Hydrol. Earth Syst. Sci. Discuss., 11, 6273-6309, 2014

Climate change impacts on the seasonality and generation processes of floods in catchments with mixed snowmelt/rainfall regimes: projections and uncertainties

K. Vormoor, D. Lawrence, M. Heistermann, A. Bronstert

Final response to referee #3:

We want to thank referee #3 for her/his very detailed comments on our manuscript. Please find below our replies referring to each of her/his points. For convenience, the comments by the referee are repeated in *gray-italic*. Text designated for inclusion in a revised manuscript is given in blue:

GENERAL REMARKS:

1.) *It is well known that there will be a shift in flood seasonality due to climate warming, from snow dominating floods to more rain controlling floods in regions with a seasonal snow cover and accordingly, a change in controlling processes (e.g. J. Parajka, 2010). However, as also mentioned in the paper, precipitation is projected to increase in the region as already documented, particularly on the western coast. Thus, it is important to account also for changes in seasonal precipitation when discussing changes in flood seasonality. The paper briefly mentions this aspect, however, it is recommended that is also include a quantitative analysis of changing (seasonal) precipitation and temperature pattern to better distinguish the relative importance of increasing temperature versus changes in precipitation.*

The following Figure shows the estimated changes in mean monthly temperature and mean monthly precipitation sums from the locally adjusted RCM projections used in our study. The results basically confirms findings from other studies and reports: The temperature projections for the six study catchments indicate a larger warming in winter than in summer which agrees with Engen-Skaugen et al. (2007), Hanssen-Bauer et al. (2003) and Hanssen-Bauer et al. (2009). They further indicate that the warming signal is increasing with larger distances in latitudinal and longitudinal direction (Hanssen-Bauer et al., 2003; Engen-Skaugen et al., 2007). Regarding the projected changes in precipitation, the results correspond to the regional differences in seasonal precipitation change as shown in Hanssen-Bauer et al. (2009).

We may consider including the Figure and its discussion in a revised manuscript. However, given the length of our manuscript, we may just extent the introduction by this information and discuss our results more explicitly in the light of these projections. Please also note that flood events are the focus of this study. Changes in mean precipitation sums will probably not completely serve to consider the effects of precipitation on floods. Only in using a hydrological model, we are able to consider the relevance of precipitation and temperature changes for changes in the seasonal occurrence and generation types of floods.

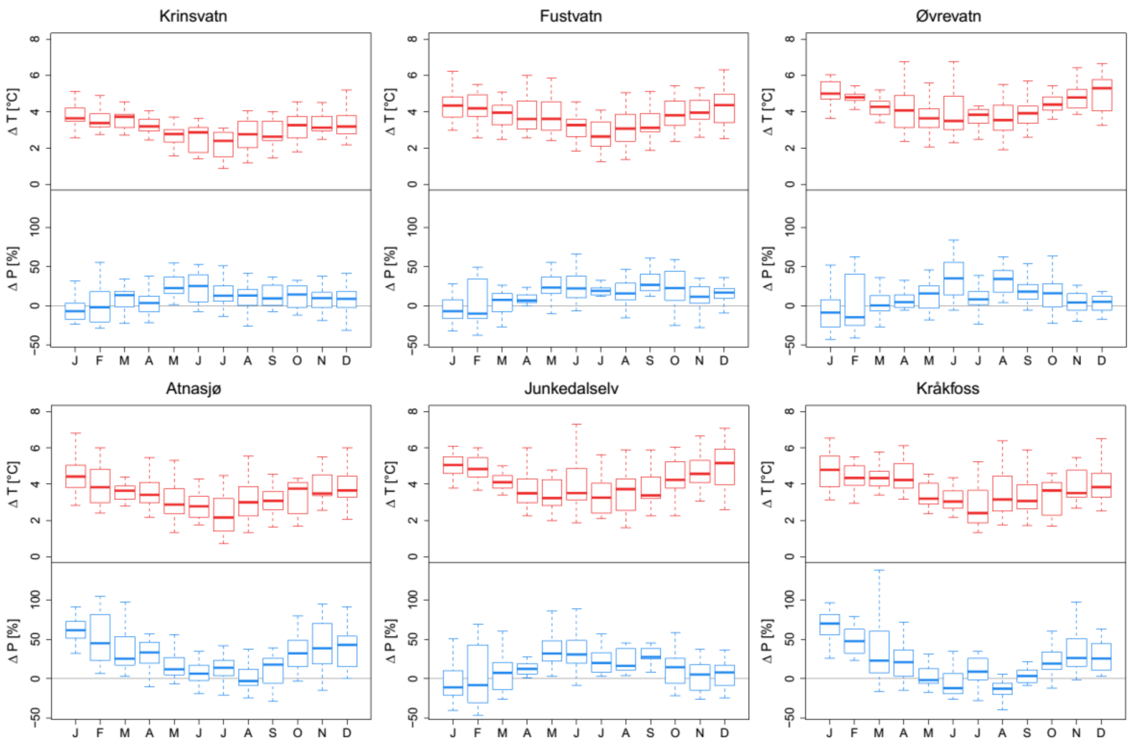

Figure 1: Projected changes in monthly mean temperature and monthly mean precipitation sums estimated by the locally adjusted RCM projections used in our study

It would further have been of interest to assess any trends in the observed period and compare these with future predictions.

For a better introduction of observed and projected changes in temperature and precipitation, we suggest splitting the first paragraph of the introduction into two paragraphs: the first will be on observed trends in (extreme) streamflow (see Specific Comment a) connected with observed changes in the meteorological triggers. The second paragraph will be on projected changes in temperature and precipitation regimes (as outlined above) and their implications for the snow regime. The latter will be closely connected to our revised discussion on the changes in the FGPs (section 4.4).

2.) The use of the AR4 scenarios rather than the CMIP5, makes the study somewhat outdated (although the main conclusions may not change that much).

Downscaled projections of CMIP5 covering the whole of Norway are only becoming available since the beginning of 2014. The design and the analyses of the study started already before these data were available. Moreover, as mentioned by the reviewer him-/herself, it probably would not affect the main conclusions.

3.) *The use of only one (conceptual based and calibrated) model in (what is likely) a non-stationary climate should be commented on, and more general, the role of hydrological model uncertainty in climate change impact studies (e.g. Velázquez et al., 2013; Bosshard, et al., 2012).*

We agree that it is necessary to comment on the issue of calibrating the HBV model under non-stationary conditions during the reference period. We used long (and indeed, probably non-stationary) calibration periods to ensure that a large variety of hydro-meteorological condition is captured so that all relevant process and parameters are covered. That implies better chances to detect parameter sets which are suitable for a range of possible conditions (Merz et al., 2009). So, calibrating hydrological model parameters under non-stationary conditions does not necessarily imply non-stationary parameters. Calibrating the model for sub-periods which are similar to future conditions may nevertheless lead to more specialized parameter sets. We will discuss this in some more detail in a revised manuscript. Please see also our replies to referee #1 and #2. In our reply to referee #1, we mention the suggested changes in a revised manuscript.

Regarding the use of only one hydrological model in the ensemble approach, we would like to refer to Velázquez et al. (2013), and suggest adding the following note to section 3.1. "Modeling strategy": It has become good practice to include more than one model for each member within the model chain to derive a range of possible projections and to allow for drawing conclusions about the uncertainty that is associated by such approaches. We only used one hydrological model in our ensemble setup since Velázquez et al. (2013) conclude that the use of multiple hydrological models in climate impact studies has rather an implication for the study of low flows and means; for high flows, various lumped and distributed models led to very similar results.

Moreover, the HBV model is extensively tested for Norway and applied by the operational national flood forecasting service at the Norwegian Water and Resources and Energy Directorate (NVE); typical runoff generation processes for the Nordic countries are well represented in the conceptual framework of the model. We will have a note on that in section 3.4.

4.) The use of only six catchments and their location. It is noteworthy that the selection does not include a catchment in western Norway, which is specifically mentioned as an area of interest due to high precipitation rates (ref. Introduction). This is also a region where precipitation is projected to increase significantly in the future (and already has).

We tried to find catchments with similar size and a comparable good data basis for applying the HBV model on a daily resolution. Some catchments in the very west of Norway are too small and too fast reacting for reasonably applying the model on a daily resolution. Moreover, the motivation was to study the changes in the seasonality of floods and their generation processes in catchments, which currently have mixed rainfall-snowmelt regimes. Most catchments in the very west of Norway are solely pluvial catchments. We will add a remark in the revised manuscript which will explain why we did not consider any catchment from the very west of Norway.

5.) The topic of the study lends itself to a regional study and six catchments is a rather low number given the high hydroclimatic variability across Norway. Only with a better coverage can one conclude on regional patterns and trends in flood patterns (in the current as well as future climate), as these can vary considerable locally. This can be achieved either by increasing the number of catchments or by using a gridded dataset for Norway (e.g. data from seNorge.no, which contains both interpolated climate and simulated runoff based on a gridded version of HBV). The current study design is in my opinion not sufficient to conclude on regional patterns in flood seasonality (refer Objective 1). Accordingly (provided that the study is not extended), the conclusions must be revised to be more catchment specific and less general.

On the one hand, it is an open question how many catchments are appropriate to allow for drawing regional conclusions. We chose catchments which represent a high variability of hydrometeorological conditions across Norway. Actually, the six study catchments represent three out of five hydrological regions in the Nordic countries as suggested by Tollan (1975) and Gottschalk et al. (1979) (see also Specific Comment c). On the other hand, we agree with the referee and will change our conclusions to be more catchment specific and less general. We will replace “different regions” by “catchments representing different regions”.

6.) Objective 3 can only be answered if the role of changing precipitation and temperature patterns are included explicitly (ref. point 1 above).

Note that the FGPs are defined by runoff components, which are simulated by the HBV model. In that way, we implicitly include the role changing precipitation and temperature patterns projected by the locally adjusted RCMs in their relevance for the generation processes of floods. Please see also our response to point 1 above.

7.) *When objective 4 is presented, we have not yet been informed about the different ensemble components. The latter aspect needs to be better introduced, including the design of the modelling strategy. Section 3.1. says what it consists of, but not why this particular design was chosen. Perhaps it is partly what is said on p.6286, line 10: “identify the fractional uncertainty emerging from different sources within the model chain for three variables…”.*

We completely agree and will add this information to the introduction. Note, however, that we already mention in the introduction why we use such a multi-model/multi-parameter design (p.6276, lines 21-23). The different ensemble components will be introduced after p.6276, line 23 as: The multi-model/multi-parameter ensemble used here consists of eight combinations of global and regional climate models (GCM/RCM combinations), two methods for locally adjusting the climate model output data to the catchment scale, and the HBV hydrological model with 25 calibrated parameter sets.

SPECIFIC COMMENTS

a) The introduction gives reference to various trend studies (in observations), but not to particular studies on trends in floods, which should be added.

We will add a reference on trend studies for seasonal floods in the introduction, and suggest adding the following sentence to the first paragraph: Regarding flood seasonality, neither significant trends towards higher autumn floods as a result of increasing autumn rainfall, nor systematic trends in spring flood magnitudes could be detected, yet (Wilson et al., 2010). There is, however, a strong trend towards earlier spring floods at many stations due to an observed increase in mean annual temperature by 0.8°C during the last century with the strongest decadal temperature during the spring season (Hanssen-Bauer et al. 2009).

b) The result of the paper should be discussed in light of similar studies, and not be limited to national (or Nordic) studies. Also pan-European trend studies would be of interest as well as studies from similar regions in other continents (e.g. U.S. and Canada).

Accepted. We will refer in a revised manuscript on comparable findings from a pan-European study, and from studies for different regions in the Alps and North America. Note, however, that we will only refer to studies with regions of similar processes and scales since we doubt that large scale modeling studies can reflect the processes in similar detail as shown in our and other catchment specific studies.

c) *Reference should be made to existing regime classifications for Norway (here only two regimes classes are suggested). Other regime classifications distinguish more classes and could also be used as a starting point for selecting representative catchments.*

We will add a section on the classification of hydrological regimes in the Nordic countries (Tollan 1975, Gottschalk et al. 1979) and discuss that in the context of our simplified discretization of flood regimes in section 2.1. We suggest adding to p.6277, l.23: A comprehensive classification of runoff regimes based on the seasonal occurrence of monthly high- and low flows is given by Tollan (1975), and reviewed in (Gottschalk et al., 1979). This classification defines five types of flood regimes for the Nordic countries with a detailed distinction between all possible combinations of high water and low water periods. However, in order to develop a broad picture of flood seasonality, it is most useful to apply the simple distinction between two basic seasons and rainfall vs. snowmelt as the most fundamental flood generation processes in Norway.

d) Clarification on the seasonality index, S_D:

i. *Why is the second term in the index included (does it add any information)?*

We also included the second term for a better readability of the index. We think that an index ranging from -1 to +1 with 0 as the center point is more intuitive than an index ranging from 0 to 1.

ii. *The first term describes the ratio between the flood peaks in m-3s-1; does this mean that you sum the POT discharge values?*

Yes, we sum up all detected peak discharge values (we will clarify this in the manuscript). We also performed this analysis with the number of events, which led to very similar results.

iii. *Is it valid to use the same two seasons for all catchments given their high variability in hydroclimatic regime (and will they be representative in the future)?*

The distinction between the seasons for S_D is very coarse. The variability in seasons is supposed to be larger for the current (reference) climate than in the projected future climate. Since the classification holds for the current situation it is likely that this will also hold for future conditions.

iv. *How will the use of a fixed threshold (here the 98.5 streamflow percentile) influence the selection of events if there is a change in annual precipitation (and thus streamflow) in the future?*

We agree that this is not clear enough in the current version of our manuscript. We did not use a fixed threshold for both the reference and future period but rather a flexible threshold for each period. Thus, we will extent the sentence on p.6284, line 10: The threshold was set to the 98.5 streamflow percentile for both the control and future period time series.

v. *How is the normal flood duration defined? Is there a different value for snow generated events as compared to rainfall (different response times)?*

We agree that this needs further explanation. So, we suggest adding this information to section 3.5.: The normal flood duration has been derived for the six catchments considered by a simple experiment using the HBV model: each catchment was artificially drained to baseflow conditions before twice the amount of annual rainfall was added to completely saturate the catchment again. Concentration and recession time to baseflow was estimated from the resulting hydrographs; concentration and recession time together give the normal flood duration.

Consequently, there are no different values for snowmelt- or rainfall-generated events. The ’normal flood duration’ refers to the maximum temporal extent of a flood in a certain catchment independent from its generation process.

vi. *Present and argue for your proposed seasonality index in light of existing definitions (e.g. J. Parajka, 2010).*

This comment also refers to Specific Comments j and t. Actually, we are applying the same seasonality measures as shown in other studies (e.g. Parajka et al. (2010)) for generating the results shown in Figure 6. Our apologies that this was not made clear in our manuscript. Consequently, the method- and results & discussion- chapters require strong modifications. We will add a paragraph to section 3.5.2 ‘Changes in FGP’ where we will illustrate in a better way the statistics we have used (including the directional statistics which are meant here), and we will give references to original work (Bayliss and Jones, 1993; Burn, 1997). The results based on directional statistics (Figure 6) will be better introduced and explained as postulated in Specific Comment t. We suggest adding the following paragraph to section 3.5.2 ‘Changes in FGPs’ (for the changes in the result section it is referred to Specific Comment t):

Two statistics were applied to show changes in the FGPs: (1) The ratios of rainfall-, rainfall+snowmelt- and snowmelt-generated events relative to all events for all ensemble realizations were estimated for the reference and future period. The change in the ratios indicates the changes in the prevalence of the different FGPs. (2) Circular kernel density functions and the circular mean Julian date of occurrence of the rainfall-, rainfall+snowmelt- and snowmelt-generated events were calculated for both periods to illustrate changes in the annual distribution and mean timing of the events. The Julian mean dates of occurrence for the events with respect to each FGP are converted to mean radians ($\overline{\Theta}$) estimated from the Julian date of occurrence D for each event i:

$$\Theta_i = \frac{D2\pi}{365} \quad (5)$$

where the Julian date D = 1 is for January 1st and D = 365 for December 31st. The $\overline{x}$ - and $\overline{y}$ -coordinates for the mean date as an angular value is derived from the sample of n events for each FGP group:

$$\overline{x} = \frac{1}{n}\sum_{i=1}^{n} \cos\Theta_i \quad (6a)$$

$$\overline{y} = \frac{1}{n}\sum_{i=1}^{n} \sin\Theta_i \quad (6b)$$

$$\overline{\Theta} = \tan^{-1}\left(\frac{\overline{y}}{\overline{x}}\right) \quad (7)$$

This approach was introduced by Bayliss and Jones (1993) and Burn (1997), and has been recently applied by Parajka et al. (2010) and Köplin et al. (2014). Note that these authors also estimate the variability of the date of occurrence. In this study, this is illustrated by the circular kernel density functions.

e) *The classification into three flood generation types is based on the contribution of rain and snow to the runoff. What about rain on snow events; how would these be classified based on the HBV model simulations?*

The classification of the FGPs is defined by the percentage of runoff components during the flood event. These are defined by the proposed balance approach. Events are classified as 'snowmelt', 'rainfall', and 'rainfall+snowmelt' events. Correspondingly, rain on snow events are implicitly included by this definition.

f) *Combining the result and discussion section can be challenging. Here, the results are discussed under specific headings, which is fine. However, this requires an overall discussion bridging between the different sections (option to add such a section at the end of the combined section).*

We would prefer to stay by our proposed narrative style in a revised manuscript. Adding a section with overall discussion would first of all lead to repetitions of what was discussed in the sub-chapters before. In the results and discussion chapter, we move from the detection of changes in flood seasonality to the reasons for these changes and finally to its underlying causes. The results are consecutively discussed under specific headings and set into the context of existing literature. In our revised discussion of Figure 6 (see Specific Comment t), we will be able to explain the changes illustrated in the previous sections and connect them to one picture. The bridge between the different sections has been given in the conclusion chapter.

g) *It is concluded that the relative role of hydrological parameter uncertainty is highest in catchments showing a high change in flood seasonality. Is this not just a result of high model sensitivity to the threshold temperature (snow/rain and melt/no melt), implying a widely different response in runoff to small changes in temperature?*

The HBV model parameters which tend to be most sensitive are not the parameters that are directly related to the snow routine of the model (TS – temperature for no melt; TX – temperature for snow, ice) but rather the precipitation and snow correction factors (PKORR, SKORR) (Lawrence and Haddeland, 2011). Moreover, the model is trained in a period with regular snowmelt seasons. All snow related processes should therefore be represented well in the relevant catchments.

h) *The abstract needs to better represent details of the study, e.g. number of catchments, multi-model in what sense, what are the ensemble components?*

We agree. The abstract will be extended with this information. We suggest extending the sentence in line 4ff.: Using a multi-model/multi-parameter approach to simulate daily discharge for a reference (1961-1990) and future (2071-2099) period, we analysed the projected changes in flood seasonality and its underlying generation processes in six catchments with mixed snowmelt/rainfall regime in Norway. The multi-model/multi-parameter ensemble consists of (i) eight combinations of global and regional climate models, (ii) two methods for adjusting the climate model output to the catchment scale, and (iii) one conceptual hydrological model with 25 calibrated parameter sets.

i) *The abstract reads "Changes towards more dominant autumn/winter events correspond to an increasing relevance of rainfall as a flood generating process (FGP) which is most pronounced* *in those catchments with the largest shifts in flood seasonality. Here, rainfall replaces snowmelt as the dominant FGP". Later it is stated (Section 4.4) "Rainfall becomes the dominant FGP in the future period in all investigated catchments". There is here a need to distinguish the relative contribution of a precipitation increase (rain or snow) vs. a shift in precipitation from snow to rain due to a temperature increase. In other words; what is the role of increasing temperature vs. changes in precipitation patters for the different catchments (should be evaluated on a seasonal basis). Ref. point 1 under General comments.*

We argue that we consider the relative role of increasing temperature vs. changes in precipitation patterns through the use of the hydrological model. The results shown in Figure 5 and Figure 6 can be directly linked to projected changes in the temperature and precipitation regime in Norway, which have already been investigated by other authors (e.g. Hanssen-Bauer et al. 2003, 2009; Engen-Skaugen et al. 2007). We will point out this linkage more clearly in the discussion of the revised manuscript (please see also our reply to General Comment 1).

j) An important observation, although a bit hidden, is given in Section 4.4, p.6290, line 21: "the rainfall-generated POT events tend to occur later in the year". This should be further elaborated and possible reasons discussed.

We will revise that part of section 4.4. and combine this with a better introduction of the methods and the results shown in Figure 6. Please see our responses to Specific Comment d-*iv* and t where we answer on that issue in more detail.

k) It is argued that the selection of only two classes is chosen to obtain a broad picture of flood seasonality. Why not simply look at changes in the flow regimes, i.e. changes in the month of the highest peak? This would allow you to analyse a more general shift in flood occurrence, not restricted by the choice of a fixed season (temporal as well as spatially).

We were looking for a simple classification of dominant flood seasonality for catchments with mixed snowmelt-rainfall regimes. S_D is intuitive (since the two seasons are associated with dominant flood generating processes), it is easy to apply (also easy to use for a geographical extension of the study), and we believe it is well suited for the scope of the study. Moreover, we show more general shifts in flood occurrences in the discussion of Figure 6 (section 4.4) where we illustrate the change in the mean annual timing of floods separated by the FGPs.

l) It is mentioned that the HBV snow and melting module has a semidistributed structure. More details are here needed as the formulation of the snow routine is vital for the study, e.g. what is the spatial resolution of the elevation zones, how is the climate input interpolated to different elevation zones, how is snow melt calculated?

Considered. We will give more details on the structure and input data of/to the 'Nordic' version of the HBV model in a revised manuscript. So, we suggest rephrasing the first paragraph in section 3.4 (p.6283, lines 4-10): In this study we apply the "Nordic" version of the model (Sælthun, 1996), which incorporates a snow module with ten equal area height zones, such that snow accumulation and melting has a semi-distributed structure. For each equal area height zone, the accumulation and melting of snow is calculated individually, and the mean is finally used to represent the snow dynamics for each catchment. The principal advantage of the HBV model relative to more physically-based models are that it only requires precipitation and temperature as climatological input. These are given as catchment mean values for the catchment centroid. Input data for precipitation and temperature is modified for the snow routine by three parameters defining the precipitation altitude gradient, and the temperature gradients for dry and wet days, respectively.

m) *Is the RCM downscaled to the scale of the catchment area or to a gridded structure? and how is the climate input distributed to the different elevation zones? More details needed.*

The RCM data is downscaled to the scale of catchment area. Observed meteorological input data to the HBV model is given as one value for both temperature and precipitation for the centroid of each catchment. These values are inferred from interpolated 1x1 km observation data (the seNorge data). For how the climate input is distributed to the equal area elevation zones, please see also our response to Specific Comment l.

n) *The reference to 'equifinality' should be deleted as I cannot see that the work specifically addresses this aspect; instead focus should be on parameter uncertainty only.*

We agree and will delete 'equifinality'.

o) *The last paragraph of Section 3.5.2 is not clear. What is the 'flood duration time of the core event' and what implication does it have that the duration is extended by adding 'the catchment specific recession time'.*

We agree that this needs further explanation. Therefore, we suggest extending this paragraph (p.6285, lines 15-18):

Events were detected using a tool implemented in the R add-on package 'seriesdist' (Francke & Heistermann (2014) [https://bitbucket.org/heisterm/seriesdist]) which allows for detecting both flood peaks and their event-specific flood duration. In order to also account for the antecedent conditions in the catchment, the detected flood duration time of the core event was extended by adding the catchment specific recession time (found in the definition of the 'normal flood duration') before the onset of the core flood. The classification approach was then applied to the extended flood duration time. This way, we made sure that all flood contributing components are considered.

p) *Section 4.3 is important, but the approach (changes in magnitudes vs. the frequency of events) has not been well introduced in the Method section.*

We agree and suggest adding the following paragraph to the section 3.5.1. (p.6285, ll.5 ff.) to better introduce the approach on changes in magnitudes vs. frequencies: In addition, the magnitudes and frequencies of the detected spring/summer and autumn/winter events were analyzed for the reference and the future period. The changes in magnitudes and relative frequencies of the events within each season can help to explain changes in flood seasonality.

q) *Figure 2: comment also on the spread, not only on the median.*

In the revised manuscript, we will extend the paragraph on p.6287, lines 7-15. The new paragraph will read as follows: As expected, the absolute range and the interquartile range of the POT event distribution from the full ensemble are larger. This is mainly the result from the large range introduced by the locally adjusted climate projections (see the 4th and 5th box in each plot). In four catchments the quartiles match the observed distribution fairly well (Krinsvatn, Øvrevatn, Atnasjø, Kråkfoss). The largest discrepancies occur for Fustvatn and Junkerdalselv. In both cases, the mismatch of the ensemble reflects the overestimation (Fustvatn) and underestimation (Junkerdalselv) resulting from the different LAMs. Still, the observed distributions of POT events are always captured by the full range of the ensemble and the data locally adjusted by EQM and XDS. The performance of the ensemble in reproducing the observed POT events is the only indicator we have of how reliable the ensemble is for future projections. For Fustvatn and Junkerdalselv, that implies a smaller reliability of the future projections compared to the remaining catchments.

r) Figure 4: add the observations to the seasonal plot.

The distributions of the observed POT events (though not divided by seasons) are already given in Figure 2. We think that adding the distributions of observed POT events to Figure 4 would overload the plot. Please note, however, that the median of the observed POT events are given for each catchment by the green bars in each plot. Please also note that we will modify the plot for a better readability referring to a comment by referee #1 (Minor Comment 9).

s) Figure 5: Is this result based on an average across the model ensemble for all 25 parameter sets?

No, the pie charts represent the total number of events by the entire ensemble. We will clarify this in the figure caption. A modified version of this figure will also show the total number of observations and the percentage change of these for the future period (please see modified version of Figure 5 in our reply to referee #1).

t) Figure 6 needs a better introduction (hard to read and not well explained). Difficult to understand the text that follows (p.6291, line12-20), and this section needs revision.

This comment is linked to Specific Comments d-vi) and j). We completely agree that a better introduction and explanation of this figure will be helpful. Also the methods which were used to derive the results shown in Figure 6 will need a detailed introduction. Therefore, besides adding a paragraph to the method section, we will revise the entire part of section 4.4. where we are discussing the results shown in Figure 6. Since Figure 6 summarizes many aspects of what has been shown in the previous sub-chapters, this will allow us to give a summarizing discussion as recommended by Specific Comment f.

TECHNICAL CORRECTIONS

i. P.6275 (line 21). The reference by Lawrence and Hisdal (2011) cite change in flood frequency, then refers to flood magnitudes; please clarify.

If the magnitude of let's say a 100 year flood increases in the future, this implies that the frequency of a given flood magnitude increases, too. For convenience, though, we suggest replacing p.6275, line21 by: Flood magnitudes of the 200-year flood are likely to increase in catchments in western and much of coastal Norway where…

ii. P.6227, line 17: rewrite as i. reads like snowmelt in inland and northernmost Norway causes high flow s during spring and summer in the whole of Norway (similar for ii.).

Thank you, we suggest to rewrite this and the following sentence by: (i) regions in inland and northernmost Norway with prominent high flows during spring and summer predominantly due to snowmelt, and (ii) regions in western Norway and in coastal regions with prominent high flows during autumn and winter predominantly due to rainfall.

iii. P.6282, line 6: 'this approach performs remarkable well'; provide details of what performs well and where.

We suggest adding the following information to p.6282, line 6: … following work of Piani et al. (2009) which illustrated that *the correction without seasonal subsampling* performs remarkably well.

iv. *Overall use comma more (particular to distinguish between the use of 'that' and 'which').*

We will re-check the use of comma throughout the text.

v. *Suggest to replace the word 'mismatch' when discussing model performance with something more informative, e.g. underestimation, …*

Considered. We will clarify the direction of the mismatches in a revised manuscript.

vi. *P.6287, line 21: Sentence starting: "For Fustvatn", is this the correct catchment here?*

Yes, this is the correct catchment. The information provided here refers to the three boxes on the right.

vii. *P.6293, line 4: replace "different regions" with "six catchments representing different …".*

We will rephrase this paragraph as follows: Using a multi-model/multi-parameter ensemble approach, the impacts of climate change on flood seasonality and their underlying flood generating processes (FGPs) have been investigated in six catchments representing different hydroclimatological regions in Norway. Furthermore, we will be more catchment specific in our conclusions throughout the text, as suggested by General Remark 5.

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