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Hydrology and Earth System Sciences Discussions

Interactive comment on "*HESS Opinions* "Should we apply bias correction to global and regional climate model data?"" *by* U. Ehret et al.

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As much as a agree with the authors that bias correction is often used as an - possibly unjustified - ad hoc "correction" of climate model data, and that the bias correction procedure as well as the raw data should be communicated to the end user, I disagree with the author's rather fundamentalistic view. In my opinion this view is inconsistent and too strong.

Among several points I would like to discuss the following:

1. The whole line of argument is based on a rather black and white painting of numerical models solidly grounded in the laws of physics vs. rather heuristic bias correction

methods. But is this distinction actually true? I am not an expert in parameterisation schemes, but following the discussion about the inherent problems of parameterisation schemes (truncation of scales and violation of scaling laws, collapsing physical processes to their mean...) and the advantages of stochastic parameterization schemes compared to deterministic parameterisations (e.g., Palmer, QJRMS, 2001; Berner et al., Mon. Wea. Rev., 2011), I would be careful about such idealised views of numerical models. Also when considering regional climate models, one usually faces inconsistencies between large and regional scales (deviations in the circulation, or unphysical moisture budgets towards the boundaries), and in particular the local scales do in general not feed back into the large scales. Of course, bias correction methods are simple and purely empirical, but is the distinction so clear cut when, e.g., considering approaches such as the one by Themessl et al., (IJC, 2011) using pyhsically motivated predictors? I would therefore ask the question: where is bias correction valid, where is it invalid? A soon to appear publication by Eden et al. (J. Climate, 2012) about different types of model errors could guide the discussion.

2. Many shortcomings that might be caused by a naive bias correction actually might already exist in uncorrected model simulations - and could potentially be corrected by bias correction. For instance, a climate model might systematically underestimate spring temperatures in a mountain catchment because the model topography is too smooth - a bias that can arguably be corrected. Calculated runoff might be far too low, because the model might produce snow where in reality rain was falling. Would a hydrologist involved in planning a flood protection system really care about the slight violation of the water budget between the corrected and the uncorrected climate model?

3. This brings me to the question of relevance. Even though the author's reasoning might be true in principle, what is the actual extent of the potential danger compared to the benefits of bias correction? The answer to this question depends most likely on the variable, on the region and on the investigated impact.

4. I find it slightly problematic to base the rejection of a whole set of methods on a list

of assumptions that actually does not apply as a whole to many of the methods. The line of argument would only hold if any of these assumptions alone would justify the author's conclusions. But is this really the case?

Apart from these points, the discussion of Maraun (Geophys. Res. Lett., 2012) should be corrected: he defines different types of biases nonstationarities and distinguishes between apparent and real nonstationarities. He could not identify any nonstationarities due to changing relative occurrences of weather types, but only found considerable bias changes due to different climate sensitivities, and apparent bias changes due to sampling variability.

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