Anonymous Referee #2

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This study deals with the hydro-climate evolution of the Skjern river basin in Denmark from 1875 to 2007 based on observations (river flow, precipitation, temperature) and hydrological modelling. A substantial part of the paper is dedicated to the question of the applicability of the hydrological model outside its calibration period and to the question of extremes in river flows.

It is a very interesting paper, with potentially important results, but as explained bellow, some major modifications are needed, on important scientific points, before publication (data quality, role of anthropogenic and natural climate change etc.) Moreover, the paper deals with many (interesting) issues, but maybe too much of them, and it is not always easy to see what is the main point of the paper, what is its main goal. And despite the length of the paper, some important aspects of the study are not detailed enough (e.g. the question of the role of climate change) while too much details are given in other places (e.g. the historical description of the basin from 1700). I think that improvements in the structure of the paper would be highly beneficial.

General comments.

1. The changes in different variables described in the paper are very large compared to what one typically see, most notably in precipitation. The question of data quality is therefore especially important here, but the authors do not really deal with those questions. The readers need to know whether the data have been homogenized or not, how quality control has been done, whether a statistical algorithm to detect ruptures has been used, whether the authors checked meta-data to verify if changes in instruments (type, location etc.) occurred etc. Moreover, I'm pretty sure that several studies already analysed precipitation trends in Denmark, or Northern Europe (or Europe). The authors need to check if their results are consistent with those analyses and to cite those studies (they cite two technical reports focused on Denmark but it is not sufficient) to put their results into context.

   Response: *We appreciate the advice to test for homogeneity. In consequence the four main stations have been tested using the Standard Normal Homogeneity Test, SNHT (Alexanderson 1986). A previous study by the Danish Meteorological Institute (Cappelen et al. 2008; Frich et al. 1996) tested the two stations 21100 and 25140, north and south of the catchment, using the SNHT on monthly time series and found that both stations were homogeneous. This fact was verified by testing of the two stations against one another.*

   The initial test using the two homogenous stations as reference stations showed that all four main stations were in fact affected by at least one in-homogeneity. Using correction factors based on the fraction between the mean before and after the break, all series were adjusted and subsequently re-tested. Two of the stations were found to be homogeneous after the first correction. The two remaining stations were re-tested using the method described in Easterling and Peterson (1995) for multiple breaks; here the precipitation series are divided into *subsections before and after the break. Each subsection is hereafter tested to identify additional breaks. The procedure is repeated until no breaks are found. One station with 5 breaks and one with 3 breaks were found, and the breaks were corrected starting from the youngest break (in time). After homogenization the trend in precipitation was reduced; from 46% to 26% (for the catchment precipitation). The change in the precipitation series requires that the hydrological model should be re-run and the extreme indexes should be updated. The results and the steps of the SNHT-analysis will be included in the article.*

Additional references to literature on precipitation trends have been found from Scandinavian (Frich et al. 1996; Schmidt 2001) from Holland (Buishand et al. 2013), and from European studies (Klein Tank and Können 2003; Thomsen 1990) and will be incorporated.

2. The paper is long, probably too long, I think. At some places a lot of details with limited interest (in the context of this study) are given, but some important aspects of the study are not detailed enough. For example, I'm not sure that it is really useful to give the formula of the Pearson correlation, RMSE or describe the Mann-Kendall test that have been used by countless studies and described in many references.

   Response: *As proposed above, the entire manuscript has been re-organized. This includes that the Pearson formula has been removed and the discussion on possible climate change drivers has been deleted.*

3. The authors dedicate a large part of the paper to the question of extremes (definition, results etc.). But their analysis is based on the results of an hydrological simulation, which is not able to capture well the evolution of river flows after 1970, which somewhat limits the interest of the analysis. I think the part on extremes could be simplified and shortened, because it is not the main point of the paper (to my opinion). For example, it is not surprising that changes in extremes depend on the reference period, and therefore I'm not sure that the analysis and discussion on this point are really useful. On the other hand, there is no validation of the model regarding its capacity to simulate extremes, which is important, and should be added.

   Response: *The issue of the models capability of simulation extremes is a valid point. An evaluation of the models ability to describe the extreme events will be included, based on the new model runs (with homogenized precipitation). Furthermore, the drought/flood index on observed data will be compared to the drought/flood index as produced from the simulated data. We agree that the section is too long. It will be shortened and the figure of the extreme events limited to showing only the detrended reference series.*

4. The discussion about the potential role of climate change is poor. The authors conclude that it is not possible to explain all the climatic changes observed in the Skjern basin with the present knowledge (p2404). But no analysis is provided to support this conclusion (and I think that they are wrong). The authors don't even talk about the potential role of natural climate change (solar variability, volcanism), that is known to have played a substantial role a least in the first half of 20th century.

Response: *We agree that the discussion on the potential role of climate change was not satisfactory. This problem is not essential for the article and is has therefore been removed.*

5. The authors try to explain the strong centennial quasi-linear trends in river flows, precipitation, temperature by three climatic indices (NAO, SCA, AMO). But as those indices do not exhibit such centennial trends, no analysis is really needed to conclude that they are no responsible. The analysis is not uninteresting by itself but I'm not sure that it is necessary given the goal of the paper, as it is more relevant for interannual to interdecadal timescales.

   Response: *We agree that the analysis of the reasons to the climatic changes was inadequate. As pointed out above, this analysis is not necessary given the objectives of the paper and it has been removed from the revised manuscript.*

6. The performance of the hydrological model is poor after 1970, even when the model is calibrated after 1970. Despite a very long discussion about potentials explanations, nothing really conclusive emerges. The discussion is more qualitative than quantitative. The authors tend to attribute those changes in performance to direct anthropogenic changes in river flows. But those changes would have needed to be quite massive and abrupt to explain that, because the change in the performance of the model is abrupt. One could think that such abrupt changes would have been better documented. Moreover, the fact that the calibration on the 1961-1970 provides the best results on the full period raises questions as it corresponds to a period of relative stability in temperature (fig 3).

   Response: *We agree that it is somehow peculiar that the changes happening after 1970 are not documented better. However, as described more clearly in the discussion of the revised manuscript, two significant changes are observed after 1970: large scale irrigation and increased fertilizer application. These changes might in combination explain the shift in model performance. This problem will be dealt with in more detail when the model results using the homogenized rainfall data are ready.*

<u>Specific comments.</u>

1. Page 2375 24-29 and page 2376 1:10. Long discussion, but not very informative. The same elements can be given in a few lines.

   Response: *The discussion on drought definitions has been compressed*

2. Page 2378. This discussion that describes the evolution of the basin since 1700 is lengthy. It is not uninteresting, but I wonder whether it is really necessary in the paper, as the analyses deal with the 1875-2007 period. Moreover, very few references are given, which is somewhat problematic. Line 1 to 11: is Fritzboger (2009) relevant for all those lines? The discussion in section 5.2.1 (50 lines) repeats many points given p2378. I don't think it is good for the structure of the paper, and it is clearly not efficient. Finally, these discussions are very qualitative.

Response: *We agree that the structure of the original paper was not satisfactorily. In the revised manuscript the discussion of historical changes and their impact has been collected in a single paragraph.*

3. Page 2379, l 3-4. How is it done? Could it have a noticeable impact on the results?

   Response: *The supplementary station with the best correlation to the main station was used to supplement the main stations time series. If the supplementary station with the best correlation did not contain data for the missing time slice, the supplementary station with the next-best correlation was used. No clear tendency was found when comparing the timing of significant breaks and the periods with missing data. Hence, it is assumed that the gap filling do not impact the results significantly.*

4. P2382. Mann-Kendall test. How autocorrelation in the series is taken into account?

   Response: *Until now autocorrelation has not been incorporated. But for the revised article an autocorrelation coefficient will be calculated for all series. Where autocorrelation is present and significant on a 5% level, a modified Mann-Kendall will be applied, using a pre-whitening of the data.*

5. P2386. This discussion is long, probably too long.

   Response: *The discussion of choice of reference period has been reduced.*

6. P2387, line 22. The figures given correspond to the main four stations used in the paper for the hydrological simulation, for the 1920-2007 period, right? It is not very clear.

   Response: *Yes that is correct. It is described more clearly in the revised article.*

7. P 2387. "and the increase can therefore not be dismissed as unrealistic." OK, but it would be better that the increase can be proved realistic. I suppose that it is not the first paper to study precipitation trends in Denmark or (northern) Europe. It would be nice to provide references that show maps of the trends etc. See also my main comments.

   Response: *Yes this is a valid point. See response for General comment #1.*

8. P2388, line 5. As there is a large trend in temperature, it is not directly obvious why there is no change in snowfall. Is there a compensation between the impact of increased temperature and the impact of increased total precipitation on snowfall?

   Response: *The description of snowfall has been clarified and moved to a paragraph where a* *discussion on the impact of snowfall on gauge catch correction is carried out.*

9. P2391, line 26. Is not possible that the issue comes from the hydrological model itself?

   Response: *We see no reasons why this effect should originate from the hydrological model itself. Please also refer to our responses to item #18 below.*

10. P2393. Semantic issues (also line 1-5 page 2396 etc.). The authors distinguish between anthropogenic changes (changes in irrigation, land use etc.) and climatic change, the changes driven by climate. But climatic changes are likely also partly caused by humans and therefore, by definition, anthropogenic. It is misleading I think, and it should be modified. For example, the authors could use the expression "direct anthropogenic changes" to talk about the "anthropogenic changes" of the current version of the manuscript. They should discuss that definition early in the manuscript and explain that climatic change could also be anthropogenic and that in that case, one can talk about indirect anthropogenic change etc.

    Response: *We agree that the use of the term "anthropogenic changes" was ambiguous in the original manuscript. In the revised manuscript "direct anthropogenic changes" is used to talk about the local changes carried out in the catchment.*

11. Section 5.2.1 I don't think discussing the changes anterior to 1950/1960 is really useful here because the authors are only interested by what happened after 1960. The discussion (that is interesting) is not particularly convincing because it is more qualitative than quantitative. But I guess that the authors tried to use all available data.

    Response: *We did try to use all available data on historical changes. However, the discussion on the changes anterior to 1950 has been compressed in the revised version of the manuscript.*

12. Section 5.2.3. Errors due to the hydrological model could perfectly influence the results in one direction, if the model do not represent well some processes that are particularly important after 1960. For example the period after 1970 is a period of rapid increase in CO2 concentration. The 1970-1990 period also corresponds over Europe to serious solar dimming because of anthropogenic aerosols. Those changes result in changes in the surface energy budget (modulation of incoming longwave radiation or incoming shortwave radiation) and therefore probably in the surface hydrological cycle. Is the hydrological model able to capture the impact of those radiative changes on river flows? It is not obvious especially since no information on radiation is used in the computation of potential evapotranspiration. For me, given the elements provided by the authors, it cannot be totally excluded that the errors after 1970 are due to the hydrological model.

    Response: *It is correct that the model is not able to account for changes in CO2 concentrations when calculating actual evapotranspiration from potential evapotranspiration and soil moisture* *content. However, this is a minor effect with today's climate. Likewise the marginally increased temperature in the same period may have increased the cropping period and hence the actual evapotranspiration, which would have counteracted the CO2 effect. Furthermore, the model is forced by measured climate data, so possible climate changes or fluctuations should be reflected in measured precipitation and potential evapotranspiration data. The potential evapotranspiration data used after 1990 are based on Penman calculations that include radiation terms. So altogether, we argue that these effects are marginal compared to the differences we found.*

13. Section 5.3 p2395. The authors use the results of an hydrological simulation rather than observations to study extremes. But there is no validation of the model focused on extremes. I think it is important to prove specifically that the model is able to capture extremes correctly if one want to study the changes in extremes with model results.

    Response: *Yes this is an important point, and this analysis will be incorporated (see also reply to General comment #3).*

14. Section7, page 2400-2402 line 5 etc. -The authors claim that because substantial changes in the Skjern basin occurred before 1960, climate change cannot be responsible for those changes. The only justification given is more or less "The IPCC says that GHG has caused an increase in the global mean temperature after 1960 and the increase in GHG concentration was rather slow at the beginning of the period studied".

    It is not false, but one cannot conclude from that that the changes described by the authors are not the result of climate change. GHG concentration began to rise well before 1960. The IPCC does not say that GHG did not cause climate changes before 1960. The detection and attribution of the impact of GHG is easier after 1960, because the signal is larger. But it does mean that there is no impact before that. And the IPCC doesn't discuss specifically the changes in Denmark and those are the ones that are relevant for the paper. Therefore I think the authors provide no relevant elements to discard anthropogenic climate change as a potential explanation of the trends seen in the Skjern basin hydroclimate.

    Second, what about non-anthropogenic forced climate change? A positive trend is seen in global temperature in the first half of 20th century, that is reproduced when climate models are forced by both natural and anthropogenic forcings, with probably an important role of solar forcing and/or volcanic eruption.

    Response: *The discussion on the link to possible climate change drivers was deleted as the section was not essential and somewhat weak.*

15. P2404. Line 1-4. I'm not sure that simulated discharges are really fully suitable to study past climate change, given the issues of the hydrological simulation. Moreover, given the elements

provided by the authors, we don't know if precipitation and temperature series have been homogenized etc. We cannot be sure that the variability in precipitation and potential evapotranspiration is perfectly realistic.

Response: *We agree that the homogenization and quality of the input should and will be improved. Regarding the hydrological simulation please see response to Specific Comment #9.*

16. Section 8.2. p2404. Line10. I disagree with that. As far as I know, centennial climate change over Europe can be well understood in terms of a combination of anthropogenic forcing (GHG, aerosols) and natural forcing (solar variability, volcanic eruption). If it is not true for Denmark then the authors have to provide references or evidences.

    For example, they can download temperature and precipitation from historical CMIP5 simulations (multi-model and multi-members) and show that the observed trends are outside the range of what is simulated by the models. It would be a step in the good direction to justify their claim.

    Response: *The section has been deleted.*

17. P2405, line 11. Right, but one should distinguish in that context simple conceptual hydrological models from more complex ones. For the one used in this study, radiation is not taken into account. As radiation is an important driver of anthropogenic and natural long-term climate and hydrological change, it might be an issue. The discussion after that is misleading because of semantic imprecisions. One can expect that a suitable hydrological model is able to represent the impacts of climate change. Obviously it cannot reproduce direct anthropogenic influences as pumping. But line 12, the authors talk about "climate change impacts" and therefore it is not very relevant there to talk about pumping etc. Replace "climate change impacts on runoff" by something like "the future evolution of runoff" and it is OK.

    Response: *Yes this is unclear and will be corrected according to the suggestions by the reviewer.*

18. P2405, line 23. Is it not an indication that the model is too simple and does not represent correctly all the physical mechanisms that can play in a non-stationary climate?

    Response: *All hydrological modeling literature suggest that this is not the case. Many studies with intercomparison of hydrological models of different complexity and different degree of physically-based process descriptions have shown that lumped conceptual models like the NAM perform just as good as more complex models with physically more correct process descriptions if the model simulations are confined to reproducing river discharges (Refsgaard and*

Knudsen.,1996[1]; Perrin et al., 2001[2]; Reed et al., 2004[3]). Refsgaard and Knudsen (1996) for instance compared three models, of which NAM was the simplest and the physically-based MIKE SHE was the most complex on catchments in Zimbabwe. In this study the models were calibrated on wet periods and tested/validated on dry periods and vice versa. They concluded that the NAM model performed just as well or even marginally better than the much more complex MIKE SHE.

[1] Refsgaard JC, Knudsen J (1996) Operational validation and intercomparison of different types of hydrological models. Water Resources Research, 32 (7), 2189-2202.
[2] Perrin C, Michel C, Andréassian V (2001) Does a large number of parameters enhance model performance? Comparative assessment of common catchment model structures on 429 catchments. Journal of Hydrology, 242, 275-301.
[3] Reed S, Koren V, Smith M, Zhang Z, Moreda F, Seo D-J (2004) Overall distributed model intercomparison project results. Journal of Hydrology, 298, 27-60.