Potsdam, January 13, 2015

Dear Editor,

We wish to thank you for your comments and decision regarding the first revised version of our manuscript. We have now uploaded another revision of our paper which addresses all the suggestions you have given in your Editor Decision.

In the introduction, we have added another sentence which underlines the motivation for our study. Moreover, we have extended the discussion on hydrological model parameter (non)stationarity and its implications for the reliability/uncertainty of our results, which also addresses many of the concerns raised by Reviewer 3. Finally, we have highlighted in the conclusions that our sample size, consisting of six catchments, is too small to allow for robust regional conclusions. However, we have pointed out two possibilities to derive more general results.

At the end of this letter, we have attached our point-by-point response to the comments of Reviewer 3. We very much appreciate the efforts you have made in handling and reviewing our manuscript and we hope that it now meets the requirements for publication in HESS.

On behalf of all co-authors,

Klaus Vormoor

Point-by-point response to Reviewer 3

Thank you for providing further suggestions for the improvement of our manuscript and for your very detailed comments during the interactive discussion and thereafter. In the following, we will address each of your points and highlight the changes we have made in our manuscript.

It is well known that a warming climate impose a change in flood seasonality, and the authors are recommended to better argue the motivation of their particular study and approach with reference to the current knowledge base, see e.g. "Understanding flood regime changes in Europe: a state of the art assessment", published in HESS (Hall et al., 2014).

We have added a sentence to the introduction where we highlight our motivation. We also refer now to the suggested reference.

In the abstract is says "rainfall replaces snowmelt as the dominant FGP"; please add the role of increasing precipitation versus increasing temperature (see also comment (1) below).

We have added that this shift is dominantly due increasing temperature.

Hydrological modelling and stationarity of model parameters: it is in this respect argued that the use of a long calibration period increases the chance that all relevant processes are covered (Merz et al., 2009). However, as stated in Merz et al. (2011), the assumption of stationarity of model parameters and model structure might constitute an important oversimplification (cited in Hall et al., 2014). This point requires further elaboration

We agree and have extended the discussion on that issue in section 4.6.

The choice of reference period (1961-1990), implies that the pronounced warming in recent years is not included in your 'current climate'. One advantage is that it may be easier to obtain robust parameters by calibration as it represents a 'more' stationary period; however, it will be less representative of the future climate. Please comment.

Considered. Together with the changes stated above, we have pointed that out in our revised manuscript.

It is argued that focusing on the far future is preferred as the signals are more pronounced by then. However, changing flood seasonality can already be observed in many countries and e.g. hydropower companies in Norway are already adapting to a changing climate. Thus, I do not find this a good argument alone.

Another argument for focusing on the far future period is that the catchments considered in this study show mixed regimes during the current climate, however to different degrees (see also your following point). It is likely that changes in the seasonality and FGPs in the northern catchments (Øvrevatn and Junkerdalselv) will occur later than in those catchments where this regime shift is already occurring. For the northern catchments we would only have small change signals in a near future period. By using multiple future periods, it would of course be interesting to study the temporal transience of the catchments. However, this would also overload the paper.

It is argued that the catchments chosen are mixed snowmelt/rainfall flood regimes (ref title). On p. 5, line 25, it is further stated "focus is on catchments which already exhibit some tendency for both snowmelt and rainfall-dominated flood regimes. However, from Figure 1 there seems to be three mixed catchments, one snowmelt dominating (Øvrevatn) and two (Atnasjø and Junkerdalselv) typically snowmelt flood regimes. Please comment.

The flood roses in Fig. 1 show the Block Maxima for the six study catchments; i.e. the observed Annual Maximum Floods (AMFs). Atnasjø and Junkerdalselv which appear to be catchments with typical snowmelt flood regimes often show secondary high flows during late summer and autumn. This is also the reason why we applied a POT approach in our analysis instead of AMFs. However, it is true that the degree of "mixed" differs between the catchments. We have added a note to the figure caption.

Ref: Comments to the reply to reviewers comment

You state (1) that only by using a hydrological model, one is able to consider the relevance of precipitation and temperature changes for changes in the seasonal occurrence and generation types of floods. In your work, you do this by looking at model simulations (ref. reply to (6)), excluding the climate forcing itself (although the inclusion of Figure 3 is very helpful in this respect – would have been great to learn how the proportion of snow/rainfall in the catchment is projected to change). Can you trust that the model gives you the correct answer in this case? What if the threshold temperature is not correctly simulated and precipitation falls as rain (in the model) and as snow (in reality); implying that the modelled snow storage is smaller than observed, giving less weight to a temperature increase (in case of flood generation)? Refer also comment (i) in the original review. This being said, I agree that changes in flood seasonality cannot be directly inferred from seasonal changes in climate, due to the role of snow storage.

We extended the discussion on hydrological model parameter instability in section 4.6., and we now also indicate its implications for the reliability of our results for the future period. We cannot calibrate parameters for the future period and consequently, we cannot fully trust the model's answer regarding the proportion of snow/rainfall in the future, especially given the non-stationarity of the model paramters. Thus, we need to focus on improved calibration approaches which can handle this issue. We have sketched one opportunity in section 4.6.

In (3) you refer to the study by Velázques et al. (2013) for support in using only one hydrological model. However, this study is only based on two catchments, and further states that the "generalisation of this conclusion would require application to more sites and should include other sources of uncertainty (e.g., calibration of hydrological models or use of different GCMs and RCMs)".

Another reason for applying the HBV model is that it has been widely and successfully applied in Norway and other countries showing similar hydroclimatological and physical conditions. We have added a note on that.

Ad (4), your argument here fails as it refers to catchments on the very west of Norway. The comment related to the lack of catchments in Western Norway, including mixed and snow dominated catchments.

This was a misunderstanding. We agree, that there exist catchments somewhat inland in Western Norway (referring to the Norwegian term 'Vestlandet'), which also show mixed regimes during the current climate.

Ad (5); six catchments are not sufficient to draw regional conclusions, particular due to the high local variability in catchment processes. This is independent of the number of regions (or regime classifications) that are covered, ref. e.g. the difficulties in explaining the low performance of the Junkerdalselv catchment.

We have pointed out more clearly in the conclusions that the study does not allow for robust regional conclusions.

Ad (b): it is stated (line 150) that the manuscript will be revised to include comparable findings from a pan-European study, and from studies for different regions in the Alps and North America. This is done to some degree, but still some key references are missing like Hall et al. (2014) and references therein, see above.

We do now also refer to Hall et al. 2014 and the reference list given therein.

Ad (c): it is stated that "it is most useful to apply a simple distinction between two basic seasons …" (see also paper p.5, line 17-19). Why, when focus is on transitional regimes and when it is argued to introduce a rainfall+snowmelt class of flood events? Further, the seasons will likely have a different definition in a future climate (notable on the long term which is the focus in this study).

This simple distinction allows for sorting flood events in spring/summer- and autumn/winter floods – which is a simple classification associated with the most dominant flood generating processes (snowmelt vs. rainfall). The simple distinction allows us to formulate the S_D-index. We agree that there might be shifts in the seasons under a future climate considering the increasing relevance of rainfall. One goal, however, was to have a simple classification which accounts for the basic snowfall and snowmelt season to be able to distinguishing between the two important FGPs. Please see also our response to your last comment.

Ad (d)

- *(iv) The implication of using a period specific threshold for the results should be discussed.*

  We do not agree since any extreme value statistics are calculated after the over threshold values are selected. Using a fixed threshold for both periods may in some cases bias the results on the frequency of events in some catchments.

- *(v) I understand it such that the 'normal flood duration' is derived based on the response of a saturated soil (see also point (o)); how does this concept apply to a snow generated event? Further, it is still not clear to me how the concentration time and recession time are determined. Please clarify.*

  No, the normal flood duration is derived based on the response during baseflow conditions and empty storages in the model. The catchment is then saturated by adding twice the amount of annual rainfall. The concentration time to the resulting flood peak and the recession time back to baseflow again are estimated from the hydrograph and defined as “normal flood duration”. This measure is the used to determine the maximum time span in which snowmelt/rainfall processes can contribute to a flood event.

Ad (e): The remark on ‘rain on snow events’ was related to what happens when it rains on a snowpack (in the model) – will this rainfall immediately infiltrate similar to rain on bare ground, or will there be a delay?

In the HBV model, rain on snow is added to the snowmelt volume. Meltwater is retained in the snowpack until a parameterized fraction of liquid water is reached; exceeding that threshold, meltwater is released from the snow pack and added as input to the soil moisture routine.

Ad (k): I agree that the way you present changes in the seasonality index is intuitive. The question is how robust (or sensitive) the index is in showing changes in flood seasonality (as well as in magnitude and frequencies) implying changing processes, when a two-season (fixed) model is chosen?

The index is robust that it can distinguish between the snow accumulation and the snow melt period also in the future. There may, however, still be more rainfall floods during late summer and early autumn for which this fixed two-season model would not be perfect. However, the projected mean annual timing of the rainfall generated floods (shown in Fig. 6) highlights the increasing importance of winter rainfall floods. Changes in the timing of snow accumulation and snow volume are very important for the future period, and this is well handled by the definition of the index.