Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-349-AC3, 2017

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Inference of analytical flow duration curves in Swiss alpine environments" *by* Ana Clara Santos et al.

Ana Clara Santos et al.

anaclara.santos@epfl.ch

Received and published: 27 October 2017

We would like to thank the referee for her/his comments that will contribute to improve the quality of our manuscript. Below we present our responses to the remarks and issues raised by the referee.

1- The paper deals with a series of catchments in Switzerland with various geospatial and climatic characteristics. The authors compared the performance of linear against nonlinear stochastic model and found out that nonlinear one outperforms. The general idea would be interesting to the hydrology community, however, it needs to be more developed. Moreover, since the paper is going to be studied by a wide range of people, it is needed to explain some concepts and parameters with more details and better

Printer-friendly version

Discussion paper

referencing.

Thanks for the overall positive assessment. We will improve the referencing according to our detailed responses hereafter.

2- The paper is not well-organized and contains technical and language problems which decrease the scientific credibility of this study although technically it has relatively convincible results.

Thanks for pointing out that the readability of the manuscript should be improved. The language will be carefully revised. We are not entirely sure what the referee refers to in terms of "technical problems" since the detailed comments essentially mention language problems and text formatting problems. Furthermore, we believe that our original manuscript was well organized.

3- I recommend accepting it, revising it with MAJOR revision. There are some points which the authors need to address before publishing this paper:

4-More attention to results and conclusion parts is needed in the abstract.

R: We agree that the abstract does not reflect exactly our conclusions and it will be adapted in order to be more consistent with discussion and conclusions. The revised abstract will in particular include the findings about the applicability of the model to conditions of snow melt and exclude the statement about snowfall and snowmelt onsets, that were not treated in the paper. We will also mention that we adopted a forward and an inverse mode to estimate recession parameters.

5- In the introduction part, very old papers are cited which is necessary to show the history of the used method, however, the number of recently published references are very low and they repeated all over the paper. It is strongly suggested to use more updated references in the paper.

R: The key references are indeed cited throughout the paper, since they are fundamental for the topic The model of Botter et al. (2007) has been applied in more recent

studies that are not directly relevant for the work at hand, but for completeness we will include them in the literature review (Doulatyari et al., 2017; Muller and Thompson, 2016; Mejia et al., 2014; Pumo et al., 2013; Muneepeerakul et al., 2010).

Muller and Thompson (2016) nicely discusses the usefulness of statistical versus process-based flow-duration curve methods for ungauged catchments and we will update our literature review accordingly. Moreover, Doulatyari et al. (2017) have one case study in common with us and make some brief observations about snow processes that are coherent with our thougts about it, worthening to mention also in our discussion (more details on R7).

6- It is expected to mention more clearly what are the benefits of this method against others which you decided to use it for the current study.

R: This model framework has already been tested "for different climatic settings" (Botter et al., 2013; Muller et al., 2014; Pumo et al., 2013), including an application to catchments with strong urbanization (Mejia et al., 2014) and an extension to explicitly account fast flow components (Muneepeerakul et al., 2010). The benefits of a process-based approach (as the one used in our paper) as opposed to purely statistical or empirical methods can be summarized as: i) explicit link of FDC shape to rainfall characteristics and catchment recession characteristics rather than an empirical or statistical link to regional FDC shapes and parameter regionalization from extensive discharge observations; ii) the method is applicable to non-stationary climatic settings thanks to the explicit treatment of rainfall and evapo-transpiration characteristics (Muller and Thompson, 2016):

7- It is very important to compare your results with other studies to show all aspects of your findings relative to others. It can reveal the novelty of your work. In the discussion part, there is no comparison of such type. It is strongly suggested to compare the results with similar studies.

R: The novelty of our works is not in terms of proposing a better or new method to

predict FDCs but in a more systematic treatment of the model application (and related parameter estimation and performance assessment) across a range of case studies covering different streamflow regimes. This is the first assessment of this type for this modeling framework, which is very promising to assess and predict river flow regimes across climate ranges (Botter et al., 2013). This systematic analysis is a precondition for its application in ungauged catchments (Muller and Thompson, 2016). This will be made clearer in the revised version.

We agree that our discussion can be extended to compare our work with existing studies on the same catchments. Following our response to a similar question raised by another reviewer, we will include an explicit comparison of our results for the catchments Murg at Wangi and Sitter at Appenzell to the work of Basso et al. (2015) and Doulatyari et al. (2017) (Schaefli et al (2013) studies some of the same cases, but in a different season, winter, making a comparison impossible.) For the above cases, despite slightly different databases and methods, we will calculate the performance indicator that we adopted (c^{KS}) and present a comparison in our discussion. Furthermore, this comparison will also focus on the presence of snow processes and how this was dealt with in those studies. We also intend to compare the KS values with the ones obtained by Ceola et al. (2010), which adopted the same performance indicator.

8- It is needed to address data sources very clearly and describe exactly how you have used your data.

R: Data sources are addressed in section 3 (Case studies) and we believe that the manuscript was precise about how we dealt with discharge and precipitation data. Discharge data is not public and can be obtained on demand from the Swiss Federal Office for the Environment (on the weblink provided in the references). The codes that officially identify the stations are listed in Table 1, in the column ID, which will be made clearer in the text. We will provide more detail information about the selected precipitation grid cells for each catchment in an excel sheet in the supplementary material, including the exact procedure on how the area-average mean precipitation is

obtained from those grids. Furthermore, we will also upload the used basin contours as a shapefile.

9- Since not all the readers are familiar with mentioned comparative methods, explain in more details what Kolmogorov-Smirnov and Akaike methods are and try to cite to studies which used the same criteria for comparison among models.

R: We will include the equation for the Kolmogorov-Smirnov distance, which, besides, was well explained on p. 7, lines 5 following. The Akaike information criterion is a very standard metric to choose between models of different complexity. We will add a comment on this and an additional hydrological references (Laio et al., 2009, Ceola et al.,2010).

10- Although the results are interesting, but they need more discussion to appropriately describe the new findings.

R: We will improve the discussion as outlined above (R7). Additionally, we will extend the discussion of the case studies influenced by snow processes (see also R11), in agreement also with a statement of reviewer #1.

11- How do you justify if for a specific year, a part of discharge was related to the melt of the ice from the other year precipitation. How is distinguished? Does it have any effect on your results?

R: We do not explicitly address the source additional water (in addition to this summer's rainfall), and accordingly, we do not distinguish between snow melt and icemelt (accumulated during previous years). We answered a similar question in our response to reviewer $\#1$ and transcribe a part of that answer here:

"A priori, the original model framework is not yet supposed to be suitable for snow-dominated catchments during periods of snowmelt (for winter flow, see Schaefli et al., 2013). Originally, the stochastic inputs to discharge production were modeled based on the mean precipitation depth (α) and the frequency of precipitation (λ_p), which are

estimated from observed precipitation and corrected according to losses (i.e. evapotranspiration) to obtain the frequency of discharge producing events (lambda), which is expected to be smaller than λ_p. In many snow-dominated catchments, snowmelt happens mostly during spring and summer discharges are essentially rainfall-driven. Nevertheless, we included some catchments with presence of glaciers (where snow and ice melt definitively continue throughout the summer) to test if the framework could work without adaptation also for those cases. In fact, it is a common assumption in catchment-scale hydrologic modeling (e.g. Schaefli et al., 2005, HESS) that catchment runoff during snowmelt can be modeled with exactly the same functional relationships as during rainfall by simply feeding so-called "equivalent precipitation" into the runoff-generation module, which is composed of rainfall and simulated snow melt. Building on this, it is tempting to think that the analytical framework used here also works for seasons where there is some snowmelt present. However, we did not want to model snowmelt explicitly at this stage since this would add additional parameters to the model. In exchange, we applied the existing framework directly. The presence of snowmelt is not neglected since the discharge producing frequency is estimated from observed discharge. The results are surprisingly good, even for catchments with glaciers. What we noticed that happened for those cases was an increasing of λ_p, which is inline with the idea that discharge results from "equivalent precipitation" (rainfall and melt). This was not discussed in detail in the submitted manuscript but we will include a discussion on this in the revised version. What is important to point out here, is that the additional source of water (i.e. snowmelt) is accommodated in the model as an increase in the frequency of inputs and not as an increase of the amount. We will discuss this in detail in the revised version. But the full extension of the model to account for snowmelt inputs explicitly is left for future research."

12-There are many points which highly need rephrasing and corrections, mainly grammatical and language issues. Moreover, it can be seen that the used language in some parts is very similar to conversation rather than a scientific text which causes ambiguity in the text. It is strongly recommended to highly take care of this issue.

We will carefully revise the language throughout the paper.

13- The discussion and conclusion parts need a complete rephrasing. Therefore I am not going to mention them in Line-by-Line comments part (..)

R: Thanks for your detailed suggestions. We will address each point in the formal rebuttal to be submitted with the revised version.

Comments about tables and figures:

Table 1.

1- There are two columns with the names "Regimes" it is not acceptable.

R: The columns are named "Regimes 16 classes" and "Regimes 3 classes", we will fit the number of classes in the first line of to make the distinction more evident.

2- It needs rows number in the first column to quickly and easily find out how many catchments are in the table

R: We will add a first column, numbering the rows.

3- What are the coordinates? It is not possible to extract any information from this column.

R: The coordinates correspond to the Swiss coordinate system (CH1903), that information will be added to the table.

Table 2.

1-The caption is not appropriate. Try to mention exactly what this table is supposed to describe in an organized pattern.

R: We are going to reorganize the caption in a more objective way.

FIGURES:

1- Figure 2 does not have vertical axis title

R: We will add both scales (representing temperature and mean monthly discharge) to the graph axes.

2. Figure 4 caption needs more detail. Precipitation frequency is mentioned in the caption but it is not in the figures. Pay attention that a figure, without the text, should be understandable.

R: We will organize the figure legends, axes and captions and make everything coherent. We also identified that the legends of the second row (Precip frequency and Discharge producing frequency) are inverted, a mistake that will be corrected.

3. Figure 6 caption needs rephrasing

R: We will rephrase the caption of figures 6 and 7.

4. Figure 9. It is better to show some trends inside the graph.

R: We will add trends for each graph (linear and nonlinear) and each type of estimation (forward and inverse).

References

- Basso, S., Schirmer, M., and Botter, G.: On the emergence of heavy-tailed streamflow distributions, Advances in Water Resources, 82,98–105, doi:10.1016/j.advwatres.2015.04.013, 2015.
- Botter, G., Basso, S., Rodriguez-Iturbe, I., and Rinaldo, A.: Resilience of river flow regimes, Proc. Natl. Acad. Sci., 110, 12925–12930, 2013.
- Ceola, S., Botter, G., Bertuzzo, E., Porporato, A., Rodriguez-Iturbe, I., and Rinaldo, A.: Comparative study of ecohydrological streamflow probability distributions, Water Resources Research, 46, doi:10.1029/2010wr009102, 2010.
- Doulatyari, B., Betterle, A., Radny, D., Celegon, E. A., Fanton, P., Schirmer, M.,

and Botter, G.: Patterns of streamflow regimes along the river network: The case of the thur river. Environmental Modelling & Software, 93:42–58, jul 2017. doi: 10.1016/j.envsoft.2017.03.002.

- Laio, F., Di Baldassarre, G., and Montanari, A.: Model selection techniques for the frequency analysis of hydrological extremes, Water Resources Research, 45, W07416 10.1029/2007wr006666, 2009.
- Mejia, A., Daly, E., Rossel, F., Jovanovic, T., and Gironas, J.: A stochastic model of streamflow for urbanized basins, Water Resources Research, 50, 1984-2001, 10.1002/2013wr014834, 2014.
- Muller, M. F., Dralle, D. N., and Thompson, S. E.: Analytical model for flow duration curves in seasonally dry climates, Water Resources Research, 50, 5510-5531, 10.1002/2014wr015301, 2014.
- Muller, M. F. and Thompson, S. E.: Comparing statistical and process-based flow duration curve models in ungauged basins and changing rain regimes, Hydrol. Earth Syst. Sci., 20, 669-683, https://doi.org/10.5194/hess-20-669-2016, 2016.
- Muneepeerakul, R., Azaele, S., Botter, G., Rinaldo, A., and Rodriguez-Iturbe, I.: Daily streamflow analysis based on a two-scaled gamma pulse model, Water Resources Research, 46, n/a-n/a, 10.1029/2010wr009286, 2010.
- Pumo, D., Noto, L. V., and Viola, F.: Ecohydrological modelling of flow duration curve in Mediterranean river basins, Advances in Water Resources, 52, 314-327, doi:10.1016/j.advwatres.2012.05.010, 2013
- Schaefli, B., Rinaldo, A., and Botter, G.: Analytic probability distributions for snow-dominated streamflow, Water Resources Research, 49, 2701–2713, doi:10.1002/wrcr.20234, 2013.

• Schaefli, B., Hingray, B., Niggli, M., and Musy, A.: A conceptual glacio-hydrological model for high mountainous catchments, Hydrol. Earth Syst. Sci., 9, 95-109, 2005.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-349, 2017.

Printer-friendly version

Discussion paper