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Hydrology and Earth System Sciences Discussions

Interactive comment on "Technical Note: Downscaling RCM precipitation to the station scale using quantile mapping – a comparison of methods" *by* L. Gudmundsson et al.

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Referee # 3 raises interesting concerns regarding the structure and the focus of the manuscript as well as our terminology. We will address these concerns as indicated in our response to the referees specific remarks:

1: Relevance of the topic

We appreciate that Referee # 3 acknowledges the relevance of our analysis.

2: Novelty of the concept

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General remarks:

The analysis presented does indeed not present any new techniques and there is a large body of literature (we refer to it in the manuscript) concerned with comparable problems.

However, we are not aware of any empirical analysis that exclusively assesses quantile mapping (QM) using an extensive data base of observations. Therefore we hope that our investigation will provide users of QM with a helpful guidance for the selection of the appropriate methods. (See also our response to point (7))

Referee # 3 also suggests to put more emphasis on the set of skill scores employed to assess the different methods. We will therefore extend the sections introducing these score to clarify some aspects of the analysis.

As the study is highly specialised and very limited in scope we deliberately chose the short format of a "Technical Note".

Comments on suggested references:

Reviewer # 3 also mentions four references with the recommendation to cite these.

We would like to note that two of these (Chen et al., 2011a,b) are highly localised case studies which are investigating the suitability of various down scaling approaches in the context of very specific hydrological applications. There is a multitude of comparable studies and we have cited many of these in the original manuscript. (An exhaustive review would be beyond the scope of the article). Nevertheless we will try incorporate the mentioned studies were appropriate.

The study of Johnson and Sharma (2011), focuses on "Accounting for inter-annual variability" which was not within the scope of our analysis. Nevertheless, this reference is a useful addition in the discussion of the limitations of the investigated methods.

The only real review article mentioned is the study of Teutschbein and Seibert (2012).

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As this article has not been available before June 5th 2012 we where not able to know of it until now. As Teutschbein and Seibert (2012) provide a comprehensive overview of different bias correction approaches it is a relevant and welcome contribution to our study.

3: Are substantial conclusions reached?

We appreciate that Referee # 3 values our ranking of the methods.

We would also like to note that cross validations is a standard method in statistical modelling which is commonly used and has very well understood properties. (See for example the relevant chapters of Hastie et al. (2001) or any other suitable textbook for a detailed coverage of this and closely related techniques). Therefore a detailed commentary on cross validation does not belong into the conclusion of this article.

Nevertheless we acknowledge that this technique is not that well known to the hydrological community and will incorporate a more detailed description of the method.

4: Clarity of the text and sufficiency of details

We acknowledge that the combined presentation of methods and results was not ideal and will restructure the text accordingly. We will also add a section commenting on the limitations of QM if faced with new data, including future time periods.

5: Are the results sufficient?

We are glad to hear that our results are sufficient.

6: Insufficient explanation of cross validation

We will extend the manuscript accordingly.

7: Indication of new/original contribution and crediting other peoples work

See also our response to point (2).

The scope of this study was the assessment of a large number of QM methods using

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an extensive data base of observed precipitation series in Norway. The only study that uses a comparable set of observation we know of was presented by Themeßl et al. (2011). However, Themeßl et al. (2011) assess a wide variety of post processing approaches, concluding that QM is most reliable, whereas the submitted manuscript focuses exclusively on QM.

8. Terminology

Referee # 3 raises interesting concerns regarding our terminology. The main issue is our use of the term “quantile mapping”.

We would like to emphasise that our terminology is entirely based on the use of “quantile mapping” and well established synonyms (see e.g section 3.2.5 of Teutschbein and Seibert (2012) for a well referenced list of synonyms) in the scientific literature. Our formalisation of QM does closely follow Piani et al. (2010) who also introduce transfer functions for the quantile mapping task. We will add references at the relevant sections to increase clarity.

Further it should be noted that some other bias correction techniques such as “Linear scaling”, “Local intensity scaling”, “Power transformation” or “Variance scaling” as e.g described in Teutschbein and Seibert (2012) differ, despite their structural similarity, in one important aspect to the parametric transformations as introduced by Piani et al. (2010). The difference is in the definition of the parameters. The parameters of the methods mentioned by Teutschbein and Seibert (2012) are derived from statistical moments of observed and simulated variables (e.g. means, variances). This does guarantee the equality of the according moments after correction, but does not attempt to adjust the quantile-quantile relation explicitely. This is contrasted by the definition of quantile mapping by means of a transfer function/parametric transformation (see Piani et al. (2010) and Eq. (1) in the discussion paper). Here QM is defined by the attempt to transform the modelled values such that its distribution matches the observations. Parametric transformations (Piani et al., 2010), attempt to achieve this by modelling

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the quantile-quantile relation in a regression setting, where parameters are found by minimising the difference between the observed and the transformed modelled distribution.

We chose not to comment in detail on this issue because it would draw attention from the scope of the article: To evaluate the power of different QM methods.

9. Abstract

We are glad to read that our abstract is complete and concise

10. Structure of the article

We acknowledge the deficiencies in the structure of the text will modify the article accordingly.

11. Language

The language was carefully checked

12. Mathematical Notation

We will formally introduce h, a, b, c, x and τ.

13. Clarifications

We extended the methods section and disentangled the introduction of methods and results according to the referees suggestion.

The paper was held deliberately short to fulfil the requirement of Technical Notes for brevity and (more importantly) also to allow the readers to access the main results in a quick and uncomplicated manner.

14. References

We will incorporate the references where appropriate.

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