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Interactive comment

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 Anonymous Referee 3 for the detailed, constructive feedback. We appreciate the suggested corrections and will address them in our revised version. Below we list our response (in bold) to the reviewer's comment *(in italic)*.

The objective of presented study is to investigate how precipitation (both summer and winter) and PET anomalies influence low flows across Switzerland both in typical and exceptionally dry years. In my opinion, authors provided detailed and important insight into climatic drivers controlling low flows based on data assessment from 380 catchments in Switzerland. In general, I found the results interesting, although the methods used are not novel. I found the main contribution in assessing a large number of catch-

ments which may help us to better understand why catchments sometimes behaves differently, which are main controls and thus what may happen in the future in a warming climate. Thanks to a large number of catchments covering different elevations, I think the results can by generalized to other regions, at least to those located in similar climates. In this respect, the results have an international value and may be very useful for hydrological community. Therefore, the results are important and certainly appropriate for HESS. However, I have some comments listed below, which need to be addressed before I can recommend the manuscript for publication. These comments are mainly related to methods and results interpretation. I hope that these comments will help authors to improve the manuscript.

Thank you.

Major comments:

Authors used winter precipitation to show how winter and snow conditions are important for summer low flows. Although this is an important aspect especially for higher elevation catchments, I am not sure to which degree authors were able to capture the snow effect by selecting just winter precipitation as a single variable. The winter precipitation does not tell us whether the precipitation is falling as rain or snow. This is, in my opinion, very important since snow contributes to runoff much later than rain and thus influence the seasonality of groundwater recharge and potentially summer low flows. Therefore, I am not sure whether the winter precipitation could correctly capture this issue well enough to make any general conclusion. Using some snow-related metrics (snowfall fraction, snowfall water equivalent, annual maximum SWE or similar) would be perhaps better to show whether there is (or is not) any relation. Therefore, I would be careful with interpretation going towards the role of snow. I do not see much evidence in authors results to make some conclusion, although several previous studies quantified this effect at different elevations.

We agree that winter precipitation is not an ideal proxy for snow. In the revised version we will more explicitly acknowledge that winter precipitation does not fully represent snow. In addition we will quantify the effect of solid vs liquid precipitation (e.g. by using a temperature threshold) and discuss if this better explains the low flow behaviors.

I am not fully convinced that assessing both winter and summer low flows is a good approach since the meteorological drivers are different for both of low flows types (see e.g. Harpold et al. (2017) for general overview). I found the mixing of both types throughout the manuscript sometimes a bit confusing. Authors did first analysis (Fig. 1 and Fig. 2) using annual low flows from all catchments (regardless whether they were summer or winter) and later they decided to further analyse only summer low flows. Although I would maybe prefer to focus only on summer low flows in the study, I accept the authors' decision to make first some results related to both winter and summer low flows together and later focus just on summer low flows. However, I am a bit confused how authors exactly proceeded to select catchments and years for summer low flows analysis (but maybe I only missed something). First, it seems that authors analysed all seasonal low flows occurred in the warm period for all study years (even in case that annual low flow occurred in winter). However, later (L306-307) it seems that authors completely excluded catchments/years in case that annual low flow occurred during winter. The latter approach could result in excluding many of the highest elevation catchments from the analysis (and thus it might lead to the conclusion that winter precipitation is not an important signature to influence summer low flows as noted in the previous comment). Therefore, please clarify how you proceeded. I would think that the first mentioned approach is more appropriate and should be used in the analysis (especially in case you are focusing on the role of snow or winter precipitation in addition to role of previous precipitation and PET).

In the revised version we will more clearly state what low flows are used to pro-

duce the results. We believe it is valuable to show both winter and summer low flows, because these are the actual lowest flows that occur in these catchments. Since summer and winter low flows are indeed generated by different drivers, we have to sometimes use a subset of all low flows to do meaningful analyses. In short, when analyzing summer low flows, we selected all low flows that occurred in July through November. We will better emphasize when and why we make this selection choice.

Regarding to the comment above, I think that mixing the summer and winter low flows in Fig. 2 (top panels) is not a suitable approach since the climatic controls are different for both type of low flows. As it is now, you are losing a lot of information, especially in higher elevation catchments, because you are trying to describe (mostly) winter low flows in these catchments using variables, which are not much relevant. Therefore, I would suggest to make the Fig. 2 just for June (or July) to November low flows. Then you would see, whether the precipitation and PET are important drivers for summer low flows even at highest elevations or whether the figure would suggest that there might by also something else (e.g. snow from preceding winter). In case you decide to keep Fig. 2 as is, please consider to split it into two figure, since the mixing of annual and summer low flows in one figure (top panels vs. bottom panels) is, in my opinion, confusing.

The purpose of Fig. 2a and 2b is to show that the importance of P and PET as drivers for annual lowest flows systematically changes with elevation. This is in our opinion a useful result that we also want to show in the revised version of this manuscript. We now realize that using a subset of these data for Fig. 2c and 2d may confuse the reader. Therefore, we will follow your suggestion of splitting the figure into two separate figures.

Authors calculated preceding PET as one of the main climate drivers. However, physi-

cally correct way is to use actual evapotranspiration (AET) instead of PET. I am aware, that calculating AET would not be such easy. Nevertheless, the relation between PET and AET is not always straightforward since higher PET do not necessarily mean higher AET (especially in lower elevation catchments with lower precipitation, higher water demand and thus lower water availability). Therefore, I would appreciate more discussion related to PET vs. AET interactions.

Obviously, we agree that AET is the physical process by which water leaves the catchment, that may lead to low flows (and could thus be considered a driver). However, the purpose of our paper is to infer the climatic drivers of low flows. AET is not a climate driver of low flow, it is the outcome of how climate interacts with the soil and vegetation in the catchment. Therefore, we choose (high) PET as a driver because PET is the climatic condition that drives AET (and subsequently low flows). In the revised version we will better emphasize our choice of PET as a driver over AET as a driver, and we discuss its limitations especially regarding the complementary relationship between AET and PET. Using AET would furthermore require an additional soil water balance model which adds uncertainty to the analysis.

Minor comments:
One of the conclusions is that low flows are controlled by either low precipitation or high PET or combination of both. This is not surprising since there are not many other options (at least for summer low flows in near-natural catchments). Therefore, I would rather highlight implications which arose from results, but which are not such trivial. In this respect, I would recommend to slightly reformulate the respective part of abstract, short summary and perhaps also hypotheses (line 75) to better highlight the novelty of your work.

Indeed, it is no surprise that PET and P drive most low flows. However, the

purpose of our manuscript is to show to what extent, and which characteristics of P and PET drive low flows, and how these vary spatially. These more detailed pictures of low flow drivers are nontrivial and we will try to better express their value in the revised paper.

Authors used winter precipitation, but this signature is not mentioned and explained in the methods section.

In the revised version, we will add the description of how we obtained winter precipitation also in the methods (rather than just in the later stages of the paper).

I suggest to create a map showing the location of catchments. I think that just a simple map of Switzerland with shaded DEM with points showing the position of catchments outlets would help those readers not familiar with Swiss hydrology. Maybe, catchment points might be coloured according to catchment elevation (or something similar).

We can add such a map. We will defer to the editor's advice on whether such a map is best included in the main paper or the supplementary material.

It is a bit questionable to describe the previous precipitation just using the sum of precipitation from the defined preceding period. The reason is that the importance of precipitation for the low flow at the specific date changes when going back in time (precipitation closer to the day with the low flow is more important than that occurred earlier). Did you also consider applying some kind of continuous precipitation index, e.g. current precipitation index CPI (Smakhtin and Masse, 2000)? I would appreciate more discussion on this issue.

We choose a time-window to reflect that low flows are typically not generated instantly, but are generated over longer time spans. We agree that precipitation during times closer to the actual low flow will probably often impact the flow more than precipitation during a longer time prior to a low flow. Alternative metrics such as CPI may account for this fact (to some extent) and their merit will therefore be discussed in the revised paper. However, precipitation in the periods immediately preceding low flows often does not significantly refill groundwater stores, and thus may have very little impact on the low flows themselves (they only result in a short peak in the hydrograph). Such effects are not captured by CPI.

L101: Please, add a brief information why you remove "unusually high annual low flows". I see the point, but it would be good to clarify it.

We will explain this in more detail in the revised version of the manuscript.

L147-149: Could you please add some references regarding this statement? Just to avoid speculations, since you cannot prove this based on your results.

We will add the appropriate references here.

Fig. 2: Here it is nicely shown that PET anomalies are relatively less important compared to precipitation anomalies (inter-annual variations up to 40 mm for PET, but up to -200 mm for precipitation).

Thank you.

L201-202: Maybe I missed something, but I do not see the described effect (wet years) from Fig. 3b

Wet years refers to years with higher low flows (Fig. 3b). We now realize this may be unclear to the reader and therefore we will make this clearer in the revised manuscript.

Fig. 3a: Why there are still some years when low flows are preceded by above-average precipitation and below-average PET (bottom-right quadrant)? Would the figure looks similar also for other than 30 days time-windows (e.g. 60 and longer)? You suggested some explanation in lines 201-205, but could you be more specific?

We will extend our description of why it makes sense that above-average P and below average PET anomalies are observed when (i) the seasonality of the flow regime outweighs the effects of shorter-term weather, and (ii) when very wet years occur (with high low flows).

Fig. 4: This figure shows both summer and winter low flows. However, earlier you stated that only summer low flows are analysed starting from Fig. 3 (L155-156). Therefore, could consider putting this figure as a Fig. 3 to be consistent. Additionally, I would maybe change the colour scale by using "cold" colours for cold months and "warm" colours for warm months.

As stated before, in the revised version we will better emphasize when and why we use summer vs. all low flows in various parts of the paper. We will evaluate if changing the color scheme makes the figure clearer.

L237: Please, specify the thresholds for "below-threshold" precipitation and "above-threshold" PET (maybe in methods as well).

We specified these thresholds in lines 251-252. In the revision we will also include this description in the main text (around line 237).

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Discussion paper

L286-287: This hypothesis would be correct only in case that most of winter precipitation would occur as snow. Therefore, not higher winter precipitation in general, but higher snowfall (snow storages) should lead to larger and later summer low flows (but only at high elevations with high snow storages). Please, consider reformulation. In this respect, you correctly pointed to the fact that winter precipitation sums do not accurately represents SWE (L299).

We refer to our earlier more detailed comment on how we address this limitation.

L325. I would maybe add "winter" to describe the precipitation in California. In contrast to humid catchments in Central Europe, the previous summer droughts in California were mostly driven by lack of winter precipitation (and snowpack).

We will add "winter".

Technical corrections
L95: Please, add the reference to RhiresD and TabsD products

We will add a Meteoswiss reference.

L106: I would not use the term "long-term" for time period of 18 years. Instead, I would directly specify from which time period the average has been calculated.

We will change this.

L142: I think you wanted to refer to Fig. 2ab

Thank you, we will change this.

Figure captions: A lot of text in figure captions (Fig. 2, 3, 5 and 6) is related to figure interpretation rather than figure description. In my opinion, these parts would better fit directly to the main text

We tried to include the main message of the figure in every caption. We believe this is informative to the reader.

L177: "Our previous results ...". I would remove "previous" since it implies something you did in some previous study. Alternatively, be more specific instead (e.g. "results shown in figure/section no. ...").

We will change this.

Fig. 6: Please, consider larger axis captions to increase the readability.

OK.

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