Interactive comment on "Estimating high quantiles of extreme flood heights in the lower Limpopo River basin of Mozambique using model based Bayesian approach" *by* D. Maposa et al.

Anonymous Referee #3

Received and published: 30 September 2014

The review of the article Estimating high quantiles of extreme flood heights in the lower Limpopo River basin of Mozambique using model based Bayesian approach

by

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submitted to Natural Hazards Earth System Sciences nhess-2014-197

In their paper Authors carry out a comparative analysis of the maximum likelihood (ML) and Bayesian parameter estimates of the generalised extreme value (GEV) distribution. They use these estimation methods to estimate the parameters of the GEV

distribution in order to calculate extreme flood maximas and their return periods in the lower Limpopo River basin of Mozambique. According to Authors the return periods of extreme flood heights based on the Bayesian approach show an improvement over the maximum likelihood estimation (MLE) method. However, both approaches indicate that the 13 m extreme flood height that occurred at Chokwe in the year 2000 due to cyclone Eline and Gloria had a return period in excess of 200 years, which implies that this event has a very small likelihood of being equalled or exceeded at least once in 200 years.

Overall remarks I deeply appreciate and admire the enormous amount of work and the effort Authors made while preparing their paper. However, I do have some doubts which, I believe, can be useful when preparing an improved version of the article. Firstly, I must admit, that I have dilemma how to assess the paper. On one hand the paper is pretty well written and important for the hydrology of South Africa region, but on the other hand, I have not spotted a single new idea in the manuscript! Both methods Authors present are very well known and fossilised in hydrological sciences (compare e.g. Dalrymple, 1960, Kaczmarek, 1977, Kuczera, 1999, Martins and Stedinger, 2000, Renard, et al., 2013). Even though the Authors present the 'old-school' methodology in new circumstances they do not put it in a way, so the readers could benefit out of it. I would suggest to rephrase the article to underline and stress the novelty of the research. In the present form the paper looks like an engineering report rather than a scientific paper published in a seasoned scientific journal. In the Introduction the Authors describe inter alia the catchment of the Limpopo River and floods that occurred in this region over few last decades. In my opinion the Introduction is a bit too wordy – the whole paper would benefit, if Authors considered to cut the long story short and limit this chapter only to the most relevant issues. The important issues, such as the goal of the paper and motivation of the Authors drown in myriads of unimportant or easy-to-check facts. I would even suggest to divide the Introduction into several shorter chapters, e.g. the real Introduction, Literature research, the Description of methods (ML and MCMC) and the Description of the catchment. In fact, the recommendation

to shorten a paper concerns all the manuscript. As far as the description of methods is concerned, I would expect a description: two or three short paragraphs per each method, concerning the methods' theoretical basis, pros and cons, their limitations and recommendations which problems can be solved by either of these methods. I also wonder what distribution functions and what parameter the Authors used as the priors? I think that this choice significantly affects the posterior results. Besides it is worth mentioning that the ML method concentrates on main mass probability and therefore it is not quite recommended for estimation of upper quantiles. The second chapter, Materials and methods, is also a concoction of different issues not quite related to each other. I would recommend to shorten it and distribute the information of Chapter 2 to the four chapters proposed earlier. In the chapter 2 the Authors also present well known facts that could be easily replaced by just references to other authors. The chapter 3 presents the results of the research. The Authors select the heavy-tailed GEV distribution function as the underlying model, but I am not convinced by the arguments supporting this choice. What could be expected, both methods (ML and MCMC) produce slightly different results of parameters and errors. The Authors claim that: 'The Bayesian estimates of annual daily maximum flood heights and their associated return periods (see Table 3) in this paper seem to be closer to reality as compared to the MLE approach.' but they do not explain why. The chapter 4 seems to be a bit of surprise. It was not announced earlier and I am not sure if it brings any extra information to the manuscript. Again, I would 'dissolve' the information of this chapter in other ones. The last chapter, Conclusions, repeats what was written earlier but in a concise way. In my opinion it is not what Conclusions should look like. There is not a single sentence of recommendation to the readers, summing up of the research or prospects for the future. This chapter should be completely re-written.

Specific comments âĂć Tables 1 and 2 could be merged – it would be easier to compare the results âĂć I lack of the figure that shows diagrams of goodness of fit of the two methods to measurements data. Maybe a common diagram made of graphs of lower left corner from Fig. 3 and Fig 5 would do?

Summary and recommendations In my opinion the reviewed paper needs substantial work to meet the standards indispensable to be published in a seasoned scientific journal such as Natural Hazards Earth System Sciences. First of all, I would suggest rephrasing all the article in a way to bold the relevant issues which now drown in the myriads of irrelevant (or obvious) facts. As far as I am concerned, Authors should put the stress on the new ideas, make them occupy the foreground of the presented issues and concentrate on more universal applications of the proposed framework which they applied only for the Chokwe gauging station on the Limpopo River.

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