

## ***Interactive comment on “Bayesian discharge rating curves based on B-spline smoothing functions” by K. M. Ingimarsson et al.***

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The opinion of the referees about this paper is critical. Basically the referees question the structure and clarity of presentation and the lack of a physical interpretation of the results.

I regret to report that my opinion about this paper is not positive as well. The original contribution of this study relies in the use of the B-spline smoothing function for giving much flexibility to the shape of the rating curve. I recognise that the topic is extremely important and I also recognise the potential value of the proposed approach. However, I think it is not very helpful to compare the newly proposed model with an approach which is less flexible. Of course flexibility leads to a better fit, but it has to be better

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proved that it leads to a practical improvement of the results. I think the newly proposed model should at least be compared with additional approaches that are characterised by more flexibility than Model 1. I fully agree with the final sentence of the authors:

"Finally, it is noted that segmentation has been commonly used in estimating discharge rating curves and it could be argued that maybe it is more appropriate than Model 2 for data sets where there is visually an apparent shift. A direct comparison between segmentation models and Model 2 is needed to compare their performance. A joint use of multi-segment discharge rating curves and B-splines could potentially be beneficial for such cases".

Problem is that such comparison should be presented in the paper and not left for future work, in order to effectively and comprehensively prove that the new approach is indeed an improvement with respect to existing ones.

Moreover, I agree with the referees that the presentation is not well structured. Sections 4 and 5 should be completely rewritten in my view, by clearly distinguishing theoretical issues from application. Moreover, theory should be better explained. What are the underlying assumptions? Are they satisfied in the application? For instance, it seems from equation 3 and equation placed a few lines above (numbers of some equations are missing) that the residuals are assumed to be Gaussian. Was Gaussianity of residuals checked? Also, visual inspection of homoscedasticity of the residuals is not clear. Figure 2 is very difficult to read, with non appropriate fonts, and it is not clear to me that Model 2 improves homoscedasticity of the residuals (which, by the way, would be a trivial results given that Model 2 is more flexible than Model 1).

In my opinion the paper is far from ready to be published in HESS in its present form. However, I think the study is valuable and the authors may want to commit themselves with a major revision of the study and the presentation. Additional comparisons are needed, together with a more structured presentation.