

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/hess-2020-667-RC2>, 2021
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Comment on hess-2020-667

Anonymous Referee #2

Referee comment on "River runoff in Switzerland in a changing climate – changes in moderate extremes and their seasonality" by Regula Muelchi et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-667-RC2>, 2021

The authors have conducted a study over 93 Swiss catchments that cover a range of elevations. They analyzed moderate annual and seasonal low and high flows under the influence of climate change (RCP 8.5), using a modeling chain comprising of 20 RCMs and one hydrological model (PREVAH). The high and low flows were evaluated based on (1) magnitude, (2) emergence, (3) changes in seasonality, and (4) frequency.

With careful revisions, I believe the article will be ready for publication. My comments below are organized by the respective section of the paper they are relevant for.

Abstract:

- From the text, it can be gleaned that the annual and seasonal moderate high and low flows were analyzed according to: (1) magnitude, (2) emergence, (3) changes in seasonality, and (4) frequency. However, this is not explicitly stated in the abstract – it is not until lines 79-81 within the introduction that the analysis is clearly laid out. I would suggest that the authors spend some time reworking the abstract so that the reader can very quickly understand what was analyzed.
- Line 13 – suggest removing the term 'downscaled' before regional climate models
- It would be nice to highlight the changing behavior of high flows per season (see lines 16 and 28/29, where high flows are referred to), as was done for low flows (see lines 15 and 26/27). If the climate model agreement is too poor to draw conclusions on changes to high flows per season, I think this needs to be explicitly stated in the abstract.

Introduction:

- Lines 58-59 read, "high flows may also cause severe damages and significant costs. Hence, potential changes in high flows have to be integrated in water management and infrastructure planning, as well". It seems much more common that infrastructure

planning is made resilient against very extreme events. Also, severe damages from flooding are more so associated with more extreme events rather than moderate extremes as defined here. I was hoping that the authors can either support their language more clearly and state how these examples are relevant for moderate extremes, otherwise I would suggest that the language be toned down.

Data:

- On line 89, the authors mention that 22 glaciated catchments were analyzed, but the number of catchments representing the other regime types considered are not provided. Suggest adding these numbers to make the text clearer.
- At least one sentence should be dedicated to why RCP8.5 was selected as the sole scenario, as opposed to the others available, and an explanation of what that pathway represents.
- Figure 1- please indicate blue shading is for water bodies. Also, see my first comment within the Results section below.

Methods:

- The authors state that the time of emergence 'may not be stable' (line 141). Could the authors expand upon what they mean by 'stable'? Did the authors also find when the KS test is rejected repetitively? This indicator could seemingly be made more robust by requiring more than just one rejected KS test.
- The model agreement that you are highlighting for magnitude (Figure 3) and frequency (Figure 9) is 90%, whereas the model agreement highlighted for emergence (Figure 6) is 66%. Can you please explain your reasoning for highlighting different levels of model agreement?

Results:

- The results are described in terms of low and high elevation regions, however the reader is not provided a clear picture or threshold of how the authors have separated these grouped catchments nor are these regions indicated within Figure 1 (please see for instance Figure 2 from Brunner et al., 2019 – Science of the Total Environment: <https://www.sciencedirect.com/science/article/pii/S0048969719306576>).
- For Figures 3, 4, 5, 6, 7, 8, 9 – would suggest replacing the word 'YEAR' with 'ANNUAL' since this corresponds to the terminology used in the main body text.
- Figures 3 and 9 indicate where models agree 90% of the time, whereas Figure 6 indicates when 66% of the models agree. Also, Figure 6 uses grey circles to show non-agreement, which is different from Figures 3 and 9, which use muted tones. What is the reason for designing these figures so differently? Where possible, suggest making the figure design cohesive.

Discussion:

- On lines 287-290 and 315-317 you offer some discussion of the significance of your work, but this comes across as very brief. In general, the discussion section is a good opportunity to substantiate the overall implications of this research. Indeed, this work is relevant for the agricultural and hydropower industries as you mention – I encourage you to make stronger statements about the relevance of your work and look for connections in literature. As an example, I highlight the relevance of your work to Switzerland's hydropower concessions, which are strongly influenced by projected low and high flow behaviour – described here: <https://hess.copernicus.org/articles/24/3815/2020/> and especially by the following authors (just an example of suggested authors):
 - Dr. Ludovic Guadard
 - Prof. Dr. Fanco Romerio
 - Prof. Dr. Hannes Weigt

- On line 292: '...will decrease in the projections' should be changed to '...has been shown to decrease within the projections' or '...is likely to decrease in the future'.

Conclusion:

- Any mention of the greater implications of your work is generally absent from your conclusion section (please see similar comment in the discussion section above).