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Hydrology and Earth System Sciences
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Discussions

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

Anonymous Referee #1

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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 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). 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 The manuscript can thus not be published as such.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.

Please find my major and minor comments below.

0.1 Major comments

Abstract

p. 1, 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.

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 regionalising 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.

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.

Methods

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

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.

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.

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 emphasised by the statement on p. 9, l. 28ff.

Discussion

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

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?

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

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

0.2 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.

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.

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

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

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

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

p. 10, l. 10ff: Mention here that KS values are shown 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.

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