Comments to 2nd round reviews

Black text: Referee and editor's comment

Blue text: Author's response

Review revised manuscript Jenicek et al (hess-2015-269) - Wouter Berghuijs (Reviewer #1 in the previous round of review)

Overall I am pleased with the response of the authors to both reviewer #1 and #2, and Michael Stölzle; the changes to the manuscript improved the readability, better express the novelty of the paper, and provided some extra insight to strengthen the analyses. However, before I am supportive of publishing the article, but please address the following comments:

Firstly, thank you for your valuable comments. Please see our corresponding replies below.

ABSTRACT

- Why don't you give the information how much lower the sensitivities for lower elevation catchments are? "On average, every decrease of maximum SWE by 10% caused a decrease of minimum discharge in July by 6% to 9% in catchments higher than 2000 m a.s.l. Towards later summer and in lower elevation catchments this effect is reduced."

  This section was reformulated as: "On average, a decrease of maximum SWE by 10% caused a decrease of minimum discharge in July by 6% to 9% in catchments higher than 2000 m a.s.l. This effect was smaller in middle and lower elevation catchments with a decrease of minimum discharge by 2-5% per 10% decrease of maximum SWE."

- "Considering years with below-average SWE maximum, the minimum discharge decreased to 75% of its normal level." This statement is not completely clear to me: is this true for ALL years with below average SWE? Also I do not think the word "Considering" is the correct one to start this sentence, e.g. replace it by "For".

  The sentence was really not clear. We reformulated it as: "For higher and middle elevation catchments and years with below-average SWE maximum, the minimum discharge in July decreased to 70-90% of its normal level."

CONCLUSIONS:

- All the conclusions are written as general findings, and not specifically linked to the studied catchments in Switzerland. Be explicit in your conclusion about what data you have used, and for which catchments your findings are representative.

  Some sentences were reformulated for clarity. See our corresponding replies below.

- Your statement that (for example see conclusions) "The sensitivity of low flows to maximum annual SWE was higher for catchments at higher elevation." is misleading without adding the information that you look at relative changes in SWE; at higher elevation, snowpack (and thus SWE) increases compared to rain. An %increase of SWE at higher elevations is thus more mm of water available for rivers, compared to lower elevations.

  The sentence in corresponding bullet was reformulated as: "The sensitivity of minimum discharges to maximum annual SWE was higher for catchments at higher elevation when looking on relative changes in minimum discharge and maximum SWE."

- "a decrease of maximum snow accumulations by 100 mm resulted in earlier runoff minima by 12 days." Unclear if this an average across catchments? If yes: it's more informative to give information on the range of sensitivities that you find, rather than only stating the average.

The corresponding bullet was reformulated as: "Low flows occurred later in the year for years with above average snow accumulations. However, the Spearman rank correlation between maximum SWE and the DOY with 7-day minimum discharge was significant only in some higher and middle elevation catchments (mean catchment elevation above 1500 m a.s.l). The differences between the catchments were determined by both higher maximum SWE and later melt-out in the higher elevation catchments."

- Replace the "the annual variability" by the "inter-annual variability"

  Thanks for noting this, we replaced all occurrences.

- I can't find the logic in the final statement: "Summer low flows are significantly sensitive to any SWE changes. Thus, a reducing effect of snow on late summer low flows in highest elevation catchments is expected due to predicted climate changes. As a consequence the sensitivity of catchments in mid and high elevations to meteorological droughts might increase." Please rephrase such that it is clear.

  The corresponding bullet was reformulated as: "Summer low flows in the study catchments were significantly sensitive to any SWE changes. Although our study did not address climate change impacts explicitly, a reduced effect of snow on late summer low flows in the highest and middle elevation catchments can be expected due to the predicted decrease of snowfall fraction in the future. As a consequence the sensitivity of catchments at mid and high elevations to meteorological droughts might increase."

- The emphasis on maximum sensitivity for high elevation catchments in a changing climate (compared to lower elevations catchments) is misleading in my opinion. You only showed that summer low flows are more sensitive to %SWE changes in these catchments. However, it is probably the case that these high elevation catchments' SWE is less sensitive to temperature rise; take the example of a low elevation catchment that has a very small snow pack. If the temperature rises a little bit the SWE will change a lot (for example completely disappear), while that same temperature rise for a high elevation catchment only leads to a e.g. 20% decrease in SWE.

  We completely agree. This comment is also addressed in the response to reviewer #2 (answer to major comment, part "Catchment sensitivity") and consequently in the first revised version of the manuscript (part 4.2, second paragraph). Based on results we actually can only state what would happen in case of any % change of SWE. Any interpretation going towards the sensitivity of catchments to temperature increase might be misleading for reasons you explained in your comment above.

  Our conclusion about higher sensitivity of higher elevation catchments than lower elevation catchments could be only related to % change of SWE (% change of SWE was calculated separately for each catchment relative to catchment mean) not to increasing temperature (see Fig. 4, Fig. 6 and Table 3). However, it is clear that snow is a more important component of the water balance at higher elevations than in lower elevations simply due to higher water volume stored in the snowpack (both absolutely and relatively to total water amount in catchment). So it is not surprising that such catchments react more sensitively to any % change of SWE (representing higher water volume than the same % change of SWE in lower elevations).

  We added more detailed explanation in section 4.2 (second paragraph) and we reformulated the last conclusion bullet.

Figure 2: Maybe because I am stupid: but what is the function of the dendogram (grey things) in this figure?

A dendrogram shows the mutual similarity of individual variables using hierarchical cluster analysis. Here, it just shows the similarity of individual predictors and response variables. This information is in the respective part of text. Additionally, we added a short explanation both in the text and in the figure caption for clarity.

Editor Decision: Reconsider after major revisions (22 Dec 2015) by Anne Van Loon

Dear authors,

I have considered your responses to the reviewers comments, your revised manuscript and the additional comments of reviewer no.1. My evaluation is that some good changes have been made, but the manuscript is not ready for publication yet. Please find some points for major revision below:

- The comments and suggestions of reviewer no.2 have not been taken into account sufficiently. You have rewritten parts of the discussion, but mainly added speculation that is not grounded in the analysis of the study.

Firstly, we thank the editor for her valuable comments and we appreciate the effort she put into this. Since the first comment above is of general nature, we assume it is covered again in the more specific points listed below. Please see our corresponding replies there.

- The manuscript lacks process understanding to answer the research questions. For example, you make an implicit link between climate change, snow and low flows, but you do not investigate the separate effects of precipitation and temperature (SWE can increase with climate change if increasing P compensates for effects of increasing T) and do not convincingly answer the question of reviewer no.2 about the higher sensitivity of catchments with temperatures around 0 degrees.

Thanks for the comment, however, note our research questions and goals. We never intended to investigate the effect of climate change and hence have not assessed separate effects of P and T in the data analysis (although it would be definitely interesting and valuable topic for an additional study). Thus, to make “an implicit link between climate change, snow and low flows” might be misleading as explained on page 16, lines 23-28 of first revised version.

Based on suggestion of the Reviewer #2 we included some additional discussion during the first revision, which necessarily is of more speculative nature, if we are not to change the scope of the paper. To address this initial comment (and the comment related to catchments with air temperature around zero degree) nevertheless, it should be appropriate to address the effect of climate driven changes in SWE independent on how these changes were caused. However, note again, that any link between T and minimum discharges would be rather speculative. This was clarified on page 16, lines 23-28 of first revised version and in the final response to major comments of the Referee #2, part “Catchment sensitivity”.

In addition, we now reformulated some parts of the discussion (mainly chapter 4.2, second paragraph) and conclusions to make it clear and to remove all unnecessary speculations related to climate change.

- The effects of evapotranspiration and other water balance issues also need more consideration. Your assumption that actual ET is lower in wet years compared to dry years was contradicted in Teuling et al. (2013: Evapotranspiration amplifies European summer drought. Geophysical Research Letters, 40(10), pp.2071-2075.) for catchments including one in Switzerland. Your argument that you do not have ET data is not sufficient, because ET can be calculated from temperature or can be obtained from large-scale datasets.

Please note, that the scope of our paper was to evaluate the relative importance of snow and precipitation. This is of interest for conditions like in Switzerland, where there is not a long dry period after snowmelt (as for instance in the western US). We did not focus on all components of hydrological cycle (including ET). However, we now decided to make a new simple comparative analysis to show the inter-annual variability of ET, P and SWE.

The ET is not that easy to estimate from standard climate data after all (besides, temperature-based ET estimation usually overestimates the temperature sensitivity). Therefore, we decided to use a radiation-based approach included in the PREVAH model instead of temperature-based approach. The PREVAH model uses observed global radiation at a daily temporal resolution to calculate ET. The Penman-Monteith equation is included in the computation of potential evapotranspiration (PET). Stomata resistances increased as a function of soil water deficit. Please see the respective new part in the methodology chapter (2.2) for details.

We included a new figure (Fig. 10) summarizing inter-annual variability of ET, seasonal P (both are sums from 01 Jun to 30 Sep) and maximum SWE separately for elevation groups from 1980 to 2009 (the time period of PREVAH data).

The ET in the warm season shows much lower inter-annual variability (using the variation coefficient Cv) than precipitation and maximum SWE (Fig. 10). These results show that ET had a smaller impact on the inter-annual variability of 7-day minimum discharges compared to the impact of precipitation and maximum SWE. Although, this is a relatively simple approach, it yet shows ET to be a less useful predictor to explain inter-annual variability of low flows compared to other predictors. However, it does not mean that ET (and its seasonal variation) is not an important predictor to explain the low flows extremity itself as shown e.g. in Teuling et al. (2013).

As for the editor's comment related to the "drought paradox": We expected higher actual ET in wet years and lower actual ET in dry years as stated on page 15, lines 31-32 of first revised version of manuscript (we assume typos in the second sentence of the editor's comment above). However, Teuling et al. (2013) described an opposite effect contrasting our original assumption at least for Rietholzbach (this Swiss catchment is not in our selection). Therefore, we removed our assumption because it probably needs deeper analysis which is beyond the scope of this paper.

- Similarly, your argument about the undercatch that is not taken into account is not convincing, because undercatch correction can be done, probably not with higher errors than caused by the methodology in the construction of the SWE dataset.

Yes, an undercatch correction can be done, but given the complexity of the problem, the available monitoring infrastructure and the complex topography in Switzerland, this would rather add uncertainty than improve the available precipitation products. Note that unlike in other countries, precipitation and snow monitoring networks in Switzerland are for the most part not collocated, the latter being mostly setup to serve avalanche forecasting purposes, not hydrology.

The MeteoSwiss gridded precipitation product (RhiresD data) used here is the most elaborate product available. While it does not explicitly account for snow undercatch it incorporates regional precipitation gradients that implicitly account for corrected data. See Frei and Schär (1998) and the Meteoswiss official documentation (Meteoswiss, 2013) for more details.

- It is unclear how more snow affects the timing of low flows (page 13). Is there more storage at higher elevations?

Yes, there is more snow at higher elevations (see Table 1). However, based on our results, the relation between maximum SWE and the day of year with 7-day minimum discharge is significant only in some higher and middle elevation catchments. The differences between catchments are probably driven by both higher maximum SWE and later melt-out in higher elevation catchments.

We reformulated the relevant part of the last paragraph of chapter 3.2 for clarity as well as respecting bullet in conclusions (according to suggestion of Wouter Berghuijs in his referee's comment). See also explanation to next two comments below.

The effect of a later melt-out (mentioned on page 14) is very important and the relation between maximum SWE and timing of min flows cannot be used predictively.

We assume that this comment is related to the same text as your comment above (timing of low flows shown in the Fig. 7 described on page 13 of first revised version). If so, we completely agree with this comment. As mentioned above, we added some additional explanation to the last paragraph of chapter 3.2.

Lower SWE max in the same catchment with the same temperature is not likely to result in earlier low flows.

We partly agree. However, based on Figure 2 (and detailed results not shown in the paper) there is a correlation between maximum SWE and the day of year with 7-day minimum discharge in some higher and middle elevation catchments (above 1500 m a.s.l.), although the spearman correlation coefficients are rather low (0.2-0.3), but significant ($p<0.05$). Differences between individual catchments could be related to both higher maximum SWE and later melt-out (as documented in Fig. 7 and related text in chapter 3.2).

Similarly as for the previous two comments, this explanation was added to the last paragraph of chapter 3.2.

Also the correlations in Table 3 give no information about the driving factor since elevation, slope, drainage density, and maximum SWE are not independent.

We partly agree. However, we did not want to show general driving factors influencing low flows (which is discussed in part 4.1 of the revised version). As described in the text related to Table 3 (chapter 3.2), this figure only shows that the sensitivity of low flows (using Theil-Sen slope) to any change of maximum SWE is higher in catchments with higher elevation, higher maximum SWE, higher S/P and does not depend on catchment area (which was one of the question of Referee #2 which we addressed in the first revised version). So, if there will be any change in maximum SWE, then the minimum discharges in higher elevation catchments will be influenced more than the minimum discharges in lower elevation catchments. However, there are differences within individual months (see Table 3 and related text on page 12 of revised version).

The previously missing information about mutual dependency of some catchment characteristics was added to the text (respective part of chapter 3.2).

- Furthermore, you refer often to groundwater recharge in the results and discussion (e.g. page 11, 13, 14, 19), but in your study you did not perform any groundwater analysis at all. These statements are too speculative.

The idea was just to mention (the general) knowledge that snow is supposed to be important for groundwater recharge and thus low flows in summer period. However, we agree that some formulations were too speculative so we either reformulated the corresponding text or we completely removed the reference to the groundwater recharge from the text in cases where it does not change the readability of the manuscript.

- The introduction needs more comprehensive rewriting to increase readability and to decrease the enumeration of literature.

When writing the Introduction, we followed the sequence “changes of SWE/winter precipitation/snowfall fraction – consequences to runoff changes – liability/sensitivity of different regions to described changes in snowpack”. In order to preserve this structure, which we find appropriate, it is difficult now to completely rewrite the text. Besides, the text was previously reformulated to include several suggestions of both reviewers (see their comments in the first round of reviews).

However, we now went through the text again and made some changes, e.g. we removed some review-style text as you suggested. We hope that the introduction is now more concise.

- You claim not to study the effects of climate change, but do speculate about these effects (e.g. on page 19). More analysis, for example on trends, might be needed to back up your claims.

To improve the discussion (not the analysis) related to climate change was one of the suggestions of Referee #2, and we were encouraged to consider this comment in our revisions by the editor. Thus, we added some text discussing possible impacts of climate change, even though the scope of the paper is different (as is explicitly stated at the beginning of the relevant discussion section 4.2 on page 16, lines 23-28 of first revised version).

We agree that in the absence of respective data analysis most of the points we've discussed are mainly speculative. In fact, this topic is interesting and planned to follow up in future research.

We reformulated the relevant part of the discussion (section 4.2) as well as relevant bullets in conclusions to be clearer and to avoid speculations.

I see the value of the new SWE dataset that is used in this study, but if that dataset is the novelty of the paper then you should change the aim of your paper and present a comparison with traditional methods to estimate SWE (e.g. by a degree-day approach).

We do believe that the SWE dataset was a major benefit for this study as furthermore shown in some other studies (Jörg-Hess et al., 2014; Jörg-Hess et al., 2015). Moreover, in the context of known uncertainties in measuring precipitation during winter (see our related reply on MeteoSwiss gridded data used in this study), the SWE dataset used here is the most elaborate dataset available (see methods chapter for further details and references).

We did not explicitly highlight in this paper that the SWE dataset is rather novel as this data set is the topic of other recent papers. However, we believe that it significantly increased the reliability of our analysis (as explained in the last paragraph of Introduction). Based on our results we pointed out that we see a big potential to use this data for future similar studies (chapter 4.4 of revised version). Testing of the reliability of SWE dataset was done in Jörg-Hess et al., 2014 as mentioned in chapter 2.2 of revised version. Again, we want to emphasize that the novel aspect of our manuscript lies in the quantification of the relative importance of snow and rain for summer low flow. While the general results might not be surprising, expressing this in numbers is novel.

These suggestions and those of the reviewers (new evaluation of reviewer no.1 and previous suggestions of reviewer no.2) need to be considered before this manuscript can be considered for publication.

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