Hydrol. Earth Syst. Sci. Discuss., 10, C5742–C5745, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C5742/2013/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Bias correction can modify climate model-simulated precipitation changes without adverse affect on the ensemble mean" *by* E. P. Maurer and D. W. Pierce

Anonymous Referee #1

Received and published: 9 December 2013

Recently, problems have been detected in the use of quantile mapping for climate change simulations. In particular, several authors have shown that quantile mapping affects GCM trends. The authors of this paper address the question, whether the mapping actually deteriorates the change signal compared to observations. As such the topic is highly relevant. Also the authors show nicely the effect of quantile mapping on trends in their synthetic example. Yet I am still concerned about the setup of the study and the conclusions drawn.

Major comment: The authors compare AOGCM simulations (as far as I understand these are really coupled simulations, not driven with observed SST) for the US with

observed data over two historical time periods and assess the effects of QM calibrated in the first period and then applied on the second period. These time periods are each 30 years long. It is well known that the climate of the US is strongly influenced by internal modes of climate variability, such as the PDO and the AMO. For instance, the AMO has a period of roughly 60 years and strongly controls the amount of precipitation over the US (e.g, Knight et al., GRL, 2006). The amplitude of this internal mode of variability is of the same order of magnitude than the observed climate change signal. See, e.g, Deser et al., Nat Clim Change, 2012, for the influence of internal variability on temperature in the US, a variable which has a much better signal to noise ratio. Thus the observed trend is only partly (if at all) a forced trend. As the GCMs are run in climate mode, their realisation of the AMO is not synchronised with the observed AMO, i.e., the 30 year long ups and downs will almost certainly not coincide with the observed ones. This has two important consequences:

1. the observed differences between observations and models are not biases, but a superposition of biases and random differences due to long term modes of climate variability.

2. there is no reason why the modelled trends should match the observed trends. The forced trends of course should, but not the overall trends which are a superposition of forced trends and random fluctuations. The defined index were a useful index if the forced signal were isolated, i.e., without internal climate variability. But in the current setting, it is not a useful measure. A perfect climate model might have a very bad index model, just because the realisation of random climate variability is out of phase with the observations, and a bad climate model could in principle have a good index value because a bad forced trend superimposed by an out of phase realisation of climate model variability might by chance produce the observed trend.

Note again, that this is not an academic problem. Internal variability is a major source of uncertainty of precipitation projections and makes up about 30-50% of the total uncertainty even on time horizons of 60 years on a continental level (Hawkins and Sutton,

Clim. Dynam., 2011). This problem has already been discussed in Maraun et al., Rev. Geophys., 2010 - the authors should be aware of it. In fact, they observe this problem for the simulations of the East Coast trends, where they found opposite trends in observations and half the models (p 11593, l 25).

I am not sure what conclusions should be drawn from this point. One which definitely has to be made is that GCM biases cannot easily be calculated and thus also not easily be removed (apart from the fact that bias correction in general works locally, but GCM circulation biases are non local, for a discussion see Eden et al., J Climate, 2012). My recommendation would be that the authors repeat the analysis with AMIP type simulations (i.e., atmosperhic models forced with observed SST to synchronise long term internal climate variability) or even better to use RCMs or nudged GCMs to avoid the erroneous correction of GCM circulation biases. But I see the point that the author's want to "correct" (coupled) GCM biases to finally provide bias corrected future simulations. Yet, again, GCM bias correction is not a simple task (and the fact that hundreds of studies have been published based on such corrections is not necessarily an indicator of quality). So far it has not been shown that GCM bias correction works in principle, it has just been applied. As the main point of the paper is about effects of quantile mapping, a compromise could be to point out all the problems listed above with proper references, and tune down the conclusions. The following points definitely need to be mentioned:

-biases are systematic differences in the physics of a model, i.e., in forced signals, not random realisations.

-it is difficult to estimate GCM biases in presence of internal modes of variability such as the AMO.

-GCM bias correction is therefore also difficult. Here the East coast example might be shown.

-one should really define which biases to correct, see Eden et al, J Climate, 2012 (note

that they call internal climate variability errors, which is at least misleading; they mean uncertainty; personal communication with the authors).

-in particular bias correction works locally (e.g., convective parameterisation errors could be corrected), but cannot shift, e.g., the storm tracks.

-currently it has not been shown that GCM bias correction really works (as it has been shown for RCM bias correction, e.g, Maraun, GRL, 2012).

Minor comments:

in the title it should be effect, not affect.

page 11586, line 26ff: "in any downscaling...". This statement is wrong. Most statistical downscaling approaches are perfect prog, i.e., by construction they do not correct GCM biases. Please state this!

page 11595, line 26ff: please rewrite the following five sentences. They all start with "we". This is tiring.

page 11588, line 27: this does not hold for rare extremes. There, parametric distributions are needed to constrain the mapping. This is, however, difficult to validate because of the rareness. Please add "moderate"

Eq 2: use a different name than just "index". It carries no information!

page 11591, l 1ff: this effect has been shown in Maraun, J Climate, 2013. Please cite.

page 11591, l10: not M-M, but M=M

Eq. 3: again, use a different name

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11585, 2013.