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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The effect of empirical-statistical correction of intensity-dependent model errors on the climate change signal" *by* A. Gobiet et al.

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"This study discusses the effect of empirical-statistical bias correction methods (quantile mapping, QM) on the change signals of climate simulations; in fact it has been previously shown that bias correction can alter the mean temperature climate change signal derived from multi-model ensembles in Europe. By means of an analytical analysis of the model error and its dependence on the value of simulated variable, the authors claim that the climate signal is artificially inflated by intensity-dependent model errors. By removing these intensity-dependent errors QM can therefore potentially lead to an improved climate change signal. The manuscript is very interesting and usually

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well written and deserves publication after some minor corrections:“

Thanks for your constructive comments and suggestions. We added our responses below your original comments:

“1) Figure 4: I found the colors used for different lines very confusing (eg Feb, March, Aug and September are difficult to differentiate). I would prefer to group seasons according to a similar color schemes (e.g blue for winter, green for spring, etc.) Also, it is striking to me how the model error characteristic in, e.g., IP (SMHI) changes so drastically from Jan to Feb, passing from a positive to a negative slope. Is there any plausible explanation for that behavior?”

Thanks for the suggestion. We changed the colors in Fig. 4 (see Fig. 1 of this response) in a way that each month in a season has the same color (e.g., the winter months December, January and February are now blue). Within the seasons we discriminate between individual months by using different line styles. The new figure 4 is attached to for illustration. The same modifications have been applied to all similar figures in the supplementary material (Figs. S1 – S8) and the captions have been adapted accordingly. Regarding the explanation of the difference between the Jan and the Feb error characteristics of the SMHI model in IP, we refrain from guessing. Such interpretation is outside the scope of the study. We don’t aim to analyse the errors of each individual model in each region in detail, since the focus of this study is not on model development, nor the physical explanation of model errors. We take them as given and focus on their influence on the climate change signal and on empirical-statistical post-processing of the model results.

“2) Figure 5: is the bold line (“ensemble average error characteristic”) the ensemble mean of the individual models’ errors, or the error of the multi-model mean? As in many works it is claimed that the MM mean usually outperforms any single models, would it be possible to show the error characteristic for the MM mean as well?”

We show the average of the individual model’s daily temperature error characteristics,

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not the error characteristics of the daily ensemble mean temperature. Both would be identical for the bias, but not necessarily for the "error characteristics" (i.e. the ECDF). We show the former, since it directly relates to the analysis we perform and to the quantities described in the formulae of our study. Although the question raised by the referee (does the multi-model mean outperform individual models?) is interesting, it is not directly relevant for our analysis. Therefore, and since we don't want to distract from the main topic of the study, we prefer not to add the error of the multi-model mean temperature to Fig. 5.

"3) Is there any reference for Eq 2?"

We are not aware of any reference for Eq. 2. This simple formula has been originally designed for this study and represents a simplified (linearized) model of "intensity-dependent" model errors. Its explanation is given in the paper. In particular, please refer to section 2.1 for the discussion of the concept of intensity-dependence and to the paragraph directly after Eq. 2 for a brief discussion of the validity of the linearization.

"4) I have some problems with the notation of eq 6 (and similarly, eq 8) Is Delta Y supposed to be Delta Yi ? And similarly is cov(s,Dy) supposed to be cov(si,Dy'i)? If the authors chose to change the notation for clarity, they should specify it in the text. Unless I am wrong about the notation, but then I do not understand eq 6, as Dy is not defined in the text, for instance."

Since our current notation of equation 6 and the following equations is misleading, we changed all affected equations following the suggestion of the referee.

"5) In fig 8 it is striking how QM and LC give sometimes opposite results. The authors briefly address this point claiming that it needs further analysis. In my opinion, the fact that the QM method applied here uses the same constant correction outside the calibration range is a major point. Would it be possible to perform a simple test (on only one month for only one model) by using a QM method with a linear correction even outside the calibration range and to compare it with both the original QM and the

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LC?”

The differences between QM and LC pointed out by the Referee are unquestionably disturbing. However, we didn’t find a way to clearly identify the reason within the time-resources we had for this study. A set of experiments with clearly defined data- and model error characteristics would be needed to distinguish between different potential reasons (i.e. this would involve carefully designed artificial data and artificial models). This was beyond the resources we had for this study and we hope it is acceptable for the referee to leave it to further research.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 5671, 2015.

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Interactive Comment

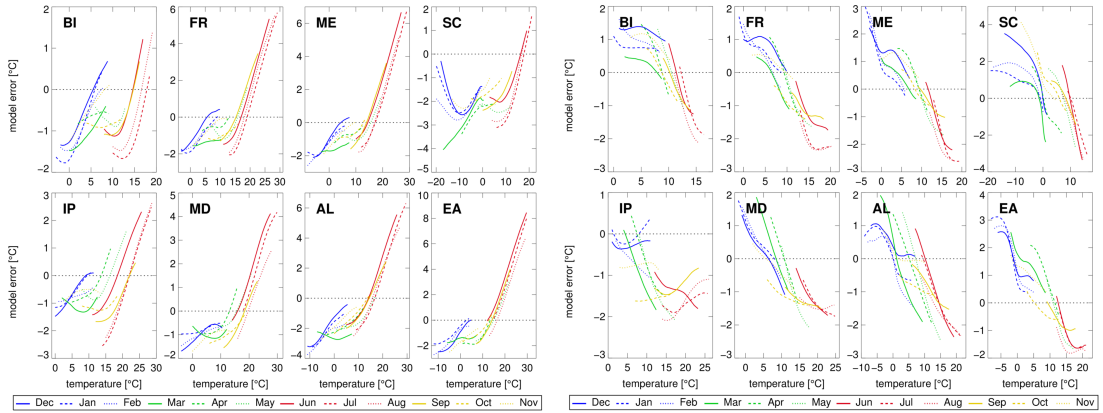

Fig. 1. Figure 4 of the manuscript.

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