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Earth System Dynamics
Discussions

Interactive comment on "Bias correction of surface downwelling longwave and shortwave radiation for the EWEMBI dataset" *by* Stefan Lange

Anonymous Referee #1

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The paper compares a large set of methods aimed at correcting, or better saying adjusting, the bias of recently developed EarthH2Observe (E2OBS) shortwave (SW) and longwave (LW) downward surface radiation (rsds and rlds, respectively). These are included in the EWEMBI meteorological forcing dataset for the ISIMIP Intercomparison Project. Bias correction is implemented by comparison with the Surface Radiation Budget (SRB) satellite observational dataset. Given the different spatial resolution of the E2OBS and SRB datasets, an additional downscaling/upscaling is performed in addition to the bias correction itself. The latter is performed by means of quantile mapping, a parametric procedure depending on the parameters that define the distribution adopted for reproducing the statistics of data (i.e. mean values, variance and upper

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Discussion paper

bound). Disparate methods are thoroughly compared and discussed, varying in the distribution type, time resolution and the way they handle the spatial resolution gap between E2OBS and SRB datasets. An independent validation against station-based observational measurements from the Baseline Surface Radiation Network (BSRN) is also performed.

1 GENERAL COMMENTS

Performing bias correction in combination with downscaling/upscaling of model outputs at observational-based datasets resolution is a well-known critical issue when postprocessing model outputs, and this is well taken into account as background to the analysis. This clearly motivates the careful choice of the variants of the methods, and the thorough discussion of their implications. To me this is a strength and also a limit of this manuscript, since the methodology is constrained by the choice of the model and observational-based assets. Nevertheless such a work provides a necessary reference for further usage of the EWEMBI dataset. For this reason, I would recommend the publication of the manuscript, provided that some minor revisions are provided.
My main concern is that the author provides some improvement to the description of results, particularly in terms of figures. I am aware that comparison among 8*3 methods, adopting different parameters over LW and SW radiation fields separately, requires a challenging effort in terms of clarity and conciseness. In some parts of the manuscript I found difficult to benchmark arguments described in the text with the mentioned figures. I will be more specific in the next section.
Another aspect that I think might be improved is a discussion of the implications of using a deterministic parametric method, rather than a stochastic one, for bias correction when a downscaling/upscaling is made necessary. A reference to Maraun, 2013 (JCLI) might be helpful in this respect. Related to this, a further appendix may be suitable, not only including such a discussion but also a basic description of the quantile mapping

methodology for those who are not familiar with it. In the current draft, this is left to references although, as far as I could check, none of the mentioned papers explicitly addresses for the quantile mapping methodology.

2 SPECIFIC COMMENTS

- **Figure 2:** it was very difficult to me to distinguish among the various lines shown in the panels. The dotted red and dashed blue lines are almost undistinguishable (particularly in (b) and (c)) and the light blue line in (a) can hardly be seen. I would suggest to split this figure in two, separately showing the beta and advanced distributions respectively, with the related parameters. As for the caption, I would suggest to explain in first place on which data the computation of the distributions and their parameters is based.
- **Table 2:** I wonder if one could improve the notation for distribution parameters and arrange it with a more mathematically appropriate symbols. Rather than plain text and footnotes, you may want to introduce a consistent notation with brackets and apostrophes to indicate means, running means and variances, as well as apexes and subscripts referring to the length of the window and the amount of years to be considered.
- **l. 32-33, p. 9:** it may be worth mentioning here how the common factor for the aggregation of bias-corrected values in the SRB-grid cell is chosen.
- **l. 22, p. 10:** As far as I understood the common factor f(i,j) is not the same as for the aggregation to the SRB-grid cell, given that it depends on whether the bias correction is applied on the lower or higher resolution. If it is not the case, it is once again not clear to me how the value of this common factor is chosen (see previous comment).

- **l. 33-34 p. 12:** the limits of parametric methods are here correctly mentioned. As stated in the General Comments section, this is a critical issue, and I think it would be worthwhile a few more arguments. If it is not too much work, I wonder if it would be possible to apply a non-parametric quantile mapping (e.g. using a cubic spline empirical CDF) to be compared with these parametric methods.

- **l. 15-16 p. 14:** looking at Figure 6 is very hardly distinguishable that the BCvmp1 at the daily time scale outperforms the same methods at the monthly time scale. This is in my opinion because Figure 6, as well as Figure 2, contains too much information that prevents from emphasizing the key points that are described in the text. The uncertainty range masks the differences among the bars. Furthermore, having five bars for every months makes very difficult to distinguish them, particularly the ones in lighter colours (BCvmp1 methods). I would suggest to split the figures in order at least to separately consider original and bias corrected p-values.

- **l. 9-11 p. 16 and l. 1-2 p. 18:** I found very challenging to carve out the important information from Figures 7-8 and link it with the arguments in the text. It seems to me that the only clear information that can be driven from them is that BCvdax methods outrank BCvdax at the daily resolution for what concerns rlds, and the other way round for what concerns rsds and rlds in the monthly mean. The author refers to a tropical/extratropical asymmetry that to my best effort is barely distinguishable. Furthermore the seasonal dependence (if any) is not mentioned in the text, still making the clarity of the two figures even more arguable. I would suggest either to restructure the layout of Figures 7 and 8 or removing this part, since it does not add much to the discussion of results.

3 TECHNICAL COMMENTS

- **l. 6 p. 7 (and elsewhere in the text):** replace “Sect.” with “Appendix”, when you reference to appendices.
- **l. 5 p. 9:** correct “it”.
- **l. 8 p. 10:** maybe “be” is needed between “to” and “made”.
- **l. 10 p. 14:** “that” is repeated twice.

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