Dear Editor,

We hereby resubmit our revised manuscript entitled "Effects of climatic anomalies on low flows in Switzerland". We revised the manuscript according to the recommendations, we addressed all comments of the reviewers and the editor, and we hope that our revised version is now suitable for publication.

We thank you for the detailed explanation of your decision and addressed the points of concern. Below we list all editor and reviewer comments *(in italic)* **and our answers (in bold).**

Thank you for considering our revised manuscript, and we hope you now consider it suitable for publication. We appreciate your time and look forward to your response.

With best regards
Marius Floriancic

(on behalf of the co-authors Wouter R. Berghuijs, James W. Kirchner, Tobias Jonas, and Peter Molnar)

Editor Decision: Reconsider after major revisions (further review by editor and referees)
(26 Nov 2019) by Kerstin Stahl

Dear Marius and co-authors,
Thanks for your online-replies to the reviews. The manuscript has received differing reviews, recommending minor revision, two times major revision and one rejection. Based on the details of the reviews, I invite you to submit a substantially revised version of the manuscript for further consideration. The revised manuscript will then be reviewed again by at least two reviewers in order to decide further.

My editorial assessment based on the reviews in particular identified the following necessary major revisions:

1) Working out the sufficiently original contribution: Three reviews contain the phrase 'not surprising' when describing the results of the study. That itself does not hinder publication, but the added value of the findings has not been sufficiently worked out yet. R4 finds the hypotheses too weak, an assessment that is also reflected in R2 and illustrated by the summary written by Ryan Teuling's students. R2, R3 and R4 find the 'initial analysis' on seasonality particularly redundant with previous work, confusing, or at least not original enough to warrant the space it takes in the manuscript. This suggests that the necessary conclusion to continue with summer low flow may also have been based on a thorough review of existing work on low flow seasonality. In what way does the study go beyond previous work, e.g. the maps in HADES, the regime stability classification for the "Modulstufenkonzept" and all the underlying research? The reviewers do make some general suggestions, where to look for the added value, for example that "results [of merely measuring whether there is correlation] could benefit from an actual quantification of the relative contributions" or to "better use the added value of the large dataset to explain controls" or really take up the 'shaping' idea from the research questions. Please take the combination of these reviewer comments seriously into account in the revisions to prove progress by and original contribution of the study.

We now better emphasize the novelty of the work by the following main changes:

1) We removed Fig.1 and the results on "low-flow timing" and only report the timing based on existing literature in the introduction. We think that this change comes with two advantages. First, it will introduce the international HESS readership to the German literature that they probably are unaware of but that contains potentially interesting information. Second, it avoids that our analysis repeats the analysis of low-flow patterns that (mostly) also appears elsewhere in work by ourselves and others.

2) We now better explain in the introduction that previous studies have shown that low flows can be driven both by PET and P. In our opinion, the submitted revised manuscript provides additional deeper data-driven insight into the durations, magnitudes, and timings of the climatic anomalies that drive low flows, and how these vary across hundreds of catchments situated in diverse landscapes (topographies, soils, etc.). These more detailed insights about low-flow generation reveal aspects that are not systematically covered by the existing literature (which focuses on other dimensions of P and PET, or studies a smaller number of sites). Therefore, we believe that the provided results (and data) may be

useful for the hydrological community, as also already acknowledged by reviewers 1, 2, 3 and Ryan Teuling's student.

3) We now quantify the relative importance of P and PET in driving low flows (see section 3.5) using a multivariate regression between the low-flow magnitudes and the P and PET anomalies. This analysis is done for all years and for the years with the lowest low flows, and it highlights how PET increases in importance in the most severe dry years in our dataset. We also included an analysis of SWE in the revised manuscript, to better represent the effect of winter precipitation on summer low flows.

4) We now also created a new discussion section (4) in which we further discuss the novelty and implications of our results.

2) The reviews also raised concerns about terminology, in particular the use of 'extreme'. I agree that this terminology requires a specification, what frequency this refers to and that 'low flow' and 'streamflow drought' need to be distinguished. Regarding terminology issues pointed out by the reviewers, please make sure your terminology meets international conventions or clearly explain and reference any Swiss terminology used. For example consider the WMO Manual on low flow estimation (Gustard and Demuth 2005) or the textbook on low flow and drought estimation methods by Tallaksen and van Lanen (2004) . Also I suggest to draw parallels to recent research on drought propagation that correlated SPI and SPEI (essentially not much different from your non-standardized precip accumulations) as well as studies using basin properties/catchment characteristics for regionalization in general.

We addressed this issue by only referring to low flow conditions in this paper (and not droughts, deficits, and extremes), and by providing a definition of low flow for any of the analyses we performed. Some insights from past drought studies are still relevant to consider (e.g., in the introduction) so some of these studies are cited. We also included a more complete reference to the work on drought propagation (see lines 64-68).

3) Two reviewers (and I) also I find the mixed results and discussion confusing as it is not easy to distinguish plain results against wider interpretation with relation to other studies and current debate. The reviewers and I will review this very carefully again and I would highly recommend changing to a clear separation of *the two as part of the revisions. It may in fact help demonstrate the added value of the analysis (see comment 1).*

We have now separated the results and discussion sections.

4) Subjective choices made, incl. the >3 sd exclusion, widow sizes for P accumulation, or the 1200 m.a.s.l., are not acceptable without good argument by physical reason or empirical proof. I expect the revised manuscript will either provide sound reasons for these decisions or test the results' sensitivities.

We understand that these choices look (and are) to some extent subjective.

First, we removed former Figure 1 (the part where we use 1200 m a.s.l. threshold was only chosen for showing different low-flow timings). Thus, this threshold is not relevant anymore.

Second, it is important to note that none of the (overall) results would change if these thresholds were modified. For example, while the 3-sigma rule is a widely-used tool in statistics to remove outliers from a dataset (Pukelsheim, 1994), using a bigger (e.g. 4-sigma) or smaller (e.g. 2-sigma) yields a similar overall outcome.

Third, we calculate the P and PET anomalies for time-windows ranging from 7 to 182 days (i.e., 7, 14, 30, 60, 90, 120, 182 days) which spans the whole time-period that seems physically most relevant when discussing the potential effects of P and PET departures from the norm on low-flow generation. In this sense the time windows are not arbitrarily chosen and are an integral part of our analysis. We better explain this now, and acknowledge that some very long-term memory effects may not be fully captured by this approach.

5) I take the liberty to add one missing consideration that has not been picked up, but that I find crucial to be addressed for the credibility of the study: magnitude and seasonality of observed low flows can be altered by water management operations, river regulation, sewage treatment plant return flows, the filling of reservoirs and minimum flow release from them, hydropeaking etc...(e.g. Pfaundler and Wüthrich, 2006). All these issues are very prominent in Switzerland and metadata on e.g. minimum flow releases and water transfers is available and can and should be used in any analysis. At the moment, the study considers *only natural climatic causes of low flows. It also does not consider the effect of trends and jumps due to human interventions over the study period. The minimum work to do is to carefully test for those, remove any records that show signs of human impact and carefully discuss how common water use and regulations may affect (intensify? compensate?) the identified climate-low flow relations.*

We agree that human activity often influences low-flow magnitudes, especially in a place like Switzerland. We also point out that no comprehensive database exists with meaningful metadata of human influence on low flows for all of the 380 Swiss catchments. To avoid that human impacts alter our results we did the following additional checks and report them in a new discussion section in the revised manuscript (section 4.3):

- **When compiling the dataset, we removed all catchments where there was obvious alteration of the flow regime (e.g., by screening the hydrographs). We removed for example all stations with hydropeaking or large reservoirs. That is also one of the reasons why the Alpine areas of Switzerland are less represented in our dataset (as they are e.g., within the report of the "Modulstufenkonzept" that is referenced by the editor).**

- **The absolute values of low-flow magnitudes are influenced by human activity. We do report and analyze the absolute magnitude of climate anomalies throughout our work, but only in the multivariate GLM model do we use the absolute magnitudes of low flows. In this case the predictive power is low, which shows that the absolute magnitude of low flow is poorly predicted by climate anomalies alone, and is more dependent on catchment attributes, and yes perhaps regulation. In the non-parametric correlations of low-flow magnitudes with P and PET we use the rank order of annual low-flows in the nineteen-year study period. There are good reasons to believe that the rank order of low flows is less influenced by human activities (i.e. the driest years will likely still have the lowest low flows, even when flows are partly managed). While individual cases can (and will) have some human imprint, we emphasize that we never establish results based on individual connections between anomalies and low flows (which can be highly affected by human influences) but rather focus and infer findings from repeating patterns across diverse conditions (which are less likely to be impacted by humans).**

- **In addition, in the revised version of the manuscript we also tested the sensitivity of our results by calculating the correspondence of low-flow magnitudes and climate-anomaly magnitudes for the 20% of catchments with the most human influence and the 20% of catchments with the least human influence (estimated using CORINE Landcover data as a proxy of water use/regulation). This analysis shows that the overall results do not change substantially, whether a catchment is near-pristine or heavily human-impacted.**

Technical editorial comments

For the revision, please consult the manuscript preparation instructions - symbols and mathematical notation and avoid multi-letter variable names, in particular see suggestion of not using "ET" (or here "PET") in the HESS instructions.

We agree that in general using multi letter abbreviations can be confusing. However, PET seems to be the exceptions to this rule, since it is also widely used in literature (including HESS: Gu et al., 2020 - https://doi.org/10.5194/hess-24-451-2020; Jansen & Teuling, 2020 - https://doi.org/10.5194/hess-24-1055-2020; Callow et al., 2020 - https://doi.org/10.5194/hess-24-717-2020; Weerasinghe et al., 2020 - https://doi.org/10.5194/hess-24-1565-2020; Alam et al., 2020 - https://doi.org/10.5194/hess-24-735-2020; Qiu et al., 2020 - https://doi.org/10.5194/hess-24-581-2020; Jiang et al., 2020 - https://doi.org/10.5194/hess-24-1251-2020; Therefore, we will make the suggested change to alternative symbols, if the editor still insists on this.

Please mitigate some of the excessive citations (parentheses with eight references are not useful). Ideally be more specific about the relevance of the individual references to this study.

We considered this comment in the revision.

Please note that manuscripts "in review" cannot be cited and will have to be removed from the references.

We removed this citation.

Figure captions: As also noted by reviewers, add legends to figures rather than writing long descriptive caption texts and in particular shorten the captions by removing duplicate legend/caption text and long explanations.

We now have legends for Figures 2a, 3, 5 and 8; all other figures show always the same two things in blue and red (precipitation and PET). For clarity we still use extended figure captions for a good reason: 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.

First letter of axes labels should be capitalized.

We changed this.

Regarding R1's comment on the data statement and your reply: that's fine - please make sure that a list of station IDs and the address/who to contact for each station ID is included with the published dataset and/or as supplement.

Thank you.

References

Gustard A., Demuth S. 2008: Manual on Low-flow Estimation and Prediction. Operational Hydrology Report No. 50. WMO-No. 1029. Geneva.

Pfaundler M., Wüthrich T. 2006: Die Saisonalität hydrologischer Extreme. Das zeitliche Auftreten von Hoch- und Niedrigwasser in der Schweiz. Wasser Energie Luft 98: 77–82.

Tallaksen L.M., van Lanen H.A.J. 2004: Hydrological Drought: Processes and Estimation Methods for Streamflow and Groundwater. Developments in Water Science 48. Elsevier, Amsterdam / Oxford.

We adjusted the manuscript according to the reviewer comments below. We already responded to these reviewer comments during the online discussion, but we repeat those responses here for the sake of clarity and completeness. However, some of our answers do not reflect the changes we ultimately made, therefore we report the original reviewer comments *in italic*, **our original answers in bold with the modifications indicated by tracked changes.**

Reply to Anonymous Referee #1

We thank Anonymous Referee #1 for the positive and 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 italics***).**

Floriancic et al. explore how anomalies in precipitation and potential evapotranspiration shape the occurrence and magnitude of annual low flows across 380 Swiss catchments. The varying time period for the precip and PET anomaly calculation, with the end point being the day of the low flow, is a novel method for completing the joint analysis of climate drivers on annual low flows. I found the conclusions to be well-supported by the data. I particularly like how Figure 6 illustrates the role of long periods of PET in development of extreme low flows. The paper is well-written, and the methods are clearly outlined. I find this manuscript to be a significant contribution to the field, and I recommend it for publication in HESS. I have the following few minor/technical comments that should be easy to address:

Thank you.

L100: change "There were years whose lowest" to "There were years when the lowest"

We ~~will correct~~have corrected this.

L140-142: Sentence starting with "However," incorrect figure reference at end of sentence – should be Fig. 2a&b.

We ~~will correct~~have corrected this.

Figure 4: Suggest changing the color-scheme to something that is color-blind friendly.

We ~~will evaluate~~have changed the color options for Figure 4 to~~ improve its visibility and~~ make it colorblind friendly.

L308-311: Based on the winter precipitation versus annual low flow analysis completed in this study, I don't think this statement is sufficiently supported. As stated earlier in the paragraph, winter precipitation does not always accurately represent SWE. With such a range of catchment elevations (and thus climate conditions), a more detailed analysis would be needed to determine the impact of SWE on summer low flows.

We agree that the role of SWE in summer low flows cannot be directly inferred with the available dataset. In the revised manuscript we ~~will point out that~~ have conducted an extra analysis in which we ~~compare~~related the ~~amount of previous winter precipitation (rather than~~average SWE~~) to~~ on 1 March over all catchments to the magnitude and timing of summer low flows. This extra analysis showed that also SWE, like winter precipitation, is very weakly related to low-flow magnitudes in catchments that experience annual low flows in summer. Therefore, although snow cover may affect low flows in higher altitude catchments (as some previous research has shown), we find little evidence of this effect in low altitude catchments in Switzerland.

L314: "most work has discussed individual drivers" – statement suggests that some work has analyzed multiple drivers of low flows, but no studies are referenced here. Section should reference the relevant studies listed in the introduction on L68-70.

In the revised version, we ~~will list the~~added appropriate citations here.

L319-321: I struggled to directly relate these broader implications statements to the results. How will the impacts be different between spring and autumn? What are the different implications of PET anomalies in May versus September? These implications are likely obvious to the authors, but on the first read through – I did not make the connection.

In the revised version, we ~~will~~ emphasize how antecedent conditions with regard to soil moisture state and subsurface water availability, and the water demand by vegetation in a catchment, matter. It is not sufficient to look at (the combination of) anomalies only, as the same combinations may occur throughout the year with different results, depending on soil moisture and vegetation state. Therefore, it is also necessary to include the timing of these anomalies together with the general climatology of a basin~~.~~ (see new discussion chapters 4.1 & 4.3)

L350: Data availability – Rather than making the data available "upon request", I would encourage the authors to provide open access to the compiled data used in their analysis (streamflow, catchment-averaged weather and climate conditions, PET, etc.) through an archiving medium such as figshare.com. While not essential, it would be beneficial.

We will publish the dataset in the "open access" ETH library collection. However, unfortunately we cannot supply the full daily mean streamflow dataset as they are only available at the Swiss cantonal authorities and the Swiss Federal Office of the Environment upon request. Nevertheless, our dataset will include the date of low flow occurrence (2000 – 2018) and the magnitude of the annual lowest flow (Qmin), and we will include a file with all contact information for the relevant organizations where the streamflow time series can be obtained.

Reply to Anonymous Referee #2

We thank Referee #2 for the constructive feedback. We appreciate the suggested improvements and will address them in our revised version. Below we list our response (in bold) to the reviewer's ~~comment~~comments (*in italic*).

In this work, the authors assess how anomalies in precipitation and potential evapotranspiration shape occurrence and magnitude of annual low flows for 380 Swiss catchments comparing preceding precipitation and evapotranspiration for different periods to the annual minimum flow. After an initial analysis of annual low flows, the authors decided to focus on summer low flows for the rest of the analysis. The paper is clearly outlined and easy to read. I agree with reviewer 1 that I particularly like Figure 6 illustrating the increasing role of PET during development of extreme low flows. However, different to reviewer 1 I have some major comments that I find important to address before publishing:

Thank you. Below we address the comments point-by-point.

Major comments:

Summer and winter low flows

After an initial analysis of annual low flows, the authors decided to focus on summer low flows for the rest of the analysis. This analysis takes relatively much of the full paper both in text and in Figures (1, 2 and 4). While I agree that it is important to differ between summer and winter low flows particularly when analyzing the drivers, I do not see that the results of general occurrence are new (e.g. Smakhtin 2001 and references therein, Fiala et al. 2010 and basically all runoff regime literature for Switzerland in particular e.g., Weingartner and Aschwanden, 1992) nor that they deserve this weight in the article. I suggest to minimize this to the introduction refereeing to the relevant references and remove Figure 1 and focus in Figure 2 on only the summer events or split in to summer and winter low flows at the beginning and then assess both for winter how snow (or precipitation and temperature) shapes low flow occurrence and magnitude and for summer how precipitation and evapotranspiration shape low flow occurrence and magnitude

We ~~will improve~~have edited the introduction ~~of~~regarding summer vs. winter low flows and ~~include~~included further literature as ~~outlined~~requested by Referee 2. ~~While we agree that there are~~

~~contributions pointing out~~We also removed Fig.1 from **the** ~~general occurrence of winter and summer low flows, we consider it valuable~~manuscript **to** ~~show that the occurrence of summer vs. winter low flows can be related to elevation (1200m, Fig. 1) and that the differences of summer vs. winter low flows are also detectable when analyzing the climate anomalies (Fig. 2a and b).~~avoid repetition of previous findings. **We furthermore use** (the new) **Fig.** ~~2a~~1a **and b to argue why we relate summer low flows to precipitation / PET anomalies, and expect these relations to hold for a wide range of low-elevation catchments**

More focus on the shaping

Instead of counting and presenting summer and winter low flows representing the annual low flow, I think it would be more interesting to try to add on the shaping of the low flows caused by precipitation and evapotranspiration. For instance, in addition to correlation between precipitation and PET separately on extreme or less extreme low flows it would be interesting to look at their combined effect. And then how much of the combined effect could be attributed to precipitation and to PET. This would allow a better relative quantification and really add to the values of this study.

We emphasize that Fig. ~~3~~2 **and Fig.** ~~6~~5 **are studying the combined effects of P and PET on low flows already. In the revision we** ~~will try to make the partitioning of~~partitioned **the effects of the two drivers more** ~~explicit, for example~~explicitly, **by** ~~multivariate regression~~comparing the predictive skill of bivariate regressions **between the** ~~low-flow magnitude (anomaly) and the two~~individual **climatic** ~~driver~~ **anomalies**~~.~~ and Q_{min}, and a multivariate stepwise GLM regression between a both anomalies at all durations and Q_{min}. **We** ~~agree~~show **with** ~~the reviewer~~this analysis **that** ~~it is interesting to quantify the relative roles~~while precipitation explains most **of** ~~P~~the variability in Q_{min} overall across all years, in very dry years the situation is opposite **and PET** ~~for low-flow generation.~~becomes a more important predictor than P, especially at shorter durations. However, it has to be understood that the predictive power of the multivariate stepwise GLM model for Qmin is low overall.

Choice of summer months

The authors focus at extreme low flows and the preceding conditions and chose 2003, 2011, 2015 and 2018 as the relevant years. The drought in 2011 was finished for most catchments before July, I urge the authors to look into the data and if so, adjust the analysis by either treating 2011 differently, i.e. not *considering it a summer but a spring low flow and or change the analysis period for all years that was defined by the authors Jul-Nov.*

We agree that the drought in 2011 is predominantly occurring in spring throughout the Swiss catchments (i.e. only 143 of 380 catchments have their low flows in July through November in 2011). ~~**We choose the 6-month period July-November because most low flows occur during this period. However, in some cases low flows do occur outside of this window (e.g., 2011). When we calculate the statistics of the main figures that use the 6-month window (e.g. Fig. 2 c&d, 3, etc.) the results do not change significantly because our chosen period captures almost all low flows. In the revised version we will more clearly acknowledge that not all low flows are captured by this window, but also that this choice does not affect the results significantly.**~~**We changed the period that we consider for warm-season low flows in the revised version of the manuscript to May through November.**

Terminology

There is a seamless transition from "drought" to "low flow" and mixed use of "droughts" and "deficits" while the citations support both. I find this mix critical since already deficit (even when regularly/seasonally occurring) causes low flows but meteorological droughts are larger deficits than normal, i.e. the regularly/seasonally occurring deficit. Please, revise the introduction to distinguish clearly between these. This would help the reader in the analysis that follows.

In the revised version we will better distinguish between drought, deficit and low flow. In short, we will minimize the use of the term (and references to) drought, since low flows are not necessarily droughts.

Likewise, the authors use often only the term "low flow" when actually referring to "extreme low flow". This can result in wrong statements (e.g. L72 "low flows are exceptional flow conditions" or L186 "triples the chance of an annual low flow"). And I would ask the authors to revise and correct the usage throughout the manuscript.

In the ~~revision~~revised version of the manuscript we ~~will be clearer when~~emphasize that we ~~refer to a (typical) low flow versus an~~ only discuss the annual 7-day lowest flows throughout the manuscript and we fully avoid the term "extreme~~ low flow. In addition, we will be clearer what we mean when we use "extreme", since~~" to enhance clarity. We agree that not being explicit about this ~~can lead~~could have led to misinterpretation (e.g. L72 in the original manuscript).

Mixed results and discussion section

In my opinion results and discussion should be separated. This allows to focus on the results. Only then what we can learn from the results and where we might to be a bit more careful, then also relate and compare to what was done elsewhere and where limitations and possibilities lay. In the present form the manuscript mixes these aspects and is more difficult to search for s specific result /argument this way.

~~We understand that separating discussion and results may in many cases be a good choice. However, we tried both options in preparing the manuscript and found the chosen option to work best, because it is easiest for the reader to see the connection between the results and their interpretation. In the revision, we will go through the entire manuscript to ensure it is as clear as possible.~~
We have separated these two sections in the revised manuscript.

Minor comments:

L25-27 Remove this sentence for the abstract

This sentence may sound trivial but is needed for the logical flow of argument in the sentence following it. Therefore, we prefer to keep this sentence. ~~We will consider reformulating~~We edited the abstract to improve its clarity.

L40/41 is "landscape" only including surface features? Maybe use rather catchment properties".

We use landscape because we thought it would make clear that it does not include all catchment properties (such as its climatic conditions). Landscapes extend into the subsurface~~; this is implied in our statement in line 40, but we will now explicitly add this in the revision~~.

L72 this statement is not correct they occur every year. Make clear that it is about extreme low flows here!

In the revision we ~~will choose~~chose more precise wording which reflects that the low flows we are studying are "annual ~~extremes~~lowest flows" and not ~~every annual extreme is~~ necessarily ~~an extreme~~extremes in the long-term record. We avoid using the term "extreme low flow" throughout the revised version of the manuscript.

L92-94 a summary table (maybe only in the supplementary material) would be helpful

~~**We will include a table in the supplementary material; the**~~**All data ~~for all catchments~~(and a summary) will be ~~made available~~provided through the "open access" platform of the ETH library.**

L121 1200m asl, why this threshold? Why not a range?

~~**We use a threshold to split the dataset into two groups (below 1200m asl and above 1200m asl). This threshold accurately reflects what type of low flow (seasonality) is expected within this dataset. We do not see how using a range would improve this observation.**~~

We removed this part from the revised version of the manuscript.

L130 remove "However, this remains to be tested".

OK.

L147-150 For these low flows, people are usually prepared. Here it would be interesting how much more extreme are others. If it was due to a lack of precipitation, the signal should be visible in spring melt.

We agree that such winter precipitation deficits can have effects on flows later in the snowmelt season, and will likely be visible in the data. However, the aim of this paper was not to explore all these hydrological connections, but rather focus only on the climatic conditions leading to the lowest flow in the year. This remains an interesting suggestion for further research.

L154 "suggesting…" could be also formulated that lower elevation Swiss catchments could be representative sample for global summer low flow? (and maybe not even global but for humid regions with seasons?); This part would better fit in the introduction or methodology/catchment section.

~~**We put this statement here because we discuss our results and their implication here. Making this statement in the introduction is leapfrogging ahead, because we have not characterized the seasonality of Swiss low flows at that stage.**~~

This part was removed from the revised version of the manuscript.

L161 altitudinal variation in 30-day anomalies: could that be influenced by catchment size? A large catchment might not react on such an anomaly a small catchment not anymore if the driving anomaly is at the beginning of the period.

We tested if catchment size affected the altitudinal variation. While such effects can be expected, no clear signal was found, probably because the catchments are relatively small (< ~~519km~~~~\textsuperscript{2~~519km^2, with a median of ~~74km~~~~\textsuperscript{2~~~~). We will discuss this in the revised paper.~~74km^2.

L174 "substantial site to site variability" can this be quantified?

To clarify what this variability refers to, we now explicitly refer to Fig. ~~2c~~1c and ~~2d~~1d for the reader to look at the spread. We also add the range in the text.

L181 can these 8L189-196 These results are not surprising (the authors refer even to studies that found the same) but nicely illustrated and supported by the data. However, it would add to the value of this study to quantify the contribution of precipitation and evapotranspiration.

We agree that these results are maybe not surprising, as they have been shown for individual cases (as referenced earlier in the paper). Our work improves past studies by (a) providing a large dataset which shows the variability and consistency in low flow-climate relations among basins; (b) quantifying the effect of duration of the climatic anomalies required to generate the extreme low flow events; and (c) separating the effects of precipitation and PET. We believe this provides a more robust picture of otherwise intuitive relations.

L189-196 Consider also to compare to Stahl et al. 2010

We ~~will~~have put our results in context of the findings of Stahl et al. 2010.

L196-197 delete sentence

It is unclear to us why this sentence should be deleted.

L203-204 it is possible to avoid that by the study design (see also my major comment on seasonal split)

We agree that it is possible to select data such that only the drier years are kept. However, that is not the purpose of our study. We rather discuss that not all annual low flows are created equal.

L222-228 that depends on how one looks at the drought: is the same scale as in the references used? The effect was also found in low flows but maybe not in the metric "annual low flow", again distinguish between drought and low flows (see also major comment on terminology above) Figure 4: Looking at the figure makes me wonder how/if the regulated catchments might influence the pattern presented. Could that be picked up in the discussion?

~~We will change this in the text, to clearly distinguish between "droughts~~In the revised version we avoid the term "drought" and only refer to "low flows". We ~~will~~ discuss the influence of flow regulation on low ~~flow timing~~ flows in the revised manuscript and provide an additional analysis in the new discussion section 4.3.

L241 How brief since the study is about anomalies?

We will remove this.

Technical comments

While I find that active voice generally a good choice, I would avoid starting every sentence with "we" (e.g. 2.1 but also elsewhere), please revise.

We ~~will consider how~~have tried to reduce the prevalence of sentences beginning with "we" (although they are usually the most compact, clear and direct way of expressing things).

L22 "dry years saw" please rephrase

We ~~will change~~have changed this.

L28 redundant, delete either "could" or "potentially"

We ~~will delete~~have deleted "could".

L44 "(PET)" -> ", PET"

We ~~will change~~have changed this.

L44 remove "should" and "usually"

We ~~will delete~~have deleted "should" and "usually".

L46 remove "made"; split sentence: ": : :to a catchment. Hence a sustained: : :"

We ~~will change~~have changed it accordingly.

L49 Make a new paragraph

Ok.

L64 "smaller" = "lower"?

We ~~will change~~have changed this.

L66 "comes" -> "occurs"; remove "the" before summer and before winter

We ~~will change~~have changed this.

L79 "useful" = "suitable"?

We ~~will change~~have changed this.

L94 "quantified" -> "estimated"

Ok.

L100 "years whose lowest annual flows were much" -> "years with lowest annual flows much"

We ~~will change~~have changed this.

L103 "low flows" -> "extreme low flows"

We ~~will change~~have changed this.

L224 remove "the" before summer and before winter

We ~~will change~~have changed this.

L238 “more strongly” -> “higher” (also in L244)

We ~~will change~~have changed this.

L259-262 rephrase to make more concise

We ~~will rephrase~~have rephrased this sentence.

Reply to Anonymous Referee #3

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~~comments (*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 catchments 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~~In the revised version we added a new analysis with SWE on 1 March as a proxy for snowmelt potential affecting low flows. This analysis confirmed that also SWE, like winter precipitation, is weakly related to low-flow magnitudes in catchments that experience annual low flows in summer. Therefore, although snow cover may affect low flows in higher-altitude catchments (as some previous research has shown), we find little evidence of this effect in lower-altitude catchments in Switzerland.

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 produce 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.~~
This is a very good point, and indeed in the main part of the paper looking at the effect of climatic anomalies we focus exclusively on catchments that have the lowest annual flow in summer and autumn, thereby excluding most high-altitude catchments. This is one of the reasons why winter precipitation (and SWE) do not affect low-flow magnitude. We explain this in the revised paper more clearly.

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~~1a and ~~2b~~1b 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~~1c and ~~2d~~1d may confuse the reader. Therefore, we will ~~follow your suggestion of splitting~~better emphasize the ~~figure into two separate figures~~different samples used to create the upper and lower panels of Fig.1.

Authors calculated preceding PET as one of the main climate drivers. However, physically 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~~have added the description of how we obtained winter precipitation also in the methods (rather than just in the later stages of the paper). We also added a new analysis with SWE on 1 March to quantify the potential effect of winter precipitation and snowmelt on Q_{min}.

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~~have added 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~~This is explained 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~~The appropriate references ~~here~~were added.

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~~edited this ~~clearer~~statement 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~~have extended 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 original manuscript. In the revision we ~~will~~have also ~~include~~included this description in the ~~main text (around line 237).~~caption of Fig. 4.

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~~addressed 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~~have added "winter".

Technical corrections

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

We ~~will add~~have added 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~~have changed this.

L142: I think you wanted to refer to Fig. 2a&b

Thank you, we ~~will change~~have changed 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~~have changed this.

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

OK.

Reply to Anonymous Referee #4

We thank Anonymous Referee #4 for the feedback. Below we list our response response (in bold) to the reviewer's comment (*in italic***).**

In this paper, the authors assess to what extend precipitation and PET anomalies trigger summer low flow events in Switzerland. The assessment employs Spearman correlations between low flow magnitude and climate anomalies in P and PET aggregated over varying lead times before the peak of the low flow event. The correlations are overall weak, but still indicate that most low flows arise from the compound effects of precipitation and PET anomalies, with longer and larger anomalies related to more extreme low flow events, as one would expect. The assessment of lead times before the peak of the event is not new (e.g. Fangmann and Haberlandt, 2019 on a monthly time scale), but indeed appropriate to assess the genesis of events.

Indeed, drivers of low flows have been studied before (as reflected by the citations, including Fangmann and Haberlandt, 2019), and the result that both PET and P are important for low flows may not be a big surprise. However, the paper provides insight into the durations, magnitudes, and timings of the anomalies that drive low flows, and how these vary across hundreds of catchments situated in diverse landscape conditions. These more detailed insights about low-flow generation reveal aspects that cannot just be derived from intuition. In addition, the aspects of low flows that we discuss are not captured by previous studies, because they study other regions, and/or they study different aspects of low flows. Therefore, we believe that the provided results (and data) may be useful for the hydrological community.

While the paper is generally easy to follow, the paper appears to suffer from weakly formulated research question and consequently from a limited scope of the study. The results remain superficial and do not provide sufficiently new insights in low flow generation in Switzerland. I therefore cannot recommend the paper for publication in its present form. I will provide detailed feedback below, which I hope to be useful for the authors for further elaborating the paper.

In the revised version we ~~will sharpen~~have sharpened the research questions, to make them more specific and clearer. Since the detailed comments on this issue are discussed below, we refer to our detailed responses there on how this ~~will be~~was done.

Weakly formulated research questions and and consequently from a limited scope of the study. The results remain superficial and do not provide sufficiently new insights in low flow generation in Switzerland. I therefore cannot recommend the paper for publication in its present form.

Below we respond to the detailed comments that refer to this concern.

Science question

Science questions (or hypotheses) of this paper (Line 73-75) are formulated in a way that everybody would immediately agree: There is little doubt that low flows will typically occur after anomalous weather conditions, and that most extreme low flows will be associated with the most extreme weather conditions. This leads directly into a quite superficial analysis and weak conclusions. I urge the authors to sharpen the science questions and, accordingly, the study design, in order to gain more significant insights in how precipitation and evaporation together generate low flow events in Switzerland. I agree with Referee 2 that the focus of the paper should be much more on the interplay of the two meteorological drivers. And their relative importance for events with different time of occurrence within the summer/fall low flow season.

We point out that lines 73-75 in the original manuscript do not represent our science questions. However, we ~~will rephrase~~have rephrased the text to make this distinction between "introductory text" and "science questions" clearer.

In the revised version we ~~will sharpen~~have sharpened the actual science "questions" ~~(listed~~ in ~~lines 81-85). For example:~~ines 113ff "We investigate (~~i) to what extent low flows are driven by~~ a) how precipitation ~~anomalies,~~and *PET* anomalies~~, or their combined effects, (ii) what magnitudes~~ separately and jointly shape the occurrence and magnitude of ~~climate anomalies are leading to~~ low flows~~, (iii) what~~ across Switzerland, (b) which durations of ~~climate anomalies are typical for low flows, (iv) how~~ these ~~climate anomalies vary across the Swiss landscape, (v) how these climate anomalies vary with~~anomalies have the ~~severity of the~~

strongest impact on low-flow ~~event."~~occurrence and magnitude, both in typical and in exceptionally dry years, and (c) how winter precipitation and snow packs influence the magnitude and timing of summer low flows."

None of the above questions is answered in previous studies for our study region. In addition, we ~~will update~~have updated all of the results and discussion paragraphs to better present the results and their implications. In particular we have added a new analysis in which we objectively compare how much of the total predictability of Q_{min} comes from precipitation and how much from PET. With this analysis we show that while anomalous precipitation is the overall dominant climatic driver, in the most driest years of the record, the relation switches and PET becomes the dominant driver for explaining variability in Qmin. This addresses the interplay of the drivers that the referee is referring to.

Methods

The paper also suffers from weakly defined analyses. The methods section does not provide all necessary methodological details; they pop-up in a mixed results and discussion section. This makes analysis rather ad-hoc and hampers a well-structured assessment of the research question. I strongly advocate organizing the paper into clearly separated methods, results and discussion sections to foster a transparent, indepth assessment.

Apologies for the confusion. In the revised version we ~~will ensure~~made sure that all methodological aspects are already explicitly mentioned in the methods section.

In the following I review the used methods found in the results section.

In Section 3.2, the purpose of this "first correlation analysis" is not clearly defined (ref. also to the vague section title). The section assesses the correlation of 30- day-anomalies. For what purpose the time window has been chosen, and what may analysing 30 days before the event tell us has not been indicated.

The purpose of section 3.~~2~~1 is to reveal what magnitude of climate anomalies are typical for low flows, how this varies between P and PET anomalies, ~~how this varies with elevation,~~ and whether P or PET appears to be more important. In the revised version we ~~will introduce~~have introduced these purposes more clearly, both in the methods section, and the results section. We ~~will~~have also ~~try to provide~~provided a more quantitative perspective on the P and PET partitioning~~.~~ (see above).

The purpose of choosing a 30-day window is to reflect that low flows are generated during a prolonged period of anomalous climate. We show the results for 30 days, but emphasize that other time~~-~~ windows (from 1 week to 120 days) yield broadly consistent results.~~ We could provide such in supplementary materials.~~ We choose 30 days as the result to present, because 30 days (as later shown) is the time window which explains most typical low flows (Section 3.~~4). These changes will also lead to updates in the text of manuscript that address these additional analyses and explanations.~~3).

Section 3.4 Duration of climatic anomalies – The analysis of durations of anomalies before the peak of the event is largely depending on very short interruptions of the climate anomaly that have no effect on streamflows. Some pooling would be necessary to filter out disturbances in this type of analysis. The second analysis based on various time windows is more robust, and the most insightful analysis of the study.

We acknowledge that short (irrelevant) interruptions may affect the determined length of a climate anomaly. To address this we ~~do multiple things. First, we~~ use a 10-day moving average of time series to filter out short duration interruptions. ~~Second, for the revised version, we plan to calculate these results also using other time windows to test their sensitivity. Third, these limitations and the sensitivity of the results will be discussed in the revised version.~~

Section 3.5 The role of winter precipitation is not a pertinent research question, it is well-known that in an Alpine environment it is rather snow-storage than accumulated precipitation that shapes summer low flow with respect to timing and magnitude. Analysing winter precipitation (instead of snow storage and snow melt) has not the potential to lead new insights in the low flow generation process in Switzerland.

As pointed out by previous reviews, winter precipitation is not always a robust proxy for winter snowpacks. We ~~will change~~have changed our discussion of the role of winter precipitation for summer low flows accordingly. We would like to emphasize that it remains largely unquantified how winter conditions (either snow specifically, or both snow and winter rain) affect low flows across the Alps. This is clearly important information as we all agree that in many high Alpine ~~landscapes~~catchments winter conditions can shape summer low flows. In addition, in the revision~~,~~ we ~~will quantify~~use SWE on 1 March in all catchments to show that also stored water in the ~~effect~~snowpack ready for melt in spring is not a strong predictor of ~~solid vs liquid precipitation (e.g. by using a temperature threshold) and~~

~~discuss if this better explains~~low flows in catchments where the ~~low~~ lowest annual flow ~~behaviors.~~is in summer (low-altitude catchments in our dataset). See also answers to previous referees on this topic.

Specific comments:

L 41: It is not either climate, or catchment, but the combined effect of meteorological drivers and catchment functioning that determines streamflow.

We obviously agree that both climate and the catchment itself shape low flows (as we tried to convey in the original text). ~~We will see how we can rephrase~~We have rephrased this to avoid confusion.

L68: Contradiction to "the effects of evapotranspiration on low-flow occurrence and magnitude have received relatively little study" (two paragraphs above).

In the revised version we ~~will rephrase~~have rephrased this to make clear what aspects of ET have not received much attention~~, rather than~~ to ~~make the generic statement we currently have~~date.

L72 Sentence does not make much sense.

In this sentence we aim to explain why focusing on climate anomalies makes sense. ~~We will consider how to rephrase~~We have rephrased it to avoid confusion.

L73 ff: Please revise hypothesis (better formulate them as science question(s) and objectives of the study. Avoid duplication of the overall aim into one objective (currently objective a).

As stated earlier, this is not the hypothesis we test in the paper. We now realize that this confusion can arise (probably because we used the word "hypothesize" in this sentence). We ~~will reformulate~~have reformulated this statement to reduce the chance of this confusion.

L114: One sentence methodology, apart from the definition of the anomaly measures, is definitely too short.

In the revised paper we ~~will add some~~have added text that explains the rationale of this analysis.

Section 3.1: This is prior knowledge and should go into the introduction

~~We agree that some of the aspects in Section 3.1 can already be stated in the introduction. We, however, like to still repeat some of these aspects to put the Swiss results into context of other studies. We also emphasize that the presented results in section 3.1 (e.g. the 1200m split) are not part of existing literature and should therefore not be presented in the introduction.~~

We agree. We removed Section 3.1 from the revised manuscript and we state the most important points in the introduction.

L222: Statement is not true. What the cited papers say is that large parts of Europe were affected by the drought events of 2003 and 2015. But papers also show how different timing and magnitude of events were across Europe.

We now realize that we oversimplified the spatial coherence reported in previous studies. We change the interpretation of our results accordingly, by not stating that Switzerland is necessarily in contrast with other regions of Europe. However, we would like to point out that the spatial gradients in low-flow timing in Switzerland appear stronger than in some other parts of Europe. ~~We will reformulate to clarify~~We have reformulated this in the revised manuscript.

L226: ditto

See response to previous point.

L239: Citation needed. What do you mean by erratic?

By "erratic", we mean that daily precipitation is more irregular in time (compared to PET). We now use the word "irregular" to be clearer. We are unsure where a citation is needed in line 239?

L285: No, snowpack is not the same as precipitation sum, snowmelt is precipitation redistributed over time.

We forgot to add an additional line of logic in our statement that connects winter precipitation as a (weak) proxy for snow for our study region. We will add this to avoid confusion. In addition, we ~~will be careful with stating~~rephrased the implications of the results as we earlier discussed that winter P and snow are not identical. We also supported the results of this part of the analysis with SWE data.

L300 ff: "if SWE is important, we expect to see stronger correlations between winter precipitation and summer low flows at higher elevations – see my previous comment (L222). The following analyses are wrongly motivated and results overinterpreted.

We ~~will revise~~have revised the text to reflect that winter P and snow (pack/fall) are not identical~~.~~, and we have added supporting analyses with SWE (see above and responses to previous referees)

L327: Remove sentence, as the paper does not represent a novel methodological Framework

We agree that no real framework is provided, and ~~will change~~have removed this statement accordingly.

Reply to the Student Review

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~~comments (*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~~have made 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~~have extended 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.

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. 1a&b)'. 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~~have changed 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. 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 2&3 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~~ removed the line ~~is really at 1200m. We would expect that the overall results will~~and do not ~~significantly change if the~~ use that threshold ~~is shifted from 1200m to 1350m.~~anymore in the revised version on the manuscript.

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~~have added an explanation and the corresponding reference.

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

Thank you, we ~~will correct~~have corrected this.

References:

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