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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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General comment

The problem of bias correction is currently attracting a lot of attention in the atmospheric, climate and hydrological communities. This paper is presenting a nice overview of the main concerns related with this common correction practice of climate model outputs, and in particular their potential impact on hydrological simulations. The authors discuss the heterogeneity of definitions of bias, their potential causes and magnitudes, the way in which they are corrected, the assumptions behind the different approaches, and the implications in practical applications. The authors also put forward their own view on the future progresses that should be made in order to avoid these bi-

ases and their statistical corrections, arguing that these corrections are mostly masking the model deficiencies rather than correcting it. I am personally inclined to agree with this view when communicating the uncertainty information to the end users (in particular for climate projections), but not when analyzing and producing the "best" forecasts and simulations. My specific comments are provided below.

Specific comments

This opinion paper is nicely presenting the problematic behind bias correction, but it mostly considers the "bad side" of bias correction that could indeed mask to the end user the reality of the deficiencies present in the model (and that could impact hydrological model simulations). But the purpose of correction is to provide the best possible forecasts or simulation. This is per se a valuable approach in which post-processing (e.g. bias correction) has a natural place, in particular for problems in which a tradeoff between computer time needed and model sophistication should be found. However the uncertainty of forecasts, projections or simulations should also reflect the presence of these system biases. I would suggest the authors to make this distinction in the paper.

At the end of Section 6, the authors indicate that information on the bias could help in identifying some model deficiencies. I think that this aspect deserves a slightly larger place in this paper (a subsection or at least a few paragraphs). For instance biases associated with problems of resolution for some specific processes were extensively discussed, see e.g. Giorgi and Marinucci (1996); or problems of coupling with the other components of the climate system, see e.g. Sen Gupta et al, (2012). In addition I would like to stress that besides the correction of the impact of model errors the identification of model error sources is possible based on post-processing techniques (see Vannitsem and Nicolis (2008) and Vannitsem (2008) for weather forecasts). In this perspective, the approach is not only a statistical tool but also a diagnostic tool. This type of analysis could most probably be extended to seasonal or decadal forecasts.

The authors indicate that the result of bias correction does not respect the balance between the different fields. This in turn could considerably affect the optimal use of hydrological models. Instead they propose to use multi-model ensemble averages in order to correct for biases (Section 6.2). I agree with the general statement on the problem introduced by bias corrections. However the proposed averaged solution does not fulfill the required balance (in nonlinear systems) and will give rise to "model outputs" much smoother than the original model runs that could not be produced by the model or nature itself. Moreover, the multi-model approach (weighted or un-weighted averages) is not free from biases that could depend on climate modifications (see the discussion of page 5377, lines 15 to 21). I suggest commenting on these problems.

The authors propose in progressively improving the models in order to avoid the use of post-processing (in particular bias correction), section 6.3. To my opinion this plan, although obviously suitable from a scientific point of view, is not realistic because the practical user requests will closely follow the model developments. For instance, the impact of floods or pollution in urbanized areas is now a major concern of our societies and models are developed at the scales of cities in order to provide practical answers (e.g. Koussis et al, 2003). Obviously these models cannot describe all the details needed in order to assume that the forecasting system is sufficiently "good" (from the user's point of view), and post-processing could be also very helpful at this scale. To my view, post-processing techniques are complementary approaches allowing for improving further the quality of forecasts or simulations, but it should be used appropriately and the corrections should be provided with information on the associated potential uncertainties in particular for climate projections or impact studies.

Page 5371, lines 23 to 26, the authors advocate for having the same biases in the LSM and HM. I do not understand why, since once the output of the GCM/RCM is corrected, it is closer to reality and should therefore be better for its use in the HMs. Could the authors clarify their statement?

Page 5371, line 27. Would you please clarify what means "offset"?

Technical and minor comments

1.The definition of bias is clearly a key aspect as stated by the authors. I agree with the authors that bias should be confined to differences between averages.

2.Page 5357, line 6. The authors suggest that GCMs are the best tools to understand climate dynamics. I do not fully agree because observations provide (maybe even more) valuable information on climate dynamics. I therefore suggest to write " Today, among the best tools. . .".

3.Page 5367, line 7, Li et al (2009) should be Li et al (2010).

4.Page 5369, line 6, replace "bias specific" as "model error source specific".

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