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Reply to comments from reviewers

We appreciate the comments from the reviewers who have given very constructive and thorough reviews. We have tried to answer the questions best possible and we believe that it has improved the manuscript considerably. Below you can find the reply to each of the two reviewers.

Yours sincerely

Torben O. Sonnenborg

Reviewer #1:

The authors provide an interesting and up-to-date analysis of the impacts of climate model uncertainty and geological model uncertainty on hydraulic head, stream flow, travel time and capture zones. The manuscript is very well written, concise, includes a clear motivation and fits well in the scope of HESS. I enjoyed reading the manuscript and recommend to accept it after a few technical corrections (see comments below).

MINOR COMMENTS:

Q1: Page 4357, lines 19-20: "no model is generally superior to the others". Looking at the Nash-Sutcliffe coefficients (E) in Table 1, I would conclude that L1 and L2 are not suitable for any streamflow simulations...

Reply: It is correct that the Nash-Sutcliffe coefficient of some models, especially model L2 (E = -0.12), is relatively low and might be less suited for stream flow simulations. We have tried to make a more balanced description of the calibration/validation results, see below.

Modifications: The description of the calibration/validation results has been changed to: "Some models are more suitable for stream flow simulations (e.g., N2) while other models are stronger on hydraulic heads (e.g., L2). However, based on an integrated evaluation of the calibration/validation statistics no model is generally superior to the others."

Q2: Table 3 is not entirely needed, as it is not that relevant for the results of this paper.

Reply: We agree that Table 3 takes up a lot of space and that it is not essential for the paper.

Modifications: Table 3 has been removed from the revised manuscript. Instead, a reference to the PhD thesis by Lauren Seaby has been added (Seaby, 2013).

Q3: Page 4358, lines 6-20: I don't believe that the DC method is the most appropriate method that should have been used here. Even though the authors argue that van Roosmalen et al. (2011) have shown that changes in the dynamics are not important when mean variables are considered, Teutschbein and Seibert (2013) proved that the DC method is the least reliable under changing conditions even when considering only the mean value (it can't deal with bias non-stationarity). This drawback should be addressed in 1 or 2 sentences.

Reply: We know the excellent study by Teutchbein and Seibert (2013) and agree to their conclusion that more sophisticated methods such as distribution based scaling in general are likely to be more robust and hence more reliable than the simple DC method for future projections. For the particular Danish situation, Seaby et al. (2013) compared the DC method producing the factors in Table 3 (removed from revised manuscript) with a double gamma distribution based scaling (DBS) showing that both the DC and the DBS methods are able to capture the mean monthly and seasonal climate characteristics in temperature, precipitation and potential evapotranspiration when tested against observed data for the period 1991-

2010. Seaby (2013) further showed that, when propagating climate projections for 2071-2100 through the same hydrological model type as used in our study, the results for the projections based on DC and DBS bias corrections were almost identical with respect to mean annual discharge, 1th percentile discharge, 99th percentile discharge and mean groundwater heads. We therefore firmly believe that using DBS corrected climate data would have resulted in almost identical results and would definitely not have affected the conclusions of our study.

Modifications: The original text "The DC method does not include changes in precipitation dynamics such as number of dry/wet days but as shown by van Roosmalen et al. (2011) this is not important when mean variables, e.g., mean monthly stream discharge or mean annual hydraulic head, are considered." has been changed to: "The reliability of the DC method for projecting changes has rightfully been questioned by Teutchbein and Seibert (2013) who found that more advanced methods were more reliable. In our specific case Seaby et al. (2013) compared the DC method with a double gamma distribution based scaling (DBS) showing that both methods were equally good in capturing the mean monthly as well as the seasonal climate characteristics in temperature, precipitation and potential evapotranspiration when tested against observed data for 1991-2010. Seaby (2013) further showed that, when propagating climate projections for 2071-2100 through the same hydrological model type as used in our study, the results for the discharge and groundwater head characteristics used in our study are almost identical for the two bias correction methods. This confirms the results of van Roosmalen et al. (2011) and justifies the use of the simple DC method for our particular application."
Teutschbein and Seibert (2013) has been added to the reference list.

REFERENCES:

Teutschbein, C. and Seibert, J.: Is bias correction of regional climate model (RCM) simulations possible for non-stationary conditions?, Hydrol Earth Syst Sci, 17(12), 5061–5077, doi:10.5194/hess-17-5061-2013, 2013.

Van Roosmalen, L., Sonnenborg, T. O., Jensen, K. H. and Christensen, J. H.: Comparison of Hydrological Simulations of Climate Change Using Perturbation of Observations and Distribution-Based Scaling, Vadose Zone J, 10(1), 136–150, doi:10.2136/vzj2010.0112, 2011.

Thank you for a constructive review.

Reviewer #2:

Major remarks

The authors present an uncertainty analysis on groundwater and discharge related future projections using an ensemble of climate change projections from 11 GCM-RCM combinations that are used to force various versions of a distributed hydrological model (HM) with 6 different geological model setups. This analysis is a valuable contribution to HESS, but it requires a few clarifications and revisions before it may be published.

Q1: Future changes are considered by comparing two 20-year periods. While this may be sufficient for temperature changes, this might be too short if hydrological changes are considered. For precipitation, at least 30 years need to be considered to get a robust climatology as for shorter periods decadal variability may significantly impact the temporal precipitation averages over such periods, and this is usually impacting other hydrological variables in the same way, at least those that strongly depend on precipitation. In this study this is certainly the case for discharge. Thus, it should be either shown that decadal variability does not play a role in the considered region, especially for discharge, or the considered time periods need to be extended to 30 years.

Reply: This problem was addressed by Seaby et al. (2013) for the same geographical area as considered here. Monthly DC factors for precipitation were calculated for 5, 10, 15, 20 and 30 year periods using six different climate model data. The analysis showed that reference periods of 10 years and below had high variability between DC factors while period lengths over 15 years appeared suitable, se Figure below.

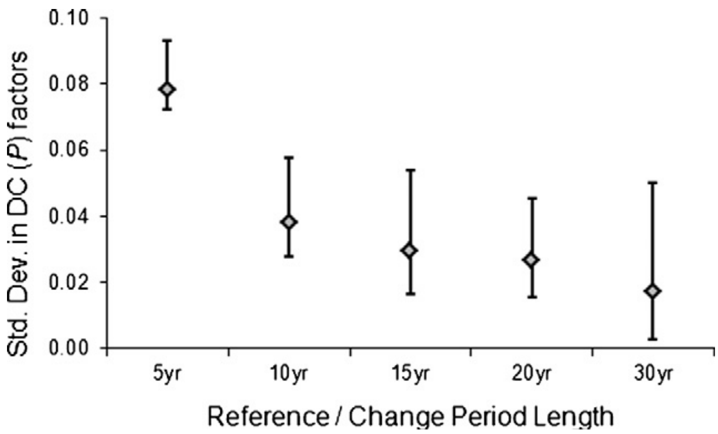

Fig. 7.Mean std. dev. of annual DC values for precipitation in six climate models from 5, 10, 15, 20, and 30 year reference periods compared to far-future periods of the same length. Error bars show the range in std. dev. across the six models.

Modifications: The following sentence has been added to the revised manuscript: "Seaby et al. (2013) analyzed the impact of the length of the reference and the future periods and found that period lengths over 15 years appeared suitable for precipitation. Hence, comparing two 20-years periods is assumed to be adequate for the particular study area."

Q2: The treatment or behaviour of the capture zone is not clear to me. I understand that a capture zone defines the area from which a specific well gets its water from. In my opinion this is purely defined by geological characteristics and should not depend on any climate forcing, i.e. the capture zone should neither depend on the climate model nor should it change under climate change conditions. Thus, if there are such dependencies on climate, then the definition of the capture zone seems to be wrong or there are some model errors.

Reply: The location of the capture zone indeed depends strongly on the geology, and so does the shape of the capture zone. However, it is well known that the size of the capture zone depends on groundwater recharge, which again is a function of precipitation. The volume of water abstracted at the well should correspond to the spatially integrated groundwater recharge. Hence, at low precipitation the recharge area (and hence the capture zone) will be relatively large while at high precipitation the capture zone will be relatively small. Therefore, the shape of the capture zone will among other factors depend on geology and the areal distribution of recharge (and hence precipitation).

Modifications: No modifications have been done.

Q3: Similarly, simulated travel times may only depend on climate if, in addition to their dependency on geology, they also depend on the amount of flowing water. Thus, to understand the behaviour of travel time with respect to climate forcing, it should be indicated how the travel times/flow velocities in each of the HM versions used depend on the flow volume.

Reply: Precipitation may affect the hydraulic heads and the hydraulic gradients in a specific area which affects groundwater discharge and hence the flow velocity. Additionally, flow paths to the abstraction well may change as the size of the recharge area changes. For example, in a situation with low precipitation a larger recharge area is required and larger volumes of the subsurface are activated in the particular capture zone compared to a situation with relatively high precipitation.

Modifications: No modifications have been done.

In summary, I suggest some revisions to be conducted before the paper may be accepted for publication.

Minor Comments

In the following suggestions for editorial corrections are marked in Italic.

In several places the use of singular and plural is erroneous. Thus, the manuscript should be carefully checked to correct those gramma errors, e.g. p.4356 – l.21 "*is*" instead of "are", p.4363 – l.18 "show", p.4364 – l.6 "*depend*", p.4364 – l.13 "*are*", p.4367 – l.11 "*depend*". In addition, cross-references to tables and figures (and even some literature references) are often set within Commas, which interrupts the text flow. In my opinion they should be placed in brackets. Examples: p.4356 – l.24, p.4357 – l.11, p.4357 – l.15.

Reply: We appreciate the advice concerning the grammar and the cross-references. We went carefully through the manuscript and checked singular and plural as good as possible. Cross-references were changed according to the suggestions.

p. 4353 – line 16
… uncertainty *due* to the climate …

Reply: Done

p. 4356 – line 23
… models *using* between …
Reply: Done

p. 4356 – line 25
… models comprises two …

Reply: Done

p. 4358 – line 4
…Model *pairings* …
Reply: Done

p. 4359 – line 9/10
… hydrological *variables* is …

Reply: Done

p. 4361 – line 3
No reference geology is defined and *as due to the DC method, the same reference climate is used for all projections, the* uncertainty …

Reply: Done

p. 4362 – line 10
Figure 4 also shows that …

Reply: Done

p. 4363 – line 8
It is written: "The relative change is almost constant for the six models …"
In this paragraph, you are still dealing with the absolute values of discharge and the respective standard *deviations*, not with the future changes. Thus, I don't understand this sentence.

Reply: The formulation of this sentence was indeed not good and has been changed in the revised manuscript, see below.

Modifications: The sentence has been changed to: "The ratio between the standard deviation and the median value is almost constant for the six models."

p. 4363 – line 25
…than to *the* geological …

Reply: Done

p. 4364 – line 6
… on *the* geological …

Reply: Done

p. 4376 - Table 5
The figure caption suggests that all numbers in Table 5 are standard deviation. But this does not make sense for the column denoted as mean change. The overall standard deviation of the change relative to the reference climate cannot be significantly smaller (or even zero) than the standard deviations associated with the sub-ensembles of geology and climate, such as is the case for Head and summer discharge. I assume that mean change does not denote a standard deviation but the projected mean change. This should be made clear in the caption.

Reply: We agree that the caption was not clear in the original manuscript. It has been changes such that it is now clear that the column "Mean change" do not denote a variance component.

Modifications: The last sentence in the table caption has been changed to: "All variance components (columns denoted "Geology" and "Climate") are presented as standard deviations. The column "Mean change" denotes the projected mean change."

p. 4379 – Figure 3 caption
… are forced by …

Reply: Done

p. 4379/80 – Figure 3/4
I suggest using the same y-axis scaling in Fig. 3b and Fig. 4 to allow an easier comparison between the two figures.
Reply: Done

References

Seaby, L.P., J.C. Refsgaard, T.O. Sonnenborg, S. Stisen, J.H. Christensen, and K.H. Jensen (2013), Assessment of robustness and significance of climate change signals for an ensemble of distribution-based scaled climate projections, Journal of Hydrology, 486, 479-493, http://dx.doi.org/10.1016/j.jhydrol.2013.02.015.

Thank you for a careful and constructive review.