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Interactive comment on "The effects of climatic anomalies on low flows in Switzerland" *by* Marius G. Floriancic et al.

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We thank Robert Lubben (and Ryan Teuling) for their feedback. We very much appreciate the suggested improvements and will address them in our revised version. Below we list our response (in bold) to the reviewer's comment *(in italic)*.

Short summary:

The reviewed paper is about the effects of climatic anomalies on low flow occurrence in 380 Swiss catchments for the period 2000-2018. The low flows are defined by the annual 7-day lowest flows. The anomalies in precipitation and evapotranspiration are *calculated for several time periods before the annual low flow occurrence (7 up to 182 days). With this data two hypotheses are tested 1) low flow occurs after anomalous weather conditions and 2) that the most extreme flows will be associated with the most extreme weather anomalies. The results of the study are that the low flows mostly occur after anomalous precipitation and evapotranspiration events. Most of the low flows (92%) are influenced by below average precipitation and 70% is influenced by above average evapotranspiration. Also, the extreme weather anomalies, or meteorological droughts, tend to generate extremely low flows. Winter precipitation, as SWE, was less important for the low flow seasonality than the climatic anomalies in this study.*

General comments:

Overall, this paper is well written and of high quality. No textual errors could be found. The two hypotheses and their origins are stated well. The need for having an answer on the hypotheses has a good relation with former research. The conclusions clearly give an answer to the hypotheses and the discussion about the effects of SWE on low flow seasonality is helpful for placing the results of this study in context and linking this study to former research. The figures of the results help to give a better understanding and can easily be linked with the corresponding text. The overall structure of the paper really shows the reader how the research is conducted and which methods are used. The methodology clear and can (almost) be reproduced with the given description. However, the method for estimating potential evapotranspiration needs a bit more clarification, see major comments. The data that has been used for the analysis is of high quality and has a high spatial density. The estimation of low flows is a good method that is widely used in low flow analysis and it identifies the occurrence of a low flow in such a way that errors in measurements and can be filtered out (Smakhtin, 2000). The precipitation and evapotranspiration anomaly quantification for different durations is a good way of finding anomalies in the climatic data if a sufficient amount of years is

used. The study is novel because, this type of low flow analysis is not carried out before on this scale with this many catchments. Although, the used method is not exceptional or novel, it gives a good view on the separate and combined effect of precipitation and evapotranspiration on low flow occurrence. Also, the used visualization method shows the effects of extreme evens well (like figure 6 in the manuscript). The visualization of enhanced PET under drought conditions is also nicely visualized. These figures help by understanding and supporting the text and give the reader a clear first glance at the study and the results.
Low flow seasonality is a relevant topic with the expected increase in weather anomalies by climate change. Understanding the climatic drivers and their impacts on low flow genesis can help understanding the processes leading to low flows and help in managing discharge. This becomes more and more relevant after the recent drought years. In my opinion, there are no mayor points that limit acceptation of this paper. However, there are a few major comments that could be useful to take into consideration.

Thank you.

Major comments:

There are three major comments that can be raised while reading this work. The first comment is about the exclusion of 2% of the low flows, this is not supported in the current context. Low flows in years with high precipitation will still be caused by anomalies in precipitation and evaporation. Therefore, the exclusion of the low flows that are above the value of three standard deviations from the mean seems not logical. In this way the method with which the low flows are estimated is in conflict with the actual used method where 2% of the data will be excluded because it is above 3 standard deviations. This means that the definition of a low flow has to be changed or that all data has to be used in this study (including the 2%). Including this data should not *influence the results in a negative way because the drivers of low flow are likely the same. Another way to clarify this could be to do the analysis with including the 2% of low flows and then conclude that the abnormally high low flows are not significant or hinder the analysis. After the analysis the high low flows can be excluded with a good reason.*

The exclusion of 2% of the low flows does not change the overall results. The reason for the exclusion is not to get rid of "wet years" but rather to remove outliers in our dataset. Measurements of low-flow magnitudes are critical, and with the 3-sigma rule we exclude outliers that might result from faulty measurements. To avoid the possibility that these data points distort our analysis we remove the upper 2% of our data points. This is similar to many robust estimation methods in statistics, in which the extreme tails of a distribution are trimmed off before statistical parameters (means, variances, etc.) are calculated. We will make that clearer in our revised version.

The second major comment involves the used potential evapotranspiration estimation method. This is only addressed very briefly via referencing to the paper of Hargreaves and Samani (1985). The used calibration parameter value and the source of solar radiation are not given in this way. The calibration value can influence the results for the Hargreaves PET estimation significantly. Especially in humid areas the PET can be overestimated when using Hargreaves (Trajkovic, 2007). A method that uses observed radiation can therefore result in less evaporation which influences the overall PET anomalies. The estimated PET can possibly be validated with the lysimeter used by (Seneviratne et al. 2012b). A clearer description of the used PET method by including the formula and the used values for the parameters will help by giving insight in the uncertainty of the PET estimation. This will also help making the methodology more clear and improve the possibility to apply this framework elsewhere.

Our analysis only depends on the relative values of the PET estimates, not on the absolute PET values themselves. Because the Hargreaves calibration parameter only re-scales the PET values, it has no effect on their relative magnitudes and therefore will have no effect on our result. We can of course present the Hargreaves formula and the values of the coefficients for readers who are unfamiliar with the particulars.

The last major comment is on figure 4 (page 9) of the manuscript. In this figure the timing of annual low flows in Switzerland is shown. The text that refers to this figure states: 'Within the Swiss Plateau, low-flow timing is more spatially consistent during some (non-extreme-drought) years (e.g. 2009, 2013, 2016), than during others (e.g. 2000, 2002, 2004, 2010, 2017)'. However, there is no reason given for this difference for each year. Is this caused by SWE, other drivers of low flows or P and PET? If it is more related to P and PET it is useful to include this in the text to further clarify the contribution of these drivers on more local scale. Also, if it is caused by SWE the results of the paper of Jenicek et al. (2016) can be related to this in non-drought years. Therefore, I suggest to get a better understanding of the variability of streamflow in non-drought years. This can be done by looking more closely at the relation between SWE, PET and P on streamflow during these years. This can also help by putting the studied drivers (P and PET) in context to other drivers of low flow like anthropogenic activity.

We will extend the discussion of different low-flow timings throughout the years. The differences in low-flow timing are most clearly related to when the climatic anomalies (of precipitation and PET) occur, as most of the low flows (no matter when they occur) are related to these climatic anomalies. We show (and will more extensively discuss) how these P and PET anomalies, but less so winter precipitation, relate to the timing of low flows.

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Specific comments:

In part 3.2 (line 140-142) of the manuscript the graph of figure 1 is used to explain the contribution of P and ET to low flow occurrence: 'However, again distinct regional differences exist: at low elevations, almost all annual low flows occur after periods of anomalously high potential evapotranspiration and anomalously low precipitation (Fig. 1ab)'. However in this figure only the occurrence of low flows per month related to the elevation level of the catchment is shown. In figure 2 the differences explained in the text of 3.2 are shown and therefore this reference should be changed to figure 2a,b.

We now realize that this can be misleading. We will change this sentence.

By implementation of this framework in another study area, can be stated that the PET estimation method maybe has to change depending on the climatic conditions of the new study area. It could be that they have to switch to radiation based methods (see major comment 2) depending on the local climate.

We agree that other PET methods may be more suitable in other places. It is important to remember that we do not intend to find the best PET estimation method in this paper, but it is more important for us to use a consistent method which takes advantage of the unique gridded air temperature data that we have and that is robust for all the 380 catchments in the different geoclimatic regions of Switzerland. It is also important to remember that our analysis only depends on the relative differences in the PET estimates (from year to year and day to day, but not from site to site), and not on their absolute values.

The description for figure 2 and 3 is quite large and maybe can be shortened by putting more explanation in the text or by making the figures clearer with a main and sub-title.

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Especially, the part about the percentages of low flows that are caused by combinations of drivers (figure 3), is already mentioned in the text.

We agree that the captions of Figures 23 are long. This is on purpose: experience has shown that readers who are browsing an article will find it much easier to understand if the main point of each figure is clearly stated in the caption, not just in the main text (which may appear several pages earlier or later, and thus will not be found unless the reader actually reads the whole paper from front to back). We also find that it is helpful to give detailed explanations directly in the caption, rather than expecting the reader to flip back and forth between the figure and wherever it is discussed in the main text. We will consider shortening the figure captions, by removing parts that are not essential.

The line in figure 2 seems higher than 1200 meter (even with the non-linear y-axis). The data seems to be more in agreement with Jenicek et al. (2016) on a separation between low and high elevation catchments around 1350 meter above mean sea level. This can also be a part of the discrepancy between Jenicek et al. (2016) and this manuscript on SWE relation to low flows.

The non-linearity of the y-axis is necessary to avoid a very uneven distribution of catchments along it. We double-checked that the line is really at 1200m. We would expect that the overall results will not significantly change if the threshold is shifted from 1200m to 1350m.

The reason for choosing the spearman correlation instead of for example the Pearson correlation is not given. This can easily be done by stating that the data is non-linear and the spearman correlation will result in a better fit with this data. See D. R. Legates G. J. McCabe (1999).

Thank you, we will add an explanation and the corresponding reference.

Link Seneviratne 2012a to the IPCC report does not work in the references

Thank you, we will correct this.

References:

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