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Hydrology and Earth System Sciences
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Discussions

# *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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Mizuochi and co-authors present a multivariate evaluation of the IPSL climate model forced with and without coupling to GCM at the global scale. Number of independent products are used to evaluate the performance of four Essential Climate Variables plus precipitation between the coupled and uncoupled modes and assess their differences. Although I am not an expert in the ORCHIDEE/LSM modelling, the manuscript is mostly clearly written, storyline is nicely motivated. Some rearrangements are recommended (see further), to better quantity different model configurations (factor assessment) and



their linkage. Overall, the presented topic is interesting and relevant to be the HESS readership. Note that the model simulations are not available for reviewer assessment, which does not comply with Copernicus guidelines, see https://www.hydrology-andearth-system-sciences.net/policies/datapolicy.htmlformoredetails.

>Thank you very much for reviewing our paper. We are glad to read your encouraging comments. Hereby we provide one-to-one reply as follows, and the manuscript will be revised based on your suggestions in the revision stage. As for the original monthly simulation data, it will be made accessible through the IPSL web platform, by both anonymous ftp and http/opendap protocols at:

> ftp.climserv.ipsl.polytechnique.fr/Data4papers/

> https://vesg.ipsl.upmc.fr/thredds/catalog/IPSLFS/datapapers

>IPSL will also issue a doi and the corresponding metadata will be documented through a catalogue (https://data.ipsl.fr/catalog/).

Main comments: (1) I have missed some extensive quantification and discussion, how much precipitation differs between WFDEI and LMDZ6A atmospheric GCM (period, annual, seasonal values) at the beginning of your analysis. There is panel C in Figure 2 introduces "coupled precipitation bias", but it is not defined, what is the references for this bias. Is it WFDEI or GPCC? It might be also helpful to introduce, how much this error is in terms of the relative annual change, to get better feeling for their differences. Which one of the two is closer to reality?

>"The coupled precipitation bias" is the pluri-annual mean difference (i.e. simulation minus reference) from 1987 to 2009 (Table 1) in the coupled simulation, using GPCC as reference. To clarify this, we propose to recognize precipitation as a fifth evaluated variable, and to provide the corresponding bias for both simulations forced and coupled (Figure 2, see new version below, and Tables 3, 4).

>It is noteworthy that the new Figure 2A shows a precipitation bias for the forced simula-

tion which is not everywhere negligible (+0.112 mm/d on average over land, compared to +0.186 mm:d for the coupled simulation). This contradicts our initial assumption, motivated by the fact that GPCC was also used for bias-correction of the WFDEI precipitation used to force ORCHIDEE (Line 153). The forced ORCHIDEE precipitation (WFDEI) is higher than GPCC in small tropical pockets, the US Great Plains, and boreal zones, which are prone to precipitation undercatch because of strong winds and/or a large fraction of snowfall (Becker et al., 2013). Schneider et al. (2014) acknowledge, for the GPCC product, that "the biggest uncertainty issue is the correction of the systematic gauge-measuring error (general undercatch of the true precipitation)", but this is very likely true for all precipitation products.

>The difference between the forced ORCHIDEE precipitation (WFDEI) and GPCC thus probably comes from their different methods to correct undercatch: based on Legates and Willmott (1990) for GPCC, and on Adam and Lettenmeier (2003) for WFDEI. Based on the literature, we are not able to assess which product is the most realistic, but we believe that a clear discussion of this issue will be useful to the HESS readers, in general and to better put in perspective the biases of the other variables. The above elements will therefore be inserted in the revised manuscript (in sections 2.1.2, 2.2.5, and 3.1) and lead also to slight changes in sections 3.2 and 4). For the last part of your comment, we did not quite understand the suggestion, but we hope the above-mentioned clarifications are enough.

>To finish with precipitation, we propose to specify in the revised manuscript that a few changes have been made in physics of the atmosphere between the version of the IPSL climate model used in this paper and the one used for CMIP6: "The only difference between the atmospheric physics used in this paper and the one used for CMIP6 concern the parameterization of deep and shallow convection and their interaction to improve the description of the intertropical convergence zone and the El Nino Southern Oscillation. The description of these differences and their impact on precipitation and other variables controlling the near surface climate can be found in Mignot et al.



(2021)."

(2) It is quite unusual that you introduce first three figures in the first paragraph of the Results section. Please, better explain the sequence of Figures and clearly explain, why you are showing all the figures. It is quite needed to make a better link between fig. 1 (depicting the factors) and following figures, especially the classes of Figures 6-8 need to be better linked. Current x-axes definitions of Fig. 6-8 is missing and need to be clearly linked to Table 2.

>We first introduce the pluri-annual mean maps for all the variables for reference products, forced and coupled simulations (in Supplementary), but the first paragraph announces that we rather focus on the bias maps which are more synthetic (Fig. 2). This paragraph also announces a summary table and a few maps of the correlation coefficient between simulated variables and reference products because the rest of Section 3.1 organizes the analysis by variable (precipitation, SSM, ET, LAI, and albedo) and not by type of performance criteria (bias and coefficient of correlation). We will also update the caption of Fig.1 to better explain all reference data (see the revised Figure at the end of this response). Some of them were used for validation (indicated as "+" symbols in the caption), the others were used for factor analysis (indicated as "x" symbols). Figs. 6-8 are the results of the factor analysis, along the x-axis classes defined in Table 2. This point is mentioned at the end of the captions. For example, Fig.6a (irrigation panel for SSM bias) represents distribution of SSM bias patterns for each irrigation class, defined in Table 2: from small (0%; class 1) to large (50-100%; class 6) fractions of irrigated area.

(3) There is positive bias for the coupled ET (see Figure 2E) in Amazonas, while the coupled precipitation bias (Figure 2C) shows large precipitation underestimation for the very same region. I don't understand, where this counter-intuitive behavior comes from. Please clarify.

>We have already commented this counter-intuitive result in the submitted manuscript

(Lines 288-292 - Results, 369-370 - Discussion). The suggested explanation involves a negative feedback in the region between precipitation and ET because the area is strongly energy-limited (Seneviratne et al., 2010; McVicar et al., 2012). As a result, excessive precipitation does not induce excessive ET as ET is not water limited, but the related excessive cloudiness drives a deficit of incoming solar radiation, which likely explains the negative ET bias.

(4) Figure 2 is already quite heavy, but it is really hard to spot the difference between left and right column. Would be nice to, say, provide the relative change between the forced and coupled runs.

>We provide below the figures showing difference between forced and coupled modes (Revised supplementary fig. S6). However, the original figures are also useful to indicate errors in each mode, our paper is organized to use them (see above, point 3) and we would like to keep them too. We will thus provide the difference panels in the supplementary, and announce it in the introductory paragraph of section 3.1.

Minor/Technical: Remove reference in preparation

>We will remove it from the revised manuscript.

Line 264: spell out CC is correlation coefficient (Pearson I guess?)

>As you guessed, it is Pearson's correlation coefficient. We will mention it in the revised manuscript.

Fig. 1D, not clear, why authors use do you use the ORCHIDEE variable in this reference overview figure and not an independent data source.

>Fig 1D shows the mean snow water equivalent simulated by ORCHIDEE offline. It is not used as a reference for validation, but for the factor analysis. In other words, it was simply used to separate less snowy area from more snowy area in the additional analysis after the fundamental validation. Independent data source is essentially needed for the validation, but we assumed it is not necessarily required in such a classification



purpose. We will add the explanation in the revised manuscript. We will also clarify the caption of Fig. 1, using symbols to distinguish the variables that are used for validation (+) and/or factor analysis (x).

Fig. 1G, link types to Table 1.

>The reviewer probably meant to link with the PFT classes of Table 2, and it is a very good suggestion. We will indeed add in the caption of Fig. 1 that the PFT classes are defined in Table 2.

Figure 3: I suggest, replace gray by white and then green by gray. Green takes too much attention, although that's non-significant pixels.

>Thank you for this useful suggestion. We changed the colors in the revised Fig. 4 below, and we will use thus new figure in the revised manuscript.

>Cited references:

>Adam, J. C., and Lettenmaier, D. P.: Adjustment of global gridded precipitation for systematic bias. J. Geophys. Res., 108, 4257, doi:10.1029/2002JD002499, 2003.

>Becker, A., Finger, P., Meyer-Christoffer, A., Rudolf, B., Schamm, K., Schneider, U., and Ziese, M.: A description of the global land-surface precipitation data products of the Global Precipitation Climatology Centre with sample applications including centennial (trend) analysis from 1901–present, Earth Syst. Sci. Data, 5, 71–99, https://doi.org/10.5194/essd-5-71-2013, 2013.

>Legates, D.R., and Willmott, C.J.: Mean seasonal and spatial variability in gauge-corrected, global precipitation. Int. J. Climatol. 10, 111–127, http://dx.doi.org/10.1002/joc.3370100202, 1990.

>Mignot, J., Hourdin, F., Deshayes, J., Boucher, O., Gastineau, G., Musat, I., Vancoppenolle, M., Servonat, J., Caubel, A., Cheruy, F., Denvil, S., Dufresne, J.-L., Ethe, C., Fairhead, L., Foujols, M.-A., Grandpeix, J.-Y., Levavasseur, G., Marti, O., Menary, M.,

Rio, C., Rousset, C.: The tuning strategy of IPSL-CM6A-LR, JAMES, revision submitted.

>Schneider, U., Becker, A., Finger, P., Meyer-Christoffer, A., Ziese, M., and Rudolf, B.: GPCC's new land surface precipitation climatology based on quality-controlled in situ data and its role in quantifying the global water cycle. Theoretical and Applied Climatology, 115(1–2), 15–40, https://doi.org/10.1007/s00704-013-0860-x, 2014.

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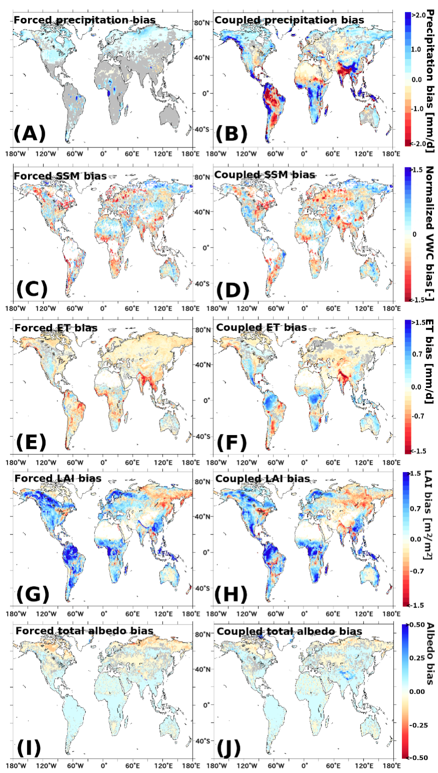


**Fig. 1.** Revised Figure 2: Temporally-averaged spatial patterns of bias (i.e., simulated values minus observed values) for the five evaluated variables, simulated in forced mode (left) and coupled mode (right)

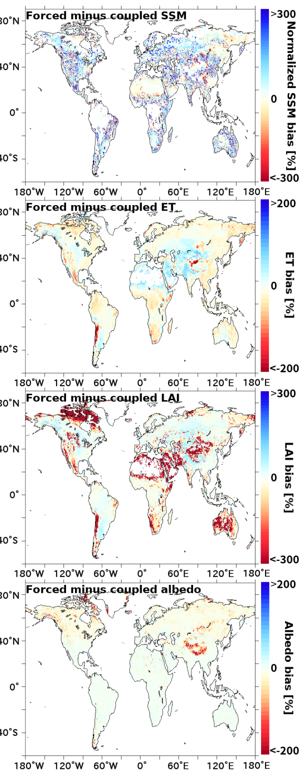

**Fig. 2.** Revised supplementary figure S6. Pluri-annual mean relative differences between forced and coupled modes for normalized SSM, ET, LAI, and albedo, in % of the forced value



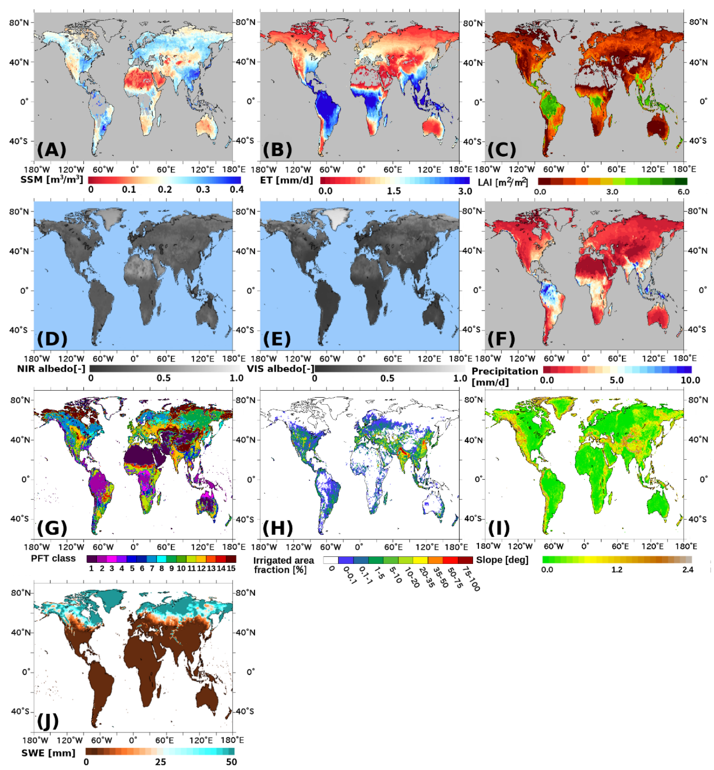

**Fig. 3.** Revised Figure 1: Spatial patterns of pluri-annual mean reference data used for validation (marked with a "+") and/or factor analysis (marked with a "x")

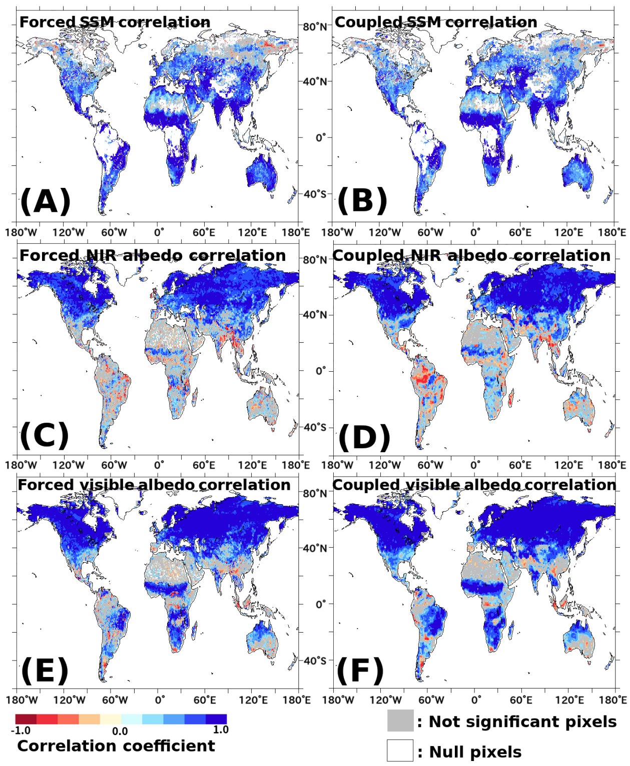


**Fig. 4.** Revised Figure 4. Color-changed version of the original Fig. 3.