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Hydrology and Earth System Sciences Discussions

Interactive comment on "Future shifts in extreme flow regimes in Alpine regions" *by* M. I. Brunner et al.

Anonymous Referee #2

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Comments on: Future shifts in extreme flow regimes in Alpine regions. Brunner, M.I. et al. Hydro. Earth Syst. Sci. Discuss, https://doi.org/10.5194/hess-2019-144

Overview: This paper considers future changes in 'extreme flow regimes' in 19 hydrological regions in Switzerland as interpreted from annual flow duration curves and annual hydrographs aggregated as monthly average flows. Two novel methods are applied to generate estimates for the 100-year regimes for the current climate (using simulations driven by observed meteorological data) and a future climate (with simulations driven by bias-adjusted climate model output). The methods are illustrated using four example regions, representing both rainfall-dominated and snowmelt-dominated flow regimes, before being applied to the 19 hydrological regions. The results point towards patterns of change which are distinct for rainfall-dominated vs. snowmelt-dominated

flow regimes, and which are consistent with previous work and with changes in the hydrometeorological drivers. The authors also propose that the two approaches applied give similar and consistent results.

General comments: Overall, I find the quality of the work presented to be quite high. The topic addressed has important practical applications, and the introduction and application of alternative methods for interpreting changes in extreme conditions from data series of limited length (30-year) is much needed in climate change impacts research. The manuscript is well-written, has a structure and figures that are in most cases clear and easy to follow. I am, however, in agreement with Reviewer #1 that the major weakness of the work is the comparisons made between simulations based on observations (for the current climate) with those based on climate model data (for the future climate) in order to interpret expected changes in flow regimes. One can only justify comparisons between reference and future simulations based on climate model data, due to the inevitable discrepancies between simulations based on observed data vs. climate model data. This needs to be corrected before the manuscript can be considered for publication.

Secondly, the choice of the use of a direct stochastic simulation method rather than an 'indirect' stochastic method (which is mentioned but not discussed at all) should be presented in more detail. In particular, the use of a direct stochastic simulation of discharge from 'sampled' discharge (either simulated or observed) entails the assumption that events with long return periods come from the same population as those with shorter return periods (and can therefore be extended based on their power spectrum or using other extrapolation techniques). With 'indirect' methods, this constraint is less severe, in that driving factors producing the flows (e.g. precipitation and initial conditions) can be modelled individually and thus will potentially produce a wider range of likely flow conditions and as of yet unobserved combinations. There are nevertheless many drawbacks with the 'indirect' methods, so the authors should use the opportunity to here to highlight why they have chosen their particular stochastic approach.

My final reservation about the manuscript as it is currently presented is that the discussion of the performance of FDC and stochastic methods is limited and rather superficial. I don't see that the two methods give uniformly similar results, particularly for high flow regimes in rainfall-dominated catchments. The magnitude of the change in maximum discharge under a future climate are both higher and lower for the stochastic method than for the FDC method and the projections give different seasons for the maximum. In addition, results comparing simulations based on observed vs. climate data for the current climate are not shown for high flows. Given discrepancies between the two methods found in other figures, one would also like to be able to assess the correspondence between the simulations for high flows. A full development and discussion of the results, with reference to the different aspects considered here (i.e. snowmelt- vs. rainfall-dominated catchments and high flow vs. low flow regimes) would significantly enhance the contribution of this work to the scientific literature. Overall, I find this to be a valuable piece of work will be worthy of publication, once the issues raised above have been addressed. Otherwise, I have only a few additional minor comments, questions and proposed corrections as given below.

Specific comments:

P1 – Abstract: Well-presented, but at this point in the manuscript I also needed a clarification as to what is meant by 'flow regime'. Perhaps you should more clearly emphasis that in your end results you are interpreting/analysing 'flow regimes' using annual hydrographs comprised of monthly averaged flows. Particularly, for the case of high flow regimes, the time unit analysed is of interest.

P2 L24-25 I don't agree with the statement that the focus on an individual characteristic "neglects the pre-conditions of low- and high-flow events", at least in terms of how most climate impact studies consider changes in this variables. For example, if one is evaluating changes in maximum daily flows based on daily simulations of 30-year periods for a present and a future climate, potentially higher flows in the period preceding the largest events are also represented in those simulations, i.e. they are not

neglected. It is true that by considering an index which targets a broader time window or discharge range you are able to more directly interpret why these changes occur, e.g. because discharge is elevated for a longer time period and is thus more susceptible to extreme precipitation. In the 'standard' one characteristic approach, this would require a further analysis. A similar argument can be made for low flow indices, i.e the antecedent conditions are simulated and can be analysed if one requires additional information as to factors responsible for the estimated changes. However, to simply state that pre-conditions are neglected is misleading and incorrect.

P3 L15-20 As mentioned in the general comments, this paragraph needs to justify why direct stochastic simulation of discharge is used in this work. I would therefore suggest that the paragraph opens with a sentence describing what stochastic simulation is in general, and that this is followed by a more thorough discussion of the advantages and disadvantages of indirect vs. direct approaches, before you jump into a detailed discussion of options for direct simulation. It is important that you place the direct stochastic simulation method you have chosen within its broader context, and argue for why it is preferable and suitable for use here. In particular, there is a wide and growing literature on 'indirect' stochastic simulation which should be covered here with at least 2-3 sentences.

P6 L17 'regional downscaling approach based on quantile mapping'....more detail is needed. In particular, it is not clear in the description given in L20-L21 at what point the bias correction with quantile mapping is applied. I assume that the same data which is used for the simulations based on observed data is used for the bias correction....is this correct? In addition, what is the time period of the observed data used for bias correction?

P6 L18 ....'39 model chains (Table A1'. The choice of models used should be discussed, rather than leaving it to the reader to decipher a table in the appendix in order to determine how many different GCMs vs. RCMs vs. RCPs vs. grid resolutions are represented by the 39 simulations. The different models, etc. represented in the ensemble will indeed have an effect on both the mean values estimated and the spread about the mean, and the 39 model chains used here are only a subset of available EUROCORDEX simulations. In particular, it is unclear to me why both EUR-11 and EUR-44 grid resolutions have been used, if the climate model data are used for hydrological simulations at a 200-m grid resolution. I suspect that the EUR-44 simulations have been included to give a larger ensemble; however, this also means that the GCM-RCM combinations that are available for both EUR-11 and EUR-44 are more heavily weighted in the ensemble. A brief (2-3 lines) discussion of the composition of the ensemble is therefore needed in the manuscript.

P7 L13-14 Need to also generate stochastic series for the current 'conditions' using the hydrological model simulations based on the 39 model chains for the period 1981-2010.

P8 L2 – 'Long-term mean'. . .can be more specific, i.e. the mean regime over the period 1981-2010? I assume from your figures that this is aggregated by month?

P8 L14-15 It is this 'reference' simulation which should actually be the 'control' simulation and be further used to evaluate future changes

P8 L17 'were treated separately'. . ..not sure what is meant by this. Perhaps simply that the results were grouped by RCP?

P10 L5-7 Why is the seasonality so variable between the FDC, Stochastic and Univariate estimates in the rainfall-dominated Thur and Jura catchments? Doesn't this undermine the credibility of these methods for considering high flows in these and other rainfall-dominated catchments?

P10 L10-12 Given my comment above, I am also curious as to what Fig. A1 looks like for high-flow regime estimates? In particular, the stochastic results for Jura in Fig. 5 suggest that the method doesn't perform better than the univariate approach in that case.

P11 Sec 3 Would also like to see a comparison of the climate simulations and the control simulation for high flows, similar to that shown for Fig 6 for low flow regimes.

P12 L10-11: Changes in mean flow of up to 50%, etc. These estimates are not reliable because you are comparing the results of simulations based on climate model data (future) with those based on observations (current), and thus are also including some the error illustrated in Figure 6 in your estimates.

P17 Section 4.1: In addition to discussing the overall merits of the two methods, it would also be useful to see a discussion of their relative performance for high vs. low flow regimes and for rainfall-dominated vs. snowmelt-dominated catchments. I also find that the second paragraph in this section is too general and should focus on discussing and expanding the results you have presented in more depth. It would also be useful, for example, if you could highlight aspects of the methods you have used which are better for quantifying changes in flow regimes useful for management purposes, relative to those commonly used for climate impact studies.

P18 L15-16: 'Both were found to provide realistic, mutually-agreeing results'. I think that this is bit overstated. In particular, the results for the high flow regimes for the 19 regions (Fig. 10) show a similar direction of change (in most cases), but the magnitude is in some cases much higher with the Stochastic method, and this would have significant implications for their application in practice. Technical comments:

P1 – Keypoints: (L22) – 'are changing' should be 'will change', i.e. you have not examined patterns of change under current conditions, which is what the English used here implies

P2 L17-18: Considering rephrasing, for example to: For planning purposes and river basin management, however, estimates not only for normal conditions but also for extreme conditions are needed.

P4 L3. . .should add 'under the current climate' after 'regime.'

P12 L4 Replace ‘expressed’ with ‘pronounced’

P14 Fig 8; P16 Fig10 caption: In line 3 should be ‘The top three rows show relative changes, and the bottom two rows show changes in months’

P18 L8-9: Replace ‘We here showed’ with ‘We have shown here’

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