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Earth System Dynamics
Discussions

Interactive comment on "A bias-corrected CMIP5 dataset for Africa using CDF-t method. A contribution to agricultural impact studies" *by* Adjoua Moise Famien et al.

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We thank the reviewer for the constructive comments and useful suggestions. Below we answer the different comments of the reviewer. We present all reviewer comments and our answers are given in blue.

General comments:

The authors present a dataset of CMIP5 GCM output of daily accumulated precipitation, daily mean, minimum and maximum near-surface air temperature, daily

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Discussion paper

mean surface downwelling shortwave and daily mean wind speed bias-corrected using the CDFt method and the global gridded observational dataset WFDEI over Western Africa. They compare the raw and the bias-corrected data to WFDEI and 2 other observational datasets, namely WFD and EWEMBI. Unsurprisingly, the bias-corrected data feature much smaller biases relative to WFDEI than the raw data. Specifically, the authors look into biases of seasonal mean values and mean annual cycles of temperature, precipitation and radiation, the 95th percentile of temperature and precipitation, and the number of days on which temperature or precipitation are above or below certain threshold values. The analysis is confined to the five ISIMIP GCMs as this enables a comparison to the same CMIP5 GCM output bias-corrected using the ISIMIP method and the dataset WFD as it was done in ISIMIP. Lastly, a crop model is driven by the different climate input datasets and maize yields are evaluated in terms of both how well they reproduce historical observations and how they are projected to change under one RCP scenario.

The writing style of the manuscript is sloppy and the English is not good. They manuscript contains many figures, which on average have a rather low information content. The discussion of these figures in the text is mostly not very insightful. Comparisons of WFD, WFDEI and EWEMBI data are in many cases unnecessary since it is clear from the definition of these datasets that they are very similar or even identical in many cases. A table that summarizes the differences and commonalities of the datasets for the considered variables should be included in order to make this more transparent to the reader.

Ok. A table summarizing the scores (Correlation, Standard deviation and RMSE) has been added.

The manuscript does not add anything to the existing knowledge on bias correction. New findings are only present in parts of the comparison of the different datasets

as well as in the sensitivity analysis of maize yields simulated using these different datasets as input. However, since the latter analysis is done for one crop model in combination with one GCM only, general conclusions cannot be drawnfrom it. A comparison of its results to corresponding ISIMIP or AgMIP studies might help put the results of this study into context.

Our main objectives of this manuscript are (i) to introduce a new bias-corrected dataset over Africa whose the CDF-t correction method has been applied for the first time, (ii) to quantify the effect of using different reference datasets on the corrected data, (iii) and to illustrate this effect on crop simulations over West Africa. Comparison of these results to corresponding ISPMIP or AgMIP studies is out of the scope of our work, and we do not intend to provide general conclusions regarding crop simulations forced by an ensemble of bias-corrected GCMs.

In addition to that, the manuscript suffers from a few methodological flaws, see my specific comments below. Overall, I think that a major revision of the manuscript is needed before it can be published in ESD.

Specific comments:

P2 L6: What are “robust biases“? And what is a “bias“ in the context of this paper? I think you should write one or two sentences about that.

Robust biases means biases that have not been reduced up to now, for instance between CMIP3 and CMIP5 GCM simulations. For instance warmer than normal SSTs in the equatorial Atlantic leads to a too southern location of the ITCZ in boreal summer over West Africa. A sentence has been added.

P2 L8: “statistical bias-corrections are necessary [...]“ – Why? Please explain

why you cannot just use the original GCM output as input to your impact models/what would happen if you did so.

We add an explanation: "For instance the too southern ITCZ location in boreal summer over West Africa in most of the GCMs leads to too weak precipitation over the Sahel and to too weak crop yields whose values cannot be used as relevant information for stakeholders and farmers"

P2 L17: "on 4 out of the 5 same CMIP5 GCMs" is not quite correct. Only 3 out of the 4 GCMs chosen in ISIMIP2b were also used in the first phase of ISIMIP.

This has been corrected.

P2 L22: Which kind of biases have been identified over Central, East and South Africa? Climate models have always been and will always be biased, so merely saying that a model is biased is an empty statement.

This has been completed.

P3 L24: Why do you not present results for northern winter and autumn?

This is due to space limit. We have chosen to focus on West Africa as it is the domain concerned in the AMMA-2050 project, and hence to the corresponding boreal summer monsoon season.

P5 L1: How did you interpolate the other variables?

All the variables are indicated here.

P5 L13: Which WFDEI version do you use? The one with precipitation corrected using CRU (WFDEI-CRU) or GPCC (WFDEI-GPCC) estimates? Please specify.

WFDEI-GPCCv5/v6; this has been indicated in the text.

P5 L22: You should inform the reader here that over land, EWEMBI is identical to WFDEI-GPCC for precipitation, daily mean, minimum and maximum near-surface air temperature and 10 m wind speed. Only for surface downwelling shortwave radiation there is a difference between EWEMBI and WFDEI-GPCC data over land.

Ok, this has been added.

P7 L4: Which SRB data exactly do you use for this comparison. Please describe that here or in Sect. 2.2.

SRB release 3.0; this has been added.

Sections 3.1 and 3.2: This is an insufficient description of the CDF-t method. Merely referring the reader to Michelangeli et al. (2009), Déqué (2007) and Vrac et al., (2012, 2016) for all the details is not enough. Please be more specific about how you use Fobs,fut to do the quantile mapping in the future period (I assume you map x to F −1 obs,fut(Fmod,fut(x))) and please describe how you estimate Fobs,cal, Fmod,cal and Fmod,fut. Are these CDFs estimated parametrically or non-parametrically/empirically and how exactly do you do the estimation? Also I do not understand how you account for seasonality: Do you apply CDF-t month by month or using moving windows? Moreover, you state that “CDF-t preserves any long-term trend in the GCMs data“ but do not give any reference that would corroborate that statement. Thinking about Eq. (1), I came to the conclusion thatCDF-t does neither preserve trends in moments nor in quantiles. Please discuss this aspect in moredetail

since the users of your data product will want to know if and how you have modified the trends present in the original CMIP5 GCM data. Lastly, you state that "GCMs data have been interpolated to WFDEI grid before being bias-corrected." Which method do you use for that interpolation?

A more detailed description of the method has been provided with more specific statements and equations. These CDFs are estimated non parametrically. Seasonality has been taken into account by performing month-by-month corrections. Regarding CDF-t preservation of the long-term trend in GCM data, a reference has been added, as well as a comment stating that CDF-t does neither preserve trends in moments nor in quantiles. The interpolation methods have been indicated too.

P8 L3: Not "every GCM has to be calibrated" but the bias-correction method has to be calibrated individually for every GCM.

Ok, this has been corrected.

Sect. 3.3 and Figure S2: I do not understand why you did what you did here. Let's take calibration period 1997–2013 as an example. Did you use 1997–2013 GCM and observational data to calibrate the CDF-t method and then apply the thus calibrated method to the same 1997–2013 GCM data? If that is what you did then I do not understand the purpose of these tests since in that case of course the remaining biases will be small and you cannot draw any conclusion in terms of overfitting. Therefore, also the concluding statement of the section would be nonsense. What you need to do to test for overfitting is a cross-validation.

You are right. This conclusion about over-fitting has been removed from the text. We choose to keep the longest period to perform the calibration period. As Referee 1 says, it is not essential to go further regarding this part.

P9 L2f: What you describe here is not what is shown in Fig. 2.

This has been corrected.

Figure 3: "Taylor diagrams relative to the mean of surface temperature" does not make sense. Please rephrase. I assume that all panels in the upper row refer to the Sahel box and all panels in the lower row to the Guinean box, correct? Please specify this in the caption. Also, please add a (separate) figure with a map showing the definition of the SAHEL, GUICOAST and any other region used in your study. Lastly, your Taylor diagrams suggest that 1979–2001 JAS mean EWEMBI and WFDEI tas have different spatial standard deviations despite EWEMBI and WFDEI tas being identical by defi- nition. Can you explain this? Have you maybe used a WFDEI data product version that is different from the one used for the production of EWEMBI? The same question applies to Figures 5, 8, 9, 12, 14, 16.

The caption has been corrected and SAHEL-GUICOAST boxes have been superimposed on Figures 1, 18 and 19.
We used the same version of dataset for WFDEI and EWEMBI. All relevant figures have been modified.

P18 L1: "daily values" of which variable?

This has been corrected.

P18 L9f: "CDF-t method is also a bit better than ISIMIP one when one refers to EWEMBI reference data" – of course, because the ISIMIP data were bias-corrected using WFD, your data were bias-corrected using WFDEI data, and EWEMBI is mostly identical to WFDEI (see my other comment above). There is no real point you are

making here or elsewhere, where you have made the same statement. Basically, you could leave out the right panels in all Taylor diagram figures related to temperature and precipitation since there is no qualitative difference to the respective middle panel.

We prefer to keep these diagrams because it enables to show more directly the proximity of the WFD and WFDEI bias-corrected data to EWEMBI than when WFDEI is used as the reference in the Taylor diagrams.

P20 L6f: Strange sentence. Please rephrase.

It has been corrected.

Figure 18: Maps of difference to WFDEI would be good here.

We added the map of the difference between EWEMBI and WFDEI associated crop yields.

Figure 19: Maps of difference between, say 1970–2000 and 2070–2100 mean values would be good here. For which RCP is this? Do you get qualitatively similar results for the other four ISIMIP GCMs? I mean, why do you do all this work and then just show results for one GCM. . .?

The map of difference has been added. This is RCP8.5 scenario (it has been indicated in the text and the caption). We have not run the simulations with the other ISMIP GCMs because it is out of the scope of our work.We chose to focus on sensitivity experiments carried out on one GCM. We do not intend to provide general conclusions regarding crop simulations forced by an ensemble of bias-corrected GCMs.

Table 2 and its description in text: Why this selection of sensitivity experiments? Why, for instance, do you do the WFDEIWFDrsds experiment but no WFDEIWFDpr experiment even though the WFDEIpr and WFDEIrsds experiments suggest a higher sensitity of yields to pr than to rsds biases?

We have chosen to show the sensitivity to (i) first the reference data sets (WDF, WFDEI, EWEMBI), (ii) then to the IPSL GCM non-corrected data and data corrected with WFD and WFDEI reference dataset, (iii) to one out of the WFDEI variables by replacing it by the corresponding raw IPSL-CM5A-LR data, (iv) at last to rsds from WFD instead of WFDEI. It is a rather extensive set of simulations. We performed the last sensitivity experiment because the difference in rsds between WFD and WFDEI is quite high (see Fig.1g-h-j-k) while the difference in pr is quite weak (see Fig.1d-e).

P25 L2: "It indicates also that WFD data and related bias-corrected simulations should not be used anymore" – I think your analysis as it currently stands is not sufficient to come to this conclusion because (i) you are comparing potential simulated yields to actual observed yields and (ii) you did not say anything about the quality of the GDHY data. Nevertheless I think that you are right in terms of rsds as Weedon et al. (2014, doi:10.1002/2014WR015638) report substantial improvements of rsds in WFDEI relative to WFD.

You are right. This sentence has been removed.

P25 L11ff: "Interannual variability of simulated yields is proportional to the mean with a very weak variability for ISIMIP yield and higher variability for CDF-t and raw simulations. All projections show a clear decrease of maize yields by a factor of 2 over all of West Africa along the 21 st century." – I think you should also show that visually by plotting yields relative to mean 1950–1980 levels, for example.

These plots have been added.

Technical corrections:

P2 L10: I think it would be better to write “in combination with“ in place of “based on“.

This has been corrected.

P27 L7ff: To be more precise, your list of variables should read “daily accumulated precipitation, daily mean, minimum and maximum near-surface air temperature, daily mean surface downwelling shortwave and daily mean wind speed” here and elsewhere in the manuscript.

This has been corrected.

All corrections will be applied in the revised manuscript.

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