Reviewer 3 – response to authors

Thank you for your detailed response and revisions following the review comments. Below you will find some general remarks, followed by some detailed comments to the authors final response to the referee comments.

General comments
A key issue raised in the first review was the low number of catchments included, which does not justify the reference to a regional analysis. This has now been corrected and focus is on the catchment scale represented by six catchment with different flood regimes. The recent paper by Köplin et al. (2014) also referred to by the authors in the revised version, is a good example of a regional study, including 189 catchments (suggest to delete the statement on p.24 related to option to include more catchment, "which would significantly increase the numerical demand of the study", as it is not considered a good argument). Generally, it is recommended to include a large number of catchments when addressing changes in seasonality due to the high local variability in catchment processes (as stated also in the paper, p.5, line 10). Thus, the added value of the current study primary lays within the detailed analyses of the selected catchments, including the uncertainty assessment.

The authors have overall responded in great details to the comments made by the reviewers, and I largely find their response well argued. However, some concerns still remain:

- 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).
- 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).
- 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.
- 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.
- 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.
- 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.

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.
- 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)".
- 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.
- 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.
- 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.
- 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).
- Ad (d)
    - (iv) The implication of using a period specific threshold for the results should be discussed.
    - (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.
- 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?
- 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?