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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.

Jones et al present a dataset of meteorological variables at half-degree resolution for Europe based on bias correction of ERA-Interim surface reanalysis data. They use observations to correct the distributions (e.g. mean and skewness) of the variables rather than just correcting the means. The new product is designed to be of use for the energy sector.

Although well presented and clear, there are two overriding problems with the manuscript in its current form. Firstly, their methodologies have limitations that are

not explored in appropriate depth - particularly with regard to their revised distributions for near-surface air temperature, relative humidity and for daily precipitation totals. Secondly, although the text repeatedly refers to an earlier dataset, also at half-degree resolution and based on bias correction of ERA-Interim reanalysis data (i.e. the WFDEI, Weedon et al. 2014 WRR) there is no comparison made between the new data and the existing dataset or other existing alternatives. Since they have corrected both the means and distributions of the ERA-Interim data I was surprised that there is no demonstration that compared to observations the new dataset represents an improvement over the existing datasets. This would both demonstrate that it is more useful for the energy sector and support their claim that their methodology is worth applying in other circumstances.

Specific comments.

a) Dew point temperature.

The authors start the manuscript by stating that previous datasets applied bias corrections that were independent for each meteorological variable. This is misleading since the WATCH Forcing Data methodology (WFD), that was used to create the WFDEI (i.e. Weedon et al 2010 Tech Rep cited by Weedon et al 2011 JHM, Weedon et al 2014 WRR) specifically adopted the approach specified by Cosgrove et al 2003 (JGR). The Cosgrove et al approach was also used by another reanalysis-based dataset not mentioned: the Princeton Global Forcing (PGF) of Sheffield et al 2006 (J Clim).

The Cosgrove et al approach ensures that changes to near-surface temperature are incorporated into changes in air pressure, specific humidity and then downwards long-wave radiation by using sequential processing (maintaining the original covariances). Surprisingly, despite raising this issue Jones et al adjusted air temperatures independently of dew point temperatures and consequently, as discussed on manuscript page 9, encountered implied relative humidities exceeding 100% (specifically avoided in the WFD, WFDEI and PGF). I strongly recommend that the authors design an approach that ensures that adjustments to air temperature and dew point temperature are applied in a consistent fashion - to avoid the possibility of their implied relative humidity exceeding 100%.

b) Air temperature.

The Cosgrove et al (2003) methodology allows for elevation differences between observation sites and the mean elevation of reanalysis grid boxes. There are elevation differences between the heights of the EOBS temperature sites and the half degree elevations of the adjusted products. Hence an important step is to ensure that firstly the EOBS temperature data have been adjusted, using a lapse rate, to sea level, then interpolated to half degree and then recover the interpolated EOBS temperatures at the height of the ERA-Interim half degree grid boxes. As such the resulting interpolated EOBS data can then be used to correct the interpolated ERA Interim temperatures (similarly interpolated at sea level, Cosgrove et al 2003). This is such a critical processing step that I assume it was an oversight that it was not stated by the authors.

The average bias of their corrected air temperature product is shown for April in Figure 8. There are numerous areas within the map where the mean air temperatures do not match the observations to within a small part of a degree Celcius especially in mountainous areas and Turkey (where there is a bias of more than 2 oC). Since their methodology is designed to fix the means as part of the bias correction I find such differences to be surprising at the least and it definitely requires explicit explanation in the text. The text should also describe how and why the air temperature bias maps vary by month or season.

Furthermore, the distributions of corrected air temperatures shown for the Scottish site differ substantially in terms of the most probable value by around one degree in winter (Dec and Jan). Somehow the modal temperature has been changed from more than the EOBS mode in the original ERA-Interim data to less than the EOBS mode. I would have expected the modes to align everywhere after the adjustments. Again

this requires specific comment. It is conceivable that the means do in fact match with say 0.1oC whereas the modes differ substantially for December and January, but this needs explanation/clarification for the reader. Are there similar issues elsewhere and if so why?

c) Precipitation.

Aside from consideration of numbers of wet days, a gamma distribution has been used to correct the distribution of daily precipitation totals. Unfortunately, the illustrated results are not encouraging. For the Scottish example in Figure 14, in May to September the distribution is improved compared to the original data, but there appears to be room for further improvement. Nevertheless, illustration of the WFDEI and/or PGF results would probably put the adjusted Scottish values into a better perspective.

In the case from Slovenia (Figure 15) the results suggest significant methodological problems. In April to August it is clear that the adjustments have made the distributions worse than the original ERA Interim data. In these months not only is the modal value further from the observations than the raw data (green line further from black obs than the orange line), but also at intermediate precipitation totals (2 to 8 mm/day) the corrected distribution is worse than the original data (in this region of the graphs higher). These problems should be discussed in the manuscript.

It is clear that the authors appreciate that different localities require different approaches (manuscript page 10 line 3). Why have the authors not designed code that, when the distribution is not improved by the adjustments the results are reverted to uncorrected values? Piani et al (2010 J Clim - NB not the Piani et al 2010 paper cited by Jones et al) adopted a variety of transfer functions for daily precipitation totals chosen according to their appropriateness by location (ranging from simple to reasonably complex). The figure showing the spatial distribution of the adjusted precipitation results is provided for October (Figure 16). However, we are not told whether other months have worse performance (as is implied for the Slovenia example at least for April to August).

The manuscript definitely needs to appraise the success of the precipitation bias corrections more critically with reference to issues linked to both location and month or season.

d) Comparison to previous datasets.

I am very surprised the authors did not explicitly demonstrate that their data provide advantages in terms of reliability compared to the existing, widely-used datasets that are currently available for the same time interval and the same spatial resolution (i.e. WFDEI and PGF). Both diagrams illustrating the point and a text discussion of the advantages of the new product merit a new section.

Technical corrections.

p2 line 22 The WFDEI now extends to the end of 2014 (not 2013 as stated).

Figure 14 There is no such thing as negative mm/day in precipitation. Regardless of the calculation of the gamma distribution extending to negative values, the plots should be truncated at 0.0 mm/day.

Figures 17 and 18: a) Remove the grid lines (they are confusing and were not used on the other maps). b) Simplify the information by using a single symbol coloured according to the size of bias (this makes the maps more comparable to the previous way biases were shown). c) Illustrate a colour scale for easy determination of the size of the bias. d) Do not use green and red at the extremes of a distribution as it disadvantages colour blind readers. Simply use blue at the opposite extreme to red.

Recommendation:

Major revisions addressing the concerns listed above.

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