Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-438-AC1, 2020

Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Multi-variable evaluation of land surface processes in forced and coupled modes reveals new error sources to the simulated water cycle in the IPSL climate model" *by* Hiroki Mizuochi et al.

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Received and published: 8 December 2020

(Our response follows each original comment from the reviewer.)

This is a well written paper reporting the multi-variable evaluation of land surface processes in ORCHIDEE land surface model (land component of the IPSL climate model) under different simulation modes (either forcing by WFDEI or coupled with LMDZ). The research mainly covered that in-depth evaluation of four interlinked essential climate variables (e.g., surface soil moisture, evapotranspiration, leaf area index,

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and albedo) using various reference datasets (ESA CCI, upscaled FLUXNET, GIMSS 3g and MODIS products), and factor analysis is also conducted by several factors (e.g.,plant functional type, leaf area index, irrigation, precipitation, slope, snow water equivalent, and evapotranspiration). I have several comments or suggestions for improvements and recommend publishing the paper after improving minor parts.

>Thank you very much for reviewing our manuscript. We are glad to receive your positive and constructive comments. Here we answer your comments one by one.

1) Line 120: there is specific information about the layer depth in the LSM. It is necessary to describe that information to notify which layer is defined as surface soil moisture.

>We used moisture in top 10 cm for ORCHIDEE SSM (Line 417). The information will be added in the revised manuscript.

2) Line 182: the comparison with both the forced and coupled simulations are conducted for 7 years (1993-1999: period 1). It would be clearer to compare the surface soil moisture against ESACCI and SMOS without dividing the period. If the forced simulation would be extended to 2014, that would be possible. The extending offline simulation seems to be possible because the essential datasets (e.g., ERA-Interim and monthly GPCC) used to force the land surface model are also available until 2014. Then, it seems that surface soil moisture is simply validated during 2010-2014.

>It is indeed conceivable to extend the offline simulation up to SMOS observation period using the latest WFDEI and GPCC datasets. However, extending the simulation also implies redoing all the evaluation work. It seems too laborious just to simplify the comparison scheme.

3) Figure S5: it seems that the definition of subperiod 1 should be corrected to “1993-1999”.

>We will correct the definition in the revision, thank you.

4) In many captions in main manuscript and supplementary, there is notation of “Tem-

porally averaged". Does it mean "annual averaged"? It is necessary to be clear the validation season for each variable.

>The "temporal average" was, if there is no annotation, taken during the entire evaluation period defined in Table 1. To clarify this, we will use "pluri-annual average" instead, in the revision.

5) There is no description about the statistically significant test. It is needed in Section 2.

>Both correlation coefficient and bias were tested by Student's t-test (also known test of no correlation and paired t-test, respectively) for each pixel. We will add the description in the revision.

6) Figure 3: this figure represents the temporal correlation coefficient with monthly time series. Based on the explanation of Figure 4, the temporal correlation of surface soil moisture over 60N-90N is calculated by only using JJA monthly dataset. If it is correct, such a description should be added in the text.

>Correlation coefficient was calculated using all available monthly time series for all seasons (not only JJA). The gray area in Figure 4 just shows less-reliable period and does not indicate data screening.

7) In several Supplementary Figures, the information of the evaluation period and season for each dataset and variable is missed. It should be clearly notated.

>We will add the description in the revision.

8) Line 199: there is inconsistence in the evaluation period between the manuscript (1987-2009) and Figure S2 (1987-2006).

>The description for Figure S1 (1987-2006) is incorrect, we will correct it to 1987-2009.

9) Lines 238-239: the impact of the chosen period would be marginal in terms of global mean aspect, but in the regional scale it is not likely to be limited.

>We will add this as a potential uncertainty or limitation of our analysis in Discussion.

10) Line 279: notation of “Figure 2E, F” is not matched to the description. It would be corrected to “Figure 2D, E”.

>We will correct it, thank you.

11) Lines 290-291: the sentence of “the coupled ET was negatively biased” is not matched to Figure 2E in which the ET bias seems to be positive. In other words, the maritime region shows the positive feedback between precipitation and ET such as the Congo.

>There is a hot-spot where ET is negatively biased in the maritime region (provided Fig. 1), although it is a bit difficult to see it in this resolution of the figure (the originally provided one has more fine resolution). But as you pointed out, the other part of the maritime region shows positive bias, and we will make the description more precise in the revision.

12) The positive bias of LAI is supported by the underestimation of LAI in the reference, but there is no discussion for the negative bias of the LAI. If the description is added, it would be helpful to understand the other aspect of the result.

>This comment refers to Lines 296-300, where it is true that the negative LAI biases are not discussed. Eastern Siberia is the main place with negative LAI biases, and we will mention possible explanations, which are consistent with Guimberteau et al. (2018): too persistent snowpack reducing the growth season, consistent with positive albedo biases in some parts of eastern Siberia; underestimated maximum LAI in the model; errors in the reference LAI product, especially at high latitudes, like for albedo. We will also send to the Discussion section, where some negative bias areas are already addressed, at Lines 353 (irrigated areas), 356 (interaction with negative ET and SSM bias), 394 (Himalayas in coupled mode), and 400 (Eastern Siberia).

13) If there is nothing of the description of the yearly variation, Figure 5 could be

changed to showing the seasonal cycle only (e.g., Figure 4).

>We will change the Figure 5 to show the seasonal cycle only.

14) Figure 8B, Figure 6 E, F: the result of LAI mean bias in the forced and the coupled simulations is totally same to each other against each class even though there is difference in the non-classified result (notated to “all”).

>We apologize that there were some errors in making the final figures (forced plots came to be the same as coupled ones accidentally). Actually, coupled plot tended to be lower than forced one (provided Fig.2), which is consistent with the difference in the "all" category. It does not change the conclusion because we had made discussion based on the right figures. We will correct the figure in the revised version.

15) In the comparison of surface soil moisture with ESACCI and SMOS reference datasets, these two datasets have spatially and temporally different missing point. For even validation, even if any dataset is missed among them, all data should be excluded. Does it do that?

>When different missing points were included in the different product during the same period, both data at the point were excluded in the analysis. We referred to it as "co-masking" in the manuscript (Line 231).

16) Line 414: the listed references for the SMAP product is not appropriate. Entekhabi et al. (2010) is the originated reference of the SMAP. Entekhabi, D., Njoku, E. G., O’Neill, P. E., Kellogg, K. H., Crow, W. T., Edelstein, W. N., C3 HESSD Interactive comment Printer-friendly version Discussion paper ... & Kimball, J. (2010). The soil moisture active passive (SMAP) mission. Proceedings of the IEEE, 98(5), 704-716.

>Thank you for the information. We will add your suggested reference, but also keep the two references as they are useful when using multiple SSM products.

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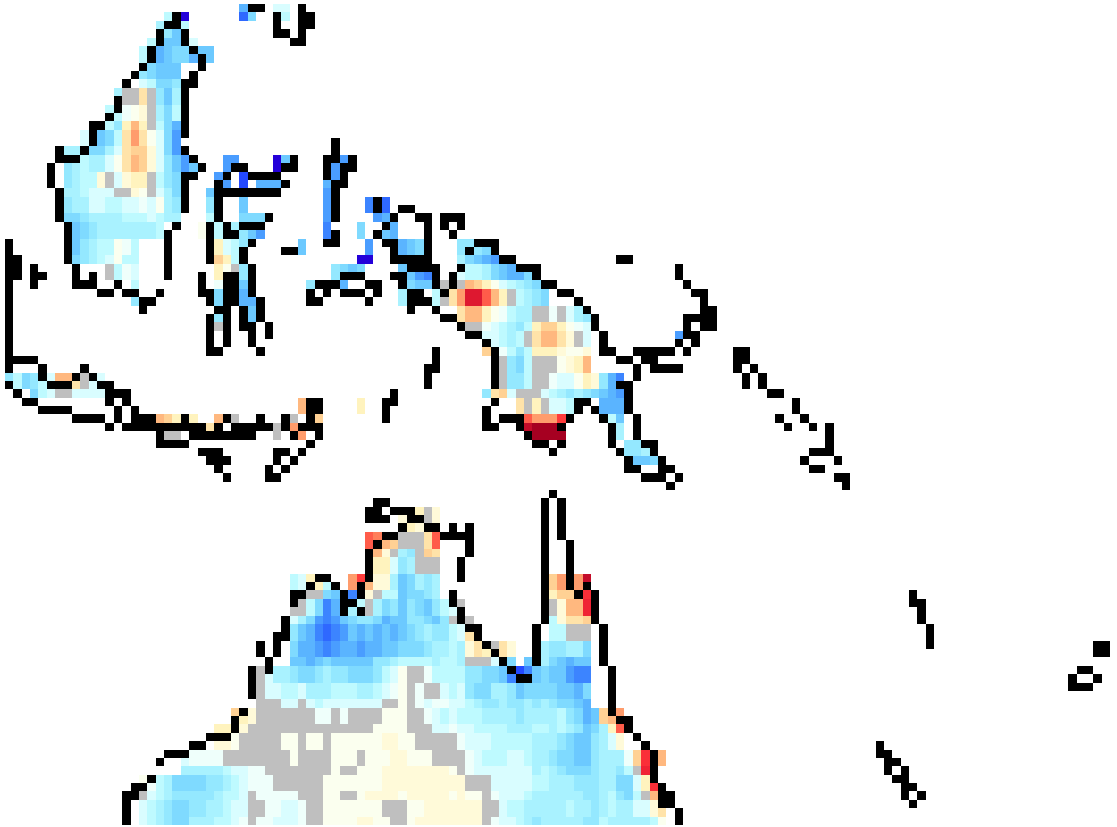

Fig. 1. Zoomed ET bias in the original Figure 2-E

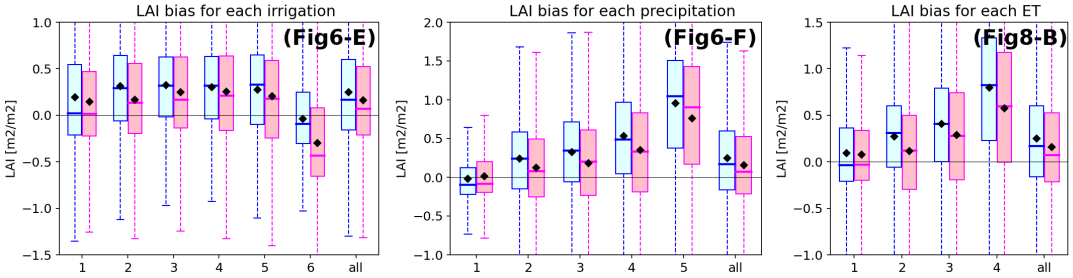

Fig. 2. Corrected LAI figures

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