

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

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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. General comments*

*In this manuscript, the authors apply a well-known stochastic framework in its linear and nonlinear form to 26 catchments in Switzerland. The authors explicitly consider a forward and inverse parameter estimation technique and present the different results between them in detail. Additionally, the performance is assessed with respect to observed discharge. A strong link between catchment elevation and model performance*

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*is found for both the linear and nonlinear model version. Overall, the nonlinear version is yielding higher performance*

*The manuscript is easy to understand. The manuscript, however, lacks a proper discussion of the results. This can be seen by the fact that the discussion does not contain any reference to previous work, of which exists plenty (these are also mentioned in the introduction).*

R: We agree with the referee that there are previous works using the same framework (they are cited in the introduction of the submitted manuscript) and that we can improve our discussion. Nevertheless, the case studies and databases are different, as well as the used metrics, which limits the options for direct quantitative comparisons. We give hereafter some more details on the planned comparisons (in response to a similar detail comment).

*My biggest point of criticism is that the model does not seem to be applicable to snow-dominated catchments. It lacks the process of snow melt and thus model parameter compensate and behave contradictorily to theory, which is mentioned throughout the manuscript. The manuscript can thus not be published as such. The authors either have to remove these catchments or more interestingly, they have to show how snow melt can be considered in this stochastic framework. The latter avenue would provide a real advance in the research. At the moment, the novelty of the presented work is the application of the inverse parameter estimation to both a linear and nonlinear stochastic framework to estimate streamflow cdfs. The manuscript has to be substantially improved with respect to motivating this point and the discussion has to at least discuss the obtained model performance with respect to previous work.*

R: 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 Schaeffli et al., 2013). Originally, the stochastic inputs to discharge production were modeled based on the mean precipitation depth ( $\alpha$ ) and the frequency of precipitation ( $\lambda_p$ ), 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  $\lambda_p$ . 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. Schaeffli 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  $\lambda_p$ , 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.

## *2. Specific comments*

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## 2.1. Major comments

### Abstract:

*p1, l. 9: The conclusions here are not the same as in the conclusions section regarding snowmelt and snowfall onset. As a matter of fact snowfall onset is not mentioned anywhere else in the manuscript. The conclusions are also not deducible from the abstract.*

R: The conclusions are going to be extended mentioning our concerns about snowmelt and snowfall, that will be the subject of future studies, as written in the abstract. Also, the abstract will include a sentence about the good results for catchments that present some snow/glacier-melting processes without requiring adaptation of the model.

### Introduction:

*p. 1., l. 21f: This statement suggests that this paper will cover to some extent prediction at ungauged basins (PUB), but this is not the case. It is thus misleading. Also the following paragraphs are (p. 2, l. 1ff and p. 2, l. 6 ff) introducing papers for regionalizing fdc parameters for PUB, which deviates from the topic of this paper - the suitability of a linear / nonlinear stochastic framework at locations where streamflow observations are available*

R: We thank the reviewer for this observation and we agree that we should have mentioned ungauged basins only as an outlook. The introduction will be modified to give less weight to ungauged catchments.

*p. 2., l. 31ff.: As pointed out correctly by the included references, this model framework has been applied in a wide range of hydro-climatic regimes. Specifically, the references to Schaefli et al. (2013) is investigating a very similar set of catchments. The difference to the presented study is that Schaefli et al. (2013) only investigated the linear model and not the nonlinear one. This is just briefly mentioned in the introduction (p. 3, l. 11).*

*The value of this study is the comparison to the nonlinear model and the parameter*

*estimation. The introduction should investigate the difference between these two in depth to motivate the topic.*

R: The work of Schaefli et al. (2013) suggests an adaptation of the linear model to winter discharges in snow-dominated catchments, while we focused our work on summer discharges, which makes impossible to compare results even for the linear model. We will make this clearer in the revised introduction.

Nevertheless, this previous work motivated us to keep studying the linear model to verify if it could be suitable also for other seasons. Interestingly the results from the linear model are better for higher elevation catchments, where there is more influence of snow in the hydrological process in general.

We will stress the comparison between the linear and the non linear model in the introduction.

*Methods:*

*p. 6, l. 14f: If recession constants are calculated from daily discharge, how does this method help for prediction at ungauged locations?*

R: As correctly noted before, this work does not concern ungauged catchments, so the methods used to obtain recession constants cannot be applied to them. We will adapt the introduction to make it clear that ungauged catchments are not the subject of this work, they are a future concern.

*p. 6, l. 21ff: I do not understand how  $\lambda_p$  is estimated from equation 7. There is also a contradiction in the description of this equation in p. 6, l. 23.*

R: Thank you for this observation,  $\lambda_p$  is not obtained from Equation 7, but from the precipitation data exclusively, being just the frequency of precipitation events. The method to obtain this parameter will be clarified and the contradiction will be corrected, the text should refer to Equation 3, not 7.

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## Case studies:

*p. 7, l. 27ff: It is not clear to me why snow-dominated catchments are considered in this study. It is clear from equation 2 and 5, that the model is not representing snow melt by temperatures above 0 degree Celsius. These basins should be removed or the model adapted to represent snow melting processes*

R: Please refer to our earlier response to the general comments.

## Results

*p. 9, l. 19ff: The fact that lambda, the frequency of discharge-producing precipitation events, is related to snow melt indicates that the model is not suitable for some catchments, which limits model applicability. It might get the right answer, but for the wrong reason. This is also emphasized by the statement on p. 9, l. 28ff*

R: The increase of  $\lambda$  in relation to  $\lambda_p$  can be explained by snow melt. The good results show empirically that the model is able to incorporate snow melt as an increase of the discharge producing frequency (as it incorporates evapotranspiration as a decrease of this frequency, as it can be seen in the lower catchments). We will make this clearer in the revised version.

## Discussion

*p. 11, l. 13f: The model has already been applied in Swiss catchments in previous work. This should be discussed here.*

R: To our knowledge, the model framework has been applied by Basso et al. (2015) to two of our case studies and by Doulatyari et al. (2017) to one of our cases, in both works, the nonlinear model was adopted. It has also been applied by Schaepli et al (2013), but to winter, making a comparison impossible. For the cases with possible comparison, despite slightly different databases and methods, we will calculate the indicator that we adopted (KS) and present a comparison in our discussion.

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*p. 11, l. 14ff: Has an increase of the discharge-producing frequency over the precipitation frequency been observed in previous work that considered snow-dominated catchments?*

R: Most of the previous studies explicitly exclude cases and/or seasons where snow processes could influence discharge production. One study that mentions an overestimation of  $\lambda$  during spring and underestimation during winter is the very recent work of Doulatyari et al. (2017), that observes that pattern for a single case, the Sitter at Appenzell (also one of our case studies), and proposes the same explanation as we did. But this type of pattern can be seen in other studies. Basso et al. (2015), for example, despite stating that catchments with snow processes were excluded, presents the same case as Doulatyari et al. (2017) where the overestimation of  $\lambda$  can be seen and an additional case where for spring,  $\lambda$  is equal to  $\lambda_p$ , the Thur at Jonschwil. That equality, during spring, in a catchment with mean elevation 1030m amsl hints towards an additional source of water, probably snowmelt. This type of comparison is not always possible because many papers report only  $\lambda$  and not  $\lambda_p$ . We will discuss this in more detail in the revised version.

*p. 11, l. 21ff: The discussion of the performance has to incorporate the results of previous studies. KS distance have also been used previously.*

R: Comparison with some of the results of KS obtained by Ceola et al (2010) will also be incorporated in the discussion. Despite having different cases, the magnitudes of the values are similar.

*p. 11, l. 24ff: The authors have to present a discussion here why the recession parameter are underestimated, not only stating that they are.*

R: We are still developing additional procedures to address this issue. We will include an appropriate discussion of our present understanding of this underestimation based on previous works from other authors.

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## 2.2. Minor comments

*p. 1., l. 3f: “The model paramters are...” This sentence is misleading because the gridded precipitation product is lumped as the input for the model.*

R: We will be clearer about adopting a spatial average of gridded precipitation and we will correct that.

*p. 4, l. 2: Figure 1 is not presented in detail in the text. It should help the reader to understand the methods better, but is only referenced here.*

R: We will add some additional explanations about Figure 1 to the text.

*p. 4, l. 25: it should read “i.e. of”.*

R: We will correct this mistake

*p. 5, l. 4: “. . . start to move in the soil. . . ” is ambiguous. It is not clear what the authors mean by this.*

R: We will clarify the discharge producing process in the text.

*p. 5, l. 27ff: I do not understand this sentence.*

R: We will improve the explanation about the forward method.

*p. 8, l. 20ff: The paragraph on the description of the biogeographical regions is not much related to the work and should be removed.*

R: The paragraph aimed to show the variety of hydrologic conditions in Switzerland and to introduce the classification of regimes proposed the Federal Office for the Environment presented in Table 1, but we will shorten this description.

*p. 10, l. 10ff: Mention here that KS values are shown in Table 2.*

R: We will mention that the values of the indicator are presented In Table 2.

*p. 11, l. 6ff: The plot for the relative performance increase does not add important*



information as the improvement for low elevation catchment can already be seen in Figure 9. It should be removed.

R: We agree that both plots present similar results, but we believe that Figure 9 is clearer. Nevertheless it can be omitted.

#### References:

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