Clim. Past Discuss., 8, C417–C420, 2012
www.clim-past-discuss.net/8/C417/2012/

Climate of the Past Discussions

Interactive comment on "Correcting mean and extremes in monthly precipitation from 8 regional climate models over Europe" *by* B. Kurnik et al.

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Received and published: 21 May 2012

As the authors of the paper indicate in the Introduction, there is growing interest in using RCM outputs directly in impacts modelling and hence in bias correction. This is reflected in a growing literature on bias correction – which is somewhat more extensive than the two recent Piani et al papers cited (see Additional references in the Supplement).

For the purposes of many impacts applications, including hydrological and crop modelling, input data is generally required at the daily resolution – and for multiple variables (including both temperature and precipitation) which should ideally be corrected in a self-consistent manner. Thus this paper which focuses only on monthly precipitation is likely to be of limited interest.

The authors acknowledge that the distribution-fitting approach by definition is expected to provide good agreement between monthly means. Thus it is of interest to also consider performance with respect to extremes. While precipitation extremes are most commonly defined using daily time series (e.g., consecutive dry days, days above 90th percentile, maximum 5-day rainfall), there is in principle no reason why extreme rainfall seasons shouldn't also be defined. However, the justification for choosing the two thresholds of 200 mm and 400 mm is not given – and should be. There is also an issue in defining an extreme dry month as one with no rainfall. While this may be appropriate for some regions and seasons – it is certainly not appropriate to call a dry summer month in many parts of the Mediterranean an 'extreme'. In the Introduction, the authors raise the problem of meteorological drought – but it is not clear that identifying single dry months is an appropriate indicator of drought. The persistence of dry conditions, e.g., consecutive dry months, also needs to be considered.

The approach taken in the paper is to consider cases of where RMSE is reduced, or skill scores are better for corrected than raw data, as cases of 'effective correction'. However, errors may still remain and the improvement may only be marginal. Ideally, the 'starting point' needs to be considered when assessing the 'improvement'. The final sentence of the abstract argues that 'the corrected precipitation fields will improve results of the climate impact models'. There should be some demonstration of this – or the wording of this claim should be modified. 'How good the input data needs to be' is likely to be related, in part, to the sensitivity of the particular impacts model(s) used.

The paper includes quite a large number of Figures (15 in total), but these are not always very informative or clear and figure captions in general are all too brief. In particular, I find it hard to understand exactly what Figures 10 to 12 are showing – what are these percentages? If both observed and simulated/corrected values are over the threshold is it assumed that they are 'correct' or is account also taken of the actual magnitude?

In general, results are simply presented and described – with rather little or no interpretative discussion. This further limits the relevance and interest of the paper. Why, for example does correction 'fail' for such a large percentage of the area in the ETH model? And why for the DM1 and SM1 models in the case of extreme high precipitation? Why does correction apparently work well in some regions and not in others?

The first paragraph of the Introduction cites some fairly old literature on changes in natural hazards. In this context, it would be good to refer to the recently published IPCC Special Report on Extremes – in particular Chapters 3 and 4 http://ipcc-wg2.gov/SREX/report/. Though particularly with respect to flood events, the hazards mentioned in this paragraph are generally (and most appropriately) defined at the daily timescale rather than monthly.

The scope of the paper is further limited because there is no discussion of issues associated with the application of the bias corrections to future projections. To what extent does the correction scheme change the projections – including the ensemble spread? And some discussion is needed of the appropriateness of the assumption that biases are stationary (i.e., time independent) – see, for example, Christensen et al, 2008.

Finally the paper would benefit from some editing to improve the grammar. In the list of modelling centres in Section 2.2 capitalisation should be used consistently, e.g., Swedish Meteorological and Hydrological Institute. For the DM2 model, I can't find the Meteorologiques, 2008 reference in the reference list.

Given these shortcomings and the limited scope of the paper, and the previously published work on bias correction at the daily time scale of the ENSEMBLES simulations (Dosio and Paruolo, 2011; Rojas et al, 2011), it is unfortunately hard to see that this manuscript brings anything new to the literature.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/8/C417/2012/cpd-8-C417-2012-supplement.pdf

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Interactive comment on Clim. Past Discuss., 8, 953, 2012.