

Reply to Interactive comment Anonymous Referee #1

Firstly, we would like to thank the reviewer for devoting time to reviewing our manuscript. We are happy to hear that the topic of our paper is of interest. In the following we will discuss the issues raised by the reviewer and hopefully answer all questions satisfactorily.

General comments:

The paper includes two studies, a quantitative one analyzing the newly introduced “snowmelt droughts” and “glaciernelt droughts” and their meteorological causing factors as well as a qualitative one investigating socio-economic impacts of “temperaturerelated” droughts. The topics of the paper are interesting and the paper is well written. I also highly appreciate the combination of the two approaches in one article.

>> Thank you for these positive comments.

However, in my opinion two points should be addressed, before publishing the article: First, the authors make use of a conceptual model to derive SWE and state that glacier was not explicitly modeled. However, some of the catchments are to a considerable percentage glaciated (up to 75%). The models are calibrated to observed streamflow and hence parameter that are sensitive to temperature - as can be found in the snow routine - are likely to be affected to compensate for a missing glacier concept. This could lead to questionable SWE estimations and affect the follow up analysis.

>> We agree with the reviewer that SWE calculations by a model that does not include glaciers and that is calibrated to observed discharge should not be used in glaciated catchments. When studying SWE results for glaciated catchments we indeed observed erroneous SWE estimates due to missing glacier processes. The issue of snow compensation was investigated in Parajka et al., 2007, however in that study the catchments of Hoalpe or Otztal were not included (partly because of glacier effects, partly because of insufficient snow depth observation density).

In our paper, modelled SWE was only used in the catchments without glaciers, so only in the analysis of snowmelt drought. The clusters of catchments without glaciers are Gailtal, Mühlviertel and Glomma (with exception of catchment Akslen). For the snowmelt drought analysis, SWE was used to:

- 1) determine the seasons and their length per catchment;*
- 2) classify droughts into snowmelt droughts and other hydrological drought types;*
- 3) correlate maximum winter snow accumulation to spring discharge and drought deficit.*

In the glaciated catchments, which were used for the analysis of glaciernelt droughts, modelled SWE was not used at all. For glaciernelt drought analysis we used:

- 1) fixed seasons: summer from 1 June to 1 September;*

- 2) *temperature and precipitation for drought classification;*
- 3) *temperature and precipitation to correlate to summer discharge and glaciermelt drought deficit.*

To clarify this issue in our manuscript we will adapt the text of lines 13-14 on page 10473. We will mention that SWE was only used for snowmelt drought analysis in the non-glaciated catchments.

Secondly, I have some difficulties talking about “drought” during a peak flow. I see that the definition of below average water availability includes these cases as well. The authors conclude from their qualitative analysis regarding “glaciermelt droughts” that socio-economic impacts were not found. I could see also only socio-economic impacts of “snowmelt droughts” in combination with another drought type. With this lack of impacts connected to the new drought types, I am not quite convinced of the usefulness of their introduction.

>> It is a bit contra-intuitive to denote anomalies in the high-flow season as a “drought”. As noted by the reviewer, it does however still comply with the definition that “drought” is below-normal water availability. In this study we did not find any impacts of glaciermelt droughts. The reasons are discussed on page 10490 (l.1-11). We did, however, find impacts for snowmelt droughts. Not in the historical databases, because the climate of the investigated countries (UK and France) is too warm for the occurrence of snowmelt droughts. But in the EDII two-third of the reported temperature-related droughts were snowmelt droughts, of which half of the events were combined events and half were pure snowmelt drought events (see Figure 11 – lower part). With cold snow season drought, snowmelt droughts are amongst the temperature-related drought types with most severe impacts. For the DIR we could not classify drought events into types, but the anecdotal evidence suggests that also in the US many impacts related to winter droughts are in fact linked to snowmelt deficiencies (p. 10485, l.5-7).

Droughts in the high-flow season do not only have socio-economic and environmental impacts in the same season (spring/summer). Impacts (e.g. crop yield loss due to restricted availability of irrigation water, reduced ecosystem services, restrictions in water supply, lower hydropower energy) often occur in the following (usually drier) season. It appears more and more in the statistical analyses of links between impacts and drought as a natural hazard to use lagged correlation or drought indicators that aggregate the hazard of a long preceding period (e.g. SPI-6 or 12, or SPEI-6 or 12) (see Dieker et al. 2010, ; Stahl et al., 2015) to include below-average water in the wet season. Hence, it is prerequisite to include drought in the wet season in the full impact analysis. This lagged influence makes it more difficult to qualitatively link drought impacts and causes, as we aimed to do in our study.

The relevance of wet-season droughts is likely the reason that in many drought studies a variable threshold level is used that by definition also gives droughts in the high-flow season (e.g. Vidal et al., 2010; Parry et al., 2012; Prudhomme et al., 2014). But in many of these studies the droughts in the high-flow season (quantified by the use of a variable threshold) are not explicitly mentioned and, therefore, they do not lead to discussion about the definition of drought. Similarly, the widely used drought index SPI (Standardised Precipitation Index; McKee et al. 1993) is never questioned in that sense although it is comparable to a variable threshold level applied to precipitation and it results in

droughts in wet months. For example, in a monsoon climate the SPI defines “drought” in the monsoon season, which might be questionable as well, but in terms of impacts in the subsequent dry season it is very relevant if the monsoon rain was insufficient to recharge the aquifers or fill up the surface water reservoirs, which serve as key storages to supply different economic sectors with ample water during the dry season.

Additionally, we want to argue that the introduction of these “drought” types is useful and that this paper fills a scientific gap. We write on page 10488 (l.9-13) that a temporary lack of snowmelt and glaciermelt has never been studied, in contrast to the opposite phenomenon: abnormally high snowmelt or glaciermelt. Although we did not find any impacts related to glaciermelt droughts and many combined events for snowmelt droughts, we do want to stress the crucial importance of this water source in many areas around the world. For example in Asia, millions of people are dependent on the meltwater from the Himalayas (Immerzeel et al., 2010). If the water they expect is not coming due to a lack of glaciermelt because of low summer temperatures or a lack of snowmelt because of high winter temperatures or low winter precipitation, this causes serious issues (see citations below). Also the current drought in California is for a large extend caused by a lack of snowfall in three consecutive winters, resulting in the lowest snow water contents ever measured and leading to serious issues with water supply. The fact that these issues are not that well documented in relation to a lack of snowmelt or not found in this research does not mean they do not exist or are not relevant.

Citations from media:

“Runoff from the Rocky Mountain snows accounts for 60 to 80 percent of the annual water supply for more than 70 million people in the Western United States. The timing of snowmelt affects the levels of water available for crop irrigation and hydro-electric power. It can also influence the risk of regional floods and bush fires.” - <http://www.scientificamerican.com/article/snowpack-ice-cover-shrinking-rocky-mountains/>

“Downstream populations are dependent on melt water for agriculture, drinking and power production” – Climate News Network (<http://www.climate-news-network.net/rockies-and-everest-lose-ice-and-snow/>)

California:

- <http://saveourh2o.org/content/Drought2014WhatYouNeedtoKnow>
- <http://www.nationalgeographic.com/west-snow-fail/>
- <http://www.nrdc.org/water/files/ca-snowpack-and-drought-FS.pdf>

Also, in the light of climate change, projected decreases in snowmelt or long-term glaciermelt are regarded as a serious concern for water supply (Seneviratne et al., 2012; IPCC 2014). Long-term projections are often in the order of magnitude of the current interannual variability or even larger at the end of 21st century in the sense that an extreme event today will be much more likely in the future. Therefore, we want to argue that it is very relevant to study the causes (and impacts) of interannual variability in snowmelt and glaciermelt today.

We could include parts of this argumentation into our paper. It would be beyond the scope of this paper, however, to include an elaborated discussion of the definition of drought. We propose to

include max. 1-2 sentences in the Introduction and a short argumentation for the advantage (and disadvantage) of the use of variable threshold levels in the Methodology in Section 3.3.

While I am very happy to see the attempt to connect the qualitative and quantitative studies, I see potential to improve the connection between the two. The newly introduced drought types do not receive much attention in the second part of the study, while the other temperature-related droughts are not analyzed in the first part (which would be probably beyond the scope of the study).

>> It would indeed be beyond the scope of this study to completely do a qualitative analysis of the impacts of the new drought types, since it would require collection of impact information (which can take years), or to redo the quantitative analysis of causes of the other temperature-related droughts, since these were analysed in a previous study (Van Loon and Van Lanen, 2012). We do, however, see room for improvement in the connection between the two parts of this study. We propose to restructure the first two sections of the paper (Sect.2 Theory and Sect.3 Causing factors) to include all temperature-related drought types. In these chapters we will first summarise the results of the previous study of Van Loon and Van Lanen (2012) before the new types are discussed. This will increase the coherence of the paper. We will also adapt some of the formulations in the manuscript.

Furthermore, we will focus on increasing the connection between the two parts of the study by investigating the causing factors of the specific events reported in the region of our case study catchments (see reply to comment of Reviewer #2).

Specific comments:

10472 L22: how was corrected for the elevation differences?

>> For Austria, we used precipitation data derived for elevation zones of 200m. These were derived from grid maps (1km resolution) estimated by external drift kriging interpolation (which accounts for the effects of topography - local gradient between topography and precipitation/air temperature).

For Norway, elevation differences were corrected by site-specific precipitation altitude gradients, which were determined during model calibration. The ranges of possible values were based on previous experience with the model and observed data from meteorological stations. The model and its implementation are described in Beldring et al. (2003) and Li et al. (2014).

We will add this information to the manuscript.

10473 L6: To which objective function was the model calibrated? I wonder a bit about the meaning of the Nash-Sutcliffe values when comparing glaciated with non-glaciated catchments see Schaeffli (2007)

>> For Austria, we used a compound objective function consisting of three parts: (1) Nash Sutcliffe (NSE), (2) NSE estimated from logarithmic transformed runoff values and (3) informed guess about the shape of the a priori distribution of model parameters. This objective function was introduced and tested by Merz et al. (2009, 2011) with good results. For Norway, the HBV model was calibrated

using the PEST parameter optimization software. The objective function to be minimized by PEST is the sum of squared differences between model simulations and observations. This objective function was also applied to discharge data. We agree that there are other alternatives to Nash-Sutcliffe and sum of squares, but for this study, using only SWE from simulations, we considered the effect to be small.

We will add this information to the manuscript.

10479 L15 in this section Pfister (2006) could be cited that also used historical sources to reconstruct (winter) droughts

>> Thank you for this suggestion. We do know the work of Pfister (2006). His historic reference does not concern historic written documents of drought impacts (what we present here) but a gauge (Schiffhäuser) and his paper focuses on one location only (Basel). Therefore, we cannot use his information in our analysis, but we will refer to his work in Section 4.1.1.

Technical corrections:

10470 L17: remove first "normal"

10476 L11: break into two sentences

10477 L15: "was not as well visible" – rephrase; 50-50% meaning?

10477 L21f: maybe influenced by SWE estimations?

10487 L7: Sentence unclear

10487 L26: What is intended to state here?; the USA

10499 Table1: the authors could add season durations

10507 Figure4: for clarity could be zoomed into the regions

10512 Figure9: the dots are not distinguishable in a b&w print

10513 Figure10: the dots are not distinguishable in a b&w print

10514 Figure11: red-green blind persons might have difficulties distinguishing between B and D. Please, explain the meaning of the dashed event in 1920 in the caption.

>> Thank you for these suggestions. We will correct these in the revised manuscript.

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