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Interactive comment on "Using ERA-Interim Reanalysis output for creating datasets of energy-relevant climate variables" *by* Philip D. Jones et al.

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General comments

(Please note that I have not read *RC1* before submitting my review to maintain independence.)

The authors describe how they obtained a bias-adjusted dataset for energy-sector relevant parameters from the ERA-Interim reanalysis. I have a substantial number of remarks. Overall, I am not really convinced that the bias-adjustment as conducted here is a significant step forward regarding the quality of the data compared to the unadjusted data. However, I certainly think that the data set deserves being published

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and documented through this paper – if certain points are clarified and the quality of the results are discussed more extensively and critically. This pertains in particular to the required independence between the data used for bias adjustment and the data used to assess the pre- and post-adjustment quality. I therefore recommend to accept the paper subject to major revisions.

My main points of criticism include:

1. If I am not mistaken, only in one case – for surface solar irradiance – an independent data set is used to assess the effects of the bias-adjustment in a conclusive way. In all other cases it appears that the data used for the adjustment are then used to assess the post-adjustment "bias" – which is not exactly zero only because of certain interpolation/remapping steps. If I am wrong here, please explain your approach in a way that prevents other readers from repeating my misunderstanding.

2. It is also unclear to what extent the station and derived gridded data are independent of data that have entered the assimilation system used to generate the reanalysis in the first place, and what the impact of interdependence might be (e.g., for near-surface temperature).

3. It is stated that the figures showing the statistical data distributions (Figs. 2, 3, 6, 7, ...) *exhibit the [corresponding] distribution fits*. I think that this is not correct, but that the figures show the empirical data densities obtained by some smoothing, e.g., with a Gaussian filter. As a consequence, also those parameters that are positive by definition (wind speed and precipitation) exhibit non-zero densities at negative values (where the theoretical distribution fits should have zero density). This should be corrected in the text and also explained in the figure captions. Also, I suggest to add the two corresponding theoretical distributions to each of the plot panels (e.g., as dashed curves), so one can judge how well the empirical distributions are approximated by the theoretical distributions.

Specific comments

Abstract: I suggest to mention in the abstract that univariate adjustment is applied. (To my understanding, multivariate approaches are mentioned in the paper several times, but were not applied to generate the present data set.)

P1 L17–18 – The benefit of performing bias-adjustment is demonstrated by comparing initial and bias-adjusted ERA-Interim data against observations: See general comment 1.

P1 L22–23 – These are reconstructions of past climates produced through the blending of observations with physical/numerical models which have been developed explicitly for climate monitoring and research: Speaking of the use of reanalyses for climate monitoring (which in my view implies the consideration of long-term trends), I recommend to add a comment of caution regarding possible spurious trends that can arise from changes in the observing system (e.g., Bengtsson et al. 2004 *Can climate trends be calculated from reanalysis data?*).

P2 L5 – climate model: I think this should rather read *weather (forecast) model*, or maybe less specifically *numerical model*.

P2 L5 – amount of observational data: Could be complemented to *amount, type, and quality of observational data*.

P2 L12–13 – the bias may be larger for [...] regions of sparse station coverage: If I understand correctly, this statement holds only if this refers to those stations from which the data enter the assimilation system. In this context a clear distinction should be made between assimilation-related stations and stations used for the bias adjustment. There is probably strong overlap, but I doubt that the two sets of stations are identical (not speaking of the various other data types that enter the assimilation system).

P2 L31–32 – The benefit of performing bias-adjustment is demonstrated by comparing initial and bias-adjusted data against station observations and gridded observation

products: See the general comment regarding independence of the data used to assess the bias-adjusted data.

Section 2: I would find it very helpful if a table that gives an overview over all the used data sets could be provided, stating if they are station data or gridded (and on what grids, originally and after interpolation), what their time resolution and period is, what they are used for in this paper (bias-adjustment versus independent assessment versus ...), and so forth.

Section 2: I think the WRDC data should also be introduced in this section.

P3 L8–9 – This section provides details of ERA-Interim, and the various gridded and station observation datasets used to assess the quality of this Reanalysis: The various observation datasets are also the ones used for the bias-adjustment, right? That should be clarified.

P3 L9–10 – may be regridded and *can be interpolated:* It should be described only what has actually been done for this paper and associated data set, not what "may be done".

P3 L14 – 3 h (forecast): Well, in this case every other time step is still an analysis, right?

P3 L1–5: I don't find where CRU and GPCC are actually used in the remainder of the paper, except where it's stated that the authors "began by comparing ERA-Interim against the gridded observational products at the monthly timescale", followed by some statements that are, however, not supported by figures. It is stated that "they are of potential use" in certain circumstances, but it appears that they do not enter the associated data set. If that's right, it should not be stated that these products are used (as done in *P3 L24*).

Section 2.4 – HelioClim: I am wondering whether this satellite-derived data set might have been adjusted to the WRDC data – in which case the improvements shown in

Fig. 8 might be not so conclusive? Please clarify.

Section 3: Given that, to my understanding, only univariate adjustment is applied to generate the present datasets, I find it distracting that much of this section is about methods for multivariate adjustment.

P5 L8 – Bias adjustment and bias correction are widely-used terms for the assessment of climate model output: One can assess climate model output without having bias adjustment in mind.

P5 L11–12 – the assumption is made there that Reanalyses are correct: I don't think that anyone assumes absolute correctness of reanalysis data – which can equally not be said of station data.

P6 L26 – for each month: I guess this is meant in a climatological sense, right? So I recommend to modify this to something like *for each month of the year*.

P7 L4–6 – The similarity of the two distributions in terms of their scale and shape parameters indicates that bias adjustment could be achieved by replacing the ERA-Interim scale and shape parameters with those inferred from the HadISD stations: Im not convinced by this argument. If a transformation based on a certain distribution type is "valid" should in my view rather depend on the degree to which the two samples are consistent with the theoretical distribution used. The fact that the parameters are similar rather indicates that the bias is not very large, after all.

P7 L12–13 – Figures 2 and 3 exhibit the Weibull distribution fits of the HadISD observations: I think that this is not correct, see my corresponding general comment.

P7 L26–27 – the number of stations in some parts of Europe is less dense, so involving greater extrapolation from stations more distant from the grid boxes: I fail to understand why this should result in larger discrepancies in less dense areas. After all, locations with no stations close to them are not evaluated – because there are no stations at that location to do that! Shouldn't it be quite the opposite, namely, that isolated stations

should show only small discrepancies after the procedure because the nearest grid box is influenced almost exclusively by that station, implying very weak interpolation effects? Please clarify. (This point is closely related to my general comment regarding missing independence of data used for adjustment and (the same) data used to check the post-adjustment "bias".)

P8 L3 – for the nearest E-OBS grid box: Is E-OBS not on the same grid as the interpolated ERA-Interim data? Please clarify.

P8 L4 – Data are then normalized as in Equations 4 and 5: It appears that the normalization is described only be Eq. 4, whereas Eq. 5 describes the back-transformation.

P9 L8–11 – Any type of bias adjustment procedure will additionally be influenced by the quality of the station observations, [e.g.,] by the large differences in potential height between some observing locations and the average height field used by ERA-Interim: Indeed, and I think this point – stations at a particular point not necessarily being representative of a "grid cell" – should be mentioned more prominently and generally in the paper (it doesn't hold just for temperature and humidity, but for all considered parameters).

P9 Equation 6: I think that the right-hand-side gives the probability density function rather than the cumulative distribution function (which the left-hand-side implies). In addition, it appears that β is the scale parameter and α the shape parameter, rather than the other way around as stated in the subsequent sentence.

P9 L24–25 – all precipitation amounts below the threshold are set to zero, further improving the agreement between E-OBS and ERA-Interim in the number of dry days per month (i.e. days with rainfall less than the 1.0 mm threshold): This seems illogical: the modified values were already below the threshold, so they were already "dry days" according to the definition.

P10 L17–18 – The method 'quantile mapping' applied to KT was preferred (and is

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used here) as it usually brings improvement with no degradation of the bias, while the other methods often degrade the bias in a noticeable way: What does this imply for the other parameters where no independent validation was conducted, and where only on adjustment method was applied? It appears to me that this point to some extent compromises the validity of the bias-adjustment approach used in this work.

P10 L21–23: Half of this paragraph is just a repetition of the corresponding figure caption (where that kind of information belongs).

P10 L30–P11 L2: Same as previous point.

Section 5: The discussion covers mostly outlook-type points, whereas I think that potential weaknesses of the bias-adjusted data (including some of the criticism I've brought up) would need more discussion.

Figures showing distribution parameters: Many of these need units (e.g., the scale parameter in Fig. 1) which should be added to the colour bars.

Figures showing distribution parameters: Some colour bars should be adjusted to have green at zero to be more intuitive.

Figures showing distribution parameters: I can't resist mentioning that the used colour bars are not colour blindness friendly.

Figure 18: I would find it helpful if the symbol shape would still have the same meaning as in Fig. 17, but with colours coding the additional dimension of "bias improvement/degradation".

Technical corrections

P2 L7–8 (and throughout the paper) – bias adjustment versus *bias-adjustment:* Should be spelled consistently.

All equations and lists: These should be formatted with appropriate punctuation.

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