The authors thank anonymous Reviewer #2, for the insightful and constructive comments on the manuscript. We agree with most of the points of view expressed in the review report and we explain how we will modify the text to account for the reviewer's comments.

Reviewer #2

Main comments

Comment 1) My main concern is the link of this work with existing studies, especially the RheinBlick project that led a very similar study (though not specifically on low flows). The authors seem to use the same model and inputs, and therefore it is unclear whether their study is a specific analysis of the outputs of this project, and if it not the case, which additional insights is brought by this study. I think this should be clarified in the introduction to avoid confusion.

Reply from authors: We agree with the reviewer that the differences between our study and the Rhineblick project should be discussed in the revised manuscript.

Our study is a specific seasonality analysis of the simulated discharges using observed inputs and bias corrected outputs of 7 RCMs from the Rhineblick project dataset. The EU-FP6 ENSEMBLES project is the main data source for the regional climate change projections for the Rhineblick project and for many other climate impact studies (Bosshard, 2012; Bosshard et al., 2013; Huang et al., 2013; Hurkmans et al., 2010; Nilson et al., 2012).

As can be seen from the Rhineblick final report (Görgen et al., 2010), the project mostly focuses on the climate change impact on the magnitude of different discharge regimes, high flows in particular. The impacts on low flows are shortly discussed in chapter 6 (page 131-134). The effects of climate change on the low flow characteristics are based on the multi-annual mean magnitude change of the lowest 7-day mean discharge (NM7Q and Q90), whereas, in our study, the effects of climate change on the seasonality of low flows (Q75) are assessed based on three seasonality indices. The periods used in the two studies are also different. Most importantly, our study is not limited to simulated discharges as we assessed *also the hydrological model errors by comparing the seasonality based on observed discharges from 101 catchments and the seasonality based on the simulated discharges. It should be noted that the observed discharge data were provided by the Global Runoff Data Centre (GRDC) in Koblenz (Germany) and the Bundesamt für Umwelt (BAFU) in Bern (Switzerland). Another distinctive aspect of our study is the assessment of correlations between catchment characteristics and seasonality of low flows to identify dominant catchment characteristics for low flows in the Rhine basin. The highest correlations are found for the catchment altitude.*

In short, our study complements the recent analyses of the Rhineblick project (Görgen et al., 2010) by analysing the seasonality aspect of low flows and extends the scope further to understand the effects of hydrological model errors and climate change on three important low flow seasonality properties: regime, timing and persistence. The differences will be added to the introduction of the revised manuscript.

Specific comments

Comment 1) Abstract: As mentioned above, the possible link with existing studies could be shortly mentioned in the abstract.

Reply from authors: We agree with the reviewer and will briefly mention relevant links between our study and existing studies in the abstract of the revised manuscript.

Comment 2) Introduction: I found that the introduction could be better structured and more focused. The authors tackle various (maybe too many) aspects of climate change impact studies and do not give much details of what can be learnt from existing work. Therefore I think the authors should better explain why the impacts of climate change on the seasonality of low flows on the Rhine basin deserve more attention. Besides, I think it would be useful to clarify at this stage of the article the links or differences with existing works, especially the recent work made by the Commission for the Hydrology of the Rhine basin (RheinBlick project).

Reply from authors: We agree with the reviewer that the importance of understanding and analysing the seasonality of low flows in the Rhine basin, and major differences between our study and other studies should be detailed in the introduction of the revised manuscript.

Comment 3) 6809 - 13: “affecting a much larger area”: do the authors refer to low flows or droughts?

Reply from authors: Yes, we mean hydrological droughts causing low flows. We will revise the sentence as below.

`` In contrast, hydrological droughts, causing low flows, develop slowly and affect a much larger area than floods.``

Comment 4) 6812 - 11: It is unclear at this stage why there is this difference in the number of catchments (101 and 134).

Reply from authors: The main reason is the limited availability of discharge data. Data from 101 sub-catchments in the Rhine basin were provided from GRDC and BAFU. We will revise the sentence as below.

``Daily observed low flow series from 101 sub-catchments are available, whereas simulated low flow series from all 134 sub-catchments are available and used to assess the effects of climate change on the three indices.``

Comment 5) 6812: I think the Study area section (section 3) could be moved before section 2, since section 2 heavily uses the catchment description (especially the 134-catchment division).

Reply from authors: The authors agree with the reviewer and appreciate this suggestion. We will change the order of the two sections in the revised version of the manuscript.

Comment 6) 6812 - 25-26: I found this sentence unclear. Do the authors mean the evaluation of model errors on low-flow descriptors?

Reply from authors: The authors agree with the reviewer and the sentence will be revised as below.

``Cases 1 and 2 are compared to assess the effects of the hydrological model errors on the three indices.``

Comment 7) 6816 - 9-10: Why the number of catchments used for calibration is again different (95 instead 101)? It is unclear whether the calibration was made by the authors or in past studies.

Reply from authors: The calibration has been done by Berglöv et al.(2009). It should be noted that seven dummy catchments were created in the FEWS-NL model system only for flow routing in order to a have better simulation performance.

Comment 8) 6816 – 11-13: The authors could shortly explain why these periods were chosen. Why not using the longer period 1961-2007 for model evaluation, which is then used as the reference period?

Reply from authors: The calibration has been done by Berglöv et al. (2009)

Comment 9) 6817 - 10-12: What is the average length of available observed flow series over the 101 sub-catchments?

Reply from authors: The average length is around 27 years for the observed flow series.

Comment 10) Section 4: In this section, the authors heavily use acronyms (for low-flow indices and sub-basins). Although it can be useful to avoid repetitions, I felt sometimes a bit lost in the meaning of each of them (especially for sub-basins). Maybe the authors could use full names sometimes in the discussion to remind the meaning of acronyms.

Reply from authors: The authors agree with the reviewer and appreciate this suggestion. We will use full names instead of acronyms in the discussion part of the revised manuscript.

Comment 11) Section 4: I found that the use of “effect” in the titles of sub-sections not very clear and too vague. What the authors wish to quantify? The sources of uncertainty? The sensitivity?

Reply from authors: The authors agree with the reviewer. We will use ``sensitivity`` for the subsection titles of the results part of the revised manuscript.

Comment 12) 6821 - 8: What “significant” means here? Did the authors apply some tests? Is this comment for observed or simulated values?

Reply from authors: Yes, we assessed the correlations between catchment characteristics and the three indices to identify the dominant catchment characteristics for the seasonality of low flows. The results were not given in the manuscript for reasons of brevity. However, we will include the estimated correlation coefficient between catchment altitude and SR with a significance level of 95%.

``A significant correlation of about0.7 ($p < 0.05$) between *SR* and catchment altitude is found for the 101 catchments as catchments with a higher altitude tend to have winter low flows and higher *SR* values.``

Comment 13) 6822 - 7: Was only the station at the outlet of each of the seven sub-basins considered, or did the authors made some averaging on all the stations within each sub-basin?

Reply from authors: Only the outlet discharge was considered as this is an integral property of all catchment discharges. It is assumed that this approach yields a better estimation of subbasin seasonality indices.

Comment 14) 6822 - 27: Why this period was chosen as the reference period. 1961-1990 is often considered as a reference period in the climate community.

Reply from authors: The current period 1961-2007 is selected arbitrarily in our study based on available observed data. We agree that 1961-1990 is often used in the climate community as reference period. However, there are also other examples of selected reference periods.

For example, the reference period for NASA-GISS is 1951-1980, for HadCRU 1961-1990, for University of Alabama in Hunstville (UAH) 1981-2010 (which is also WMO standard) and for NOAA-NCDC 1971-2000.

Comment 15) 6823 - 6-9: I found this comment a bit strange. How can the author say that the low flows are “well simulated” since they only compare simulations with other simulations here? Maybe they should more clearly say that the simulations obtained with simulated inputs are close to those obtained with observed inputs. Then, only if the model simulations using observed inputs were shown to be accurate when evaluated against flow observations, their conclusion would be valid.

Reply from authors: We agree with the reviewer. The authors appreciate the insightful suggestion and will incorporate it in the revised version of the manuscript.

Comment 16) 6826 - 19: “Representative” of what?

Reply from authors: We refer to climate change uncertainty. We will revise the text as below.

“If these seven climate scenarios are representative for the uncertainty in climate change, it appears from Figure 6 that the GCM/RCM uncertainty has the largest influence on *WP*.”

Comment 17) Discussion and conclusion: I found that the authors could push a bit further the information they give on the comparisons of their various model runs. They show that there is some level of error induced by the hydrological model on the reference period, and then they show that some trends on low flow indices between reference and future periods are found. However, they do not discuss the relative importance of the noise induced by the model on the reference period (delta between observed and simulated) and the delta change they show between simulations in present and future conditions. Can the delta change between present and future be considered significant compared to the noise in present conditions? If the errors in current conditions are much larger than the change between present and future, can this change be considered significant? Does this depend on the sub-basin and/or the low-flow index? Although this question goes probably a bit beyond the objectives of this article, I think it would be interesting to discuss this question to make a better link between the results shown by the authors.

Reply from authors: The authors appreciate these insightful suggestions for the discussion and conclusion parts.

In this study, the errors induced by the hydrological model and observed inputs were not explicitly assessed and; therefore, not discussed in detail. Further, the measurement errors in the observed discharges and the effect of different data lengths for the observed discharge series were only implicitly addressed in the manuscript. We have run the hydrological model using observed inputs and the bias-corrected outputs from an ensemble of seven climate scenarios. The good simulation performance of the hydrological model is our main justification to use its outputs as reference for comparison of current and future conditions. The comparison of Table 3 and revised Table 4 in the revised version of the manuscript will help readers understand more clearly that the delta change between present and future conditions is significant. As discussed in the manuscript, the delta change varies between the sub-basins (e.g. Figure 5). Moreover, the delta change also depends on the seasonality index.

We will incorporate this paragraph in the discussion section of the revised manuscript.

Comment 18) 6827 - 18: What the "sixteen experiments" refer to? Model runs?

Reply from authors: Yes, we refer to model runs. We will revise the text as below.

``Sixteen model runs were considered.``

Comment 19) Table 1: Indicate the target periods for the first two lines.

Reply from authors: We will add information about the periods in Table 1 in the revised version of the manuscript.

Comment 19) Tables 3 and 4: These tables could be grouped to ease comparison. Or at least sub-basins could be put in columns in the two tables to ease comparison.

Reply from authors: The authors agree with the reviewer. We will present the seven subbasins in columns in Table 4 to ease comparison.

Comment 20) Fig. 3: The legend on the maps is not legible (too small).

Reply from authors: We will increase the font size of the legend in Figure 3 in the revised version of the manuscript.

References:

Berglöv G, German J, Gustavsson H, Harbman U, Johansson B. 2009. Improvement HBV Model Rhine in FEWS, Final Report, SMHI Hydrology, *Tech.* . Norrköping, Sweden.

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