that show seasonal biases: Please denote where biases are statistically significant.

I had a version of the maps indicating the statistical significance according to a Student's t-test. Given the size of the maps, I thought it was a bit hard to see. I will give it a try again (indicating either the significant or non-significant areas) and see how well it works. Alternatively, I could estimate the area of significant differences and add the numbers to table 2.

- Figure 15: Please add a map that identifies the different IPCC regions.

I could add the respective map from Iturbide et al. (2020) to the supplementary material, complementing Table S1.

- The text includes 17 Figures, of which not all are discussed in great detail. I recommend to remove figures that are not discussed in more detail, such that the total number of figures does not exceed ten.

I could omit Figure 17 (see above), and it might also be possible to omit Figures 15 and 16. The latter, because of the other reviewer's suggestion to consider the JJA season for the Northern and the DJF season for the Southern Hemisphere. If I do that, then Table S1 and Figure S12 will also be removed from the supplementary material.

Another way to reduce the number of figures could be to combine the maps with the histograms in one figure for each variable. Possibly (some of) the histograms could be omitted, where they are not really discussed. The maps, I think, I need to keep. In the same way, the number of figures in the supplementary material could be reduced, possibly removing the histograms.

Possibly, I could also omit Figure S3 from the supplementary material because of the small differences between the two versions of the GLEAM data and just mention it in the text.