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Hydrology and Earth System Sciences Discussions

Interactive comment on "Climate change effects on irrigation demands and minimum stream discharge: impact of bias-correction method" *by* J. Rasmussen et al.

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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, especially with respect to the simulation and comparison of current and future climate conditions which was not described adequately in the original manuscript. Below you can find the reply to each of the two reviewers.

Yours sincerely, Jørn Rasmussen

Reviewer #1: The authors address an important topic, which is of interest to a broad audience. The authors aim is to assess the effects of downscaling methods on the impact of climate changes on irrigation demands and low flow of streams. So, the main focus is on differences in results in downscaling methods and their ability to predict extremes.

Question #1: However, the authors also compare the future to the current climatic conditions. For the current climate, however, it is not clear if observations or simulations are used. The comparison may therefore be not valid, as i) the observations comprises a period of only 20 years and the future climate a period of 30 years, and probably more important ii) different methods to calculate evapotranspiration (Makkink vs. Penman-Monteith) have been used. From the text, it is not clear if the comparison is valid. Additionally, I have some concerns on the parameterization of ET in the model, and on the method that is used to describe the irrigation demand. These, and some other issues that I explain below, need to be addressed before this paper can be considered for publication.

Reply: We agree that the original version of the paper was not clear with respect to data used for current and future climate. In the first version observations were used for current climate while simulations were used for future climate. This has been changed in the revised version such that for the DBS method simulated climate data from 1981-2010 are used for current climate while simulated data from 2071-2100 are used for future climate. Hence, i) both comprise a period of 30 years and ii) the same method is used to calculate evapotranspiration in both periods (Penman-Monteith).

Modifications: A new scenario has been added to the study, 'Current DBS' which acts as the basis for evaluating the changes when comparing with the DBS scenario. Thus, changes are calculated as the difference between Current and DC, and between Current DBS and DBS. This way, the influence of the bias correction method can be isolated and studied more clearly.

Question #2: p. 4993, l. 14-15 The authors state that the aim of the study is to assess the effects of downscaling methods on the impact of climate, but in p. 4994, l. 3-5 the authors also state that the study focuses on the changes from the current climate to the far future. These are two different goals. For the first aim, comparison with the current climatic conditions is not needed, and for the second one, the methods used for the current and future climate should be the same. This holds, for example, for the period used (20 vs 30 years) and for the methods used to calculate ET (Makkink vs Penman-Monteith). 30 year periods are generally considered to represent the climatic mean.

Reply: We agree that the original paper was not clear with respect to the goals. The primary objective of the paper is to assess the effects of downscaling method on the impact of climate change. As explained above the procedure to evaluate the impact has been changed in the revised paper such that both the periods used are the same (30 vs 30 years) and the methods used to calculate ET are the same (Penman-Monteith vs Penman-Monteith).

Modifications: The procedure has been changed, so it is now easier to study the effect of the downscaling method (see question 1).

Question #3: p. 4995, l. 20-28; p. 4996, l. 1-10: the authors state that ETact is direct RCM output, but that they prefer using ETref, with the output variables of the RCM. These output variables, however, correspond to the atmospheric conditions related to the actual/real vegetation that is present. The air temperature, for example, is determined by the actual vegetation: a dry site will have a relatively low cover and low ETact, which results in higher Tair. The ETref represents a grass cover of 12 cm height etc, optimally provided with water. Tair of the reference grass will be different from that of the actual vegetation. This also holds for other variables as the vapour pressure deficit. So, if the variables given by the RCM refer to the atmospheric conditions above the ac-

tual vegetation, these should not be used to simulate the ETref. The authors need to clarify the validity of their calculations of ETref.

Reply: In principle, we agree that ETref should be evaluated on the basis of atmospheric variables representing conditions corresponding to reference conditions. However, the method applied corresponds to what is normally done using measured data: the Penman-Monteith equation is applied using measured air temperature, humidity, etc. for the conditions prevailing at the measurement site. This is the standard procedure to calculate ETref. Additionally, the RCM does not include the effect of irrigation on evapotranspiration which is very important for the current study and the analysis carried out. Hence, the ETact provided by the RCM cannot be used for our study. Furthermore, the RCM representation of vegetation (25 km grid) is much coarser than the representation in our hydrological model (500 m), where we also can include irrigation.

Modifications: None

Question #4: p. 4996, l. 20-22; table 4: A RCM simulation of the historic climate has been done, which presumably represents a 30 year period, and simulates ETref using Penman-Monteith. Instead of using the observed climatic conditions for the comparison between current and future climate, the RCM's of the historic climate and future climate (both 30 years) should be used. So, the comparison should be RCMhistoric to RCMfuture (both with ETref according to Penman-Monteith), instead of observed to RCMfuture (Makkink and Penman-Monteith, respectively). This also holds for the results section and/table 4. Maybe the authors did do the right comparison (in Figure 5 for example, they mention a 'current scenario'), but this is not clear (at least, this 'current scenario' only represents 20 years, see conclusions-section).

Reply: We agree that the RCM's of the historic and future climate should be used, both using Penman-Monteith for calculating ETref. This has been changed in the revised manuscript.

Modifications: See reply to Question #1 above.

Question #5: p. 4996, l. 26-19: relative change factors are derived using observations for the current climate. For ETref this may be problematic, because ETref_Makkink will be different from ETref_PenmanMonteith. For a sound bias correction, the methods used to calculate ETref should be the same for the current and the future climatic conditions.

Reply: We agree that the best procedure is to use the same method for calculating ETref for current and future climate. In the revised manuscript ETref is calculated using the Penman-Monteith equation for both periods when the DBS correction is used.

Modifications: In the revised manuscript the DBS method is based on simulated climate data for both current and future climate. Hence, the same equation is used for calculating ETref (Penman-Monteith).

Question #6: p. 4998, l. 23: ETref is not observed, but calculated (see p. 4994, l. 13), also for the current climate. So, it would be better to replace this by 'ETref for the current climate. . .

Reply: We agree that the suggested formulation is more precise.

Modifications: The sentence has been corrected.

Question #7: p. 5001, l. 16-9: The authors indicate that irrigation is described using a demand given scheme. The demand, however, is calculated indirectly, using soil moisture contents. In agro-hydrology, it is common practice to relate relative crop yield to the relative transpiration rate, Yact/Ypot Tact/Tpot (De Wit, 1958;Ben-Gal et al., 2003). The water demand is given by the potential transpiration Tpot, which reduces to the actual transpiration Tact if water availability is limiting. As the hydrological model involves the calculation of transpiration, it would be reasonable to focus on Tpot-Tact to identify the need for irrigation. Tpot-Tact also provides direct information on the amount of irrigation that is needed to allow optimal transpiration. By focusing on Tpot the focus is on both water availability and water demand, which is reasonable to consider as both

will change with the changing climate.

Reply: While there are other suited methods for calculating irrigation demands, we found the applied method to show reasonable performance when comparing modeled yearly irrigation amounts with actual reported irrigation amounts. Just as the above method for determining irrigation demand, the applied method is able to take both water availability (in the study denoted "AW" or "available water") and water demand (expressed in the SMD-value) into account. Furthermore, several parameters related to irrigation demands were included in the calibration.

Modifications: No modifications have been made.

Question #8: Additionally, the available water for crop transpiration is not the difference between the actual soil moisture content and the soil moisture content at wilting point (soil water pressure head of -16000cm), because the reduction of root water uptake and transpiration already starts at much lower soil water pressure heads (about -500cm for grass, linearly reducing to -8000 cm). In hydrological models this reduction is often given by the function of (Feddes et al., 1978). Reply: The reviewer is right, and this phenomenon was included in the model although it was not described in the paper.

Modifications: An explanation as to how this aspect is included in the model, has been included in section 2.3.1: "However, the transpiration is corrected to account for the reduction of root water uptake and transpiration that occurs at lower moisture deficit. In the model, this is included by reducing the transpiration linearly from when the AW is a fraction of 0.75 of the maximum available water content (MAW) to the wilting point. Thus, transpiration will occur at the maximum rate until this fraction is reached. Then the transpiration will decrease linearly until the wilting point is reached, at which point transpiration becomes zero".

All in all, the authors should improve the description of irrigation demand in the model.

Question #9: p. 5002, l. 11-15: Additionally, how climate-robust are crop factors and should these be used for trees, for which interception is large? In general, crop factors are derived based on measurements, where soil evaporation, transpiration, and interception are involved. The crop factor approach may be preferred for model simplicity, but the empirical crop factors can only be applied under the environmental conditions in which they were determined (and probably not for future climatic conditions). The use of crop factors especially is a problem for trees (also for the current climate) for which interception (see e.g. (Savenije, 2004) is much larger than for the reference grass. Some discussion on the climate-robust estimation of ET is given, but the focus mainly is on the effect of CO2. More discussion is required on e.g. changes in vegetation characteristics, the water and energy balance, use of empirical crop factors, growing season.

Reply: The use of crop factors is indeed problematic especially for forest where the interception loss is significant. However, the catchment considered is dominated by agriculture (78%) and forests only cover a very small part (6%) of the area. Still, we agree that climate change may impact vegetation characteristic, probably such that higher crop factors should be used in the future.

Modifications: In the discussion the following paragraph was included: "Other factors and parameters that may change from the current climate to the future climate, but which have not been considered in this study include crop coefficients, Leaf Area Index, and Root depth. However, the changes on these parameters are highly uncertain, and including them are outside the scope of this study."

Technical corrections: p. 4990, l. 13: flow in -> flow is Reply: Done. p. 5000, l. 15: add a reference to the SWAP model, e.g. (Van Dam et al., 2008) Reply: Done. Table 3: Root depth grass -> root depth agricultural grass (to be consistent with table 2) Reply: Done. Fig 6, caption: for the current climate, and for the future climate according to the DC method and the DBS method. Reply: Done. p. 5008, l. 26: that the while -> that while Reply: Done. p. 5009, l. 22: Vidaa -> Vidå Reply: Done. p. 5010, l. 14: a decrease -> decreases Reply: N/A. p. 5014, l.8: limiting the transpiration -> limiting

the potential reference evapotranspiration Reply: Done.

Thank you for a constructive review. âĂČ

Reviewer #2:

The authors compare, in a well organised manuscript, the influence of different downscaling techniques on the projected impact of climate change on irrigation demands and low flow of streams. I think more insight in the influence of downscaling technique on the projected impact is very useful and should be the main focus of this paper rather than (again) comparing the DC and DBS method.

Question #1: This can be achieved by two major adjustments/extensions. The uncertainty related to the choice of downscaling technique should be compared to the uncertainty related to the choice of RCM, GCM and/or emission scenario. Therefore some additional RCM-GCM scenarios should be added to the study.

Reply: It is correct that it would be interesting to examine the uncertainty related to the choice of downscaling technique, RCM, GCM and emission scenario. This is, however, beyond the scope of our contribution and it is not the objective of our study. The objective of the present study is to investigate the impact of downscaling method on projected low flow in streams when field irrigation is applied. Irrigation demand depends on the water content in the root zone which again depends on precipitation amounts and dynamics during the growing season (which will be different for different downscaling methods). This in turn affects groundwater abstraction since all irrigation water in the study area originates from groundwater. When groundwater is abstracted less water discharges to the streams. Hence, the choice of downscaling method may be important for this special problem. To highlight the idea of the paper, an additional analysis has been included in the revised manuscript where it is assumed that no irrigation (and groundwater abstraction) takes place. In this case low flow in the streams is not a function of the water content in the root zone during the growing season as no groundwater recharge is generated in this period. Therefore, the impact of downscaling method on low flow will be more significant when for irrigated catchments than for catchments that are not irrigated. We believe that this is an important finding which to our knowledge has not been published before.

Modifications: The motivation for doing the study has been described more clearly and the objectives of the paper has been reformulated. Model results with and without groundwater abstraction for irrigation are presented in the revised manuscript.

Question #2: The other point is the inappropriateness of the set of downscaling techniques. The applied delta change methods only adjust for changes in the mean, which obviously is not sufficient as other important characteristics, like different modes of variability are projected to change too. Yet, more sophisticated delta change methods have been successfully applied (Bakker and Bessembinder, 2012; Van Pelt et al., 2012). As a matter of fact the transformation algorithms (here referred to as "flavours") for delta change methods and bias correction methods are interchangeable. Thus, the flavor applied in DBS could also be applied in the delta change method, maybe in a slightly adjusted form. For the choice of different downscaling techniques, obviously insufficient methods should be left out. For the bias correction methods at least a control period should be included to test the appropriateness of the method (how well does the DBS method reproduce the irrigation regime and low flow of streams in the control period?). Besides, it would be interesting to consider methods that adjust the multi-day variability rather than daily variability (van Pelt et al., 2012, Wood et al., 2002; 2004).

Reply: First, we are aware that the delta change method is very simple and not adequate for all situations. Nevertheless, the method is widely applied and therefore it is relevant to analyze when it can be applied without significant loss of accuracy. Second, the objective is to investigate the impact of using a very simple bias-correction method compared to a more sophisticated method. Therefore, two methods that are highly different were chosen: the DC method only corrects the monthly mean and preserved the dynamics from the control period, while the DBS method reproduces the future dynamics in climate projected by the climate model. We are sure that this is a relevant

investigation to carry out.

We agree that DBS method should also be used and tested for a control period. This has been included in the revised manuscript.

Modifications: The performance of the DBS method for the control period is evaluated and presented in the revised manuscript, notably in table 4 and figures 6 and 7.

The study would seriously benefit from including more RCM-GCM combinations and a more elaborate choice and evaluation of the chosen downscaling methods. In the following, some minor points are summed.

2 Methods

2.2.1. Current climate

Question #3: - The observed dataset is rather short (1990-2010 or 1991-2010?) for analysing year-to- year variability. Why not using alternatives like E-OBS (Haylock et al., 2008)? - Please, explain a the Makkink equation, adjusted for Denmark. Reference is hard to trace.

Reply: The observed dataset covers the period 1991-2010, i.e., a 20 year period. In a parallel study (Seaby et al., 2012, under revision to J. Hydrology) we examined the impact of the length of control period and found that the variability is captured well if the length of the period is beyond 15 years. The dataset provided by Haylock et al. (2008) is based on very few rainfall stations, at least for Denmark. Hence, the spatial distribution is not captured well. Additionally, it is not clear how Haylock et al. (2008) handled rain gauge catch corrections. Large errors can be found if dynamic correction of the measured rainfall is not carried out (Stisen et al., 2011). Therefore, we prefer to use the dataset described in the paper.

The adjusted Makkink equation is explained in the revised manuscript.

Modifications: The Makkink equation is described in section 2.1.1

2.1.2. Future climate

Question #4: - Christensen et al. (2010) is wrongly referenced as they did not analyse RCMs nested in GCMs, but nested in the reanalysis ERA40. Also manuscript title of Christensen et al. (2010) is wrong. It should be “Weight assignment in regional climate models” rather than “weight assessment”. Please, include doi numbers if possible. This substantially improves the traceability of the referred documents in case of slightly incorrect citations.

Reply: We agree with the reviewer.

Modifications: The Christensen et al (2010) reference is no longer found in the paper, and doi numbers have been added where available.

Question #5: - Why a different ETref is used for the future climate? How do both methods compare?

Reply: We agree that the method used in the original manuscript was not adequate.

Modifications: In the revised manuscript a new scenario has been added to the study, 'Current DBS' which acts as the basis for evaluating the changes when comparing with the DBS scenario. Thus, changes are calculated as the difference between Current and DC, and between Current DBS and DBS. This way the same method is used for calculating ETref for the historic and future period, both using Penman-Monteith for the DBS method.

- “bias correction” mentioned between equations 2 and 3 is not a bias correction, but an adjustment or perturbation etc. Reply: While the DC method effectively removes model bias by only perturbing change, it is a form of bias correction, though we do agree that the terminology could be improved. Modification: At equation 3, the sentence now reads: “...is the precipitation after perturbation using the change factor”.

2.2.1 Estimation of the CO2-effect on crop evapotranspiration

Question #6: - SWAP needs a reference

Modifications: Reference added

4.2 Evaluation of model performance

Question #7: - Introduce stations before. Which stations show good performance (R2-NS of 0.75 and 0.89)? - A R2-NS of -0.02 and 0.07 means absolutely no skill. You should argue why this model is good enough for this study anyway.

Reply: The model was found to simulate low flow very well, which is the most critical to this study, but had some difficulty capturing the peak flows. The Nash-Sutcliff coefficients presented in the paper are calculated for the entire year, and therefore incorporate both the low flow and peak flow, which leads to the low NS-values. Hence, we believe that the developed model is able to capture the effects of using different downscaling methods, although the reliability of the results at the large discharge stations with the higher performance is found to be superior to the smaller stations with the lower performance.

Modifications: A short description of the discharge stations has been included in section 4.1. The acceptable performance in low flow situations has been underlined in section 4.2.

5 Results

Question #8: - Please, better explain the evaluation metrics. - Comparison of minimum and maximum yearly values is pointless. They are too much influenced by natural variability.

Reply: We agree with the reviewer.

Modifications: Minimum and maximum values have been changed to 5-percentile and 95 percentile values respectively.

Question #9: Besides, two periods of different length are evaluated (20 years and 30 years) – Show the performance of the DBS in the control period

Reply: The DBS for the control period now forms the basis for comparison with the DBS results for the future, and are therefore added to all figures and tables where applicable. The comparison between observed climate and DBS for the control period is clear in figures 6 and 7, as well as in table 4.

Modifications: None

References: Stisen, S., T.O. Sonnenborg, J.C. Refsgaard, L. Troldborg, and A.L. Højberg (2011), Evaluation of climate input biases and water balance issues using a coupled surface-subsurface model, Vadose Zone Journal, 10, 37-53, doi:10.2136/vzj2010.0001.

Thank you for a constructive review.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4989, 2012.