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Discussions

Interactive comment on "Methodology for flood frequency estimations in small catchments" *by* V. David and T. Davidova

Anonymous Referee #1

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The article "Methodology for flood frequency estimation in small catchmentsÓ by David and Davidova, submitted to Natural Hazards and Earth System Sciences, illustrates a regional procedure to estimate flood quantiles corresponding to return periods of 10 and 100 years in small ungauged basins in the Czech Republic. The procedure is statistically-based and follows the traditional approach in which the flood quantile is related to some basin characteristics through statistical relationships.

As noted by the authors, the topic is of practical relevance for that area, where reliable flood estimates are hardly available. Unfortunately, in my opinion, the paper cannot be published without a complete review of the procedure and a more consistent presentation of the results. My concerns are listed below.

General comments

My first issue is about the number of parameters involved in the descriptor transformation. As one can see from figures 2-9, most of the relationship between the quantile and the single descriptor are nearly linear; I do not believe that 4 parameters can lead to any significant improvement of the fitting with respect to a 2 parameters model. Moreover, in figure 5, the fitted curve is constant for almost all the range of variation of shape factor, while rapidly changes for very small shape factors, denoting a very unstable fitting. These visual considerations rise a question about the reliability of the estimated parameters. It is evident that, in most of the cases, 4 parameters for each descriptors are not appropriate, and just make the procedure less robust (i.e. more sensitive to small changes in the calibration data). The authors should then provide a method to evaluate whether a parameter is significant, in order to keep it only when it really improves the estimation. I suggest to start the analysis with a more robust procedure (e.g. the GLS regression of Stedinger and Tasker, 1985) and then try to improve the results by non-linear manipulation of variables.

Another issue, strictly related to the previous one, is the "model selection" procedure. It is not clear throughout the paper which was the sequence of steps considered by the authors. The procedure is applied to each descriptor alone as "...relationship .... were performed individually for each of the considered catchment descriptors" (page 6333 line 3-4). Figures 2-9 seem to be the results of such first step. However, in section 4.3, one reads "The methodology was first parameterized for all tested catchments descriptors...without considering the least important descriptor..." (page 6338 line 18-21). This procedure seems to be equivalent to first estimate the parameters of the complete model (all the descriptors together) and then to remove step-by-step the least significant variable. My questions about this issue are:

- Are the parameters re-calibrated when a multi-descriptors model is used? Or, is it simply a combination of the parameters estimated individually?

- Which is the criterion used to identify the least significant descriptor?

Specific comments

Here some more specific comments:

- line 18-21 page 6329: Does the method really avoid linearization? Probably linearization comes directly from nonlinear transformation (depends on the parameters estimation method).
- On page 6331, the non-linear transformation of the variables is described in equation (1) and (2). Reading the text, it seems that such procedure is derived from the works of Asquith and Slade (1996) and Olson (2009), but actually both papers apply the generalized least squares (GLS) method as proposed by Stedinger and Tasker (1985) and in a number of subsequent articles. Proper references, if available, should be reported. The section must be reviewed to avoid misleading references to paper which have different contents (the cited paper deal with regionalization, but focus on GLS approach rather that on the non-linear one).
- The parameter estimation is performed using the “GRG non-linear method available in Excel” (lines 7-8, page 6333), but there is no clear reference to the method. A concise description of the algorithm should be provided, even if it is a standard Excel procedure, in order to evaluate whether the algorithm is adequate for the problem. As one can see, there is a quite large number of parameters (4 for each included descriptor plus a_0 and d_0), so non-linear optimization procedure should be used carefully to be sure that results are reliable and the procedure converges to a proper minimum. More tests on convergence and stability of the procedure should be presented.

- On page 6331, at the end of section 2, the authors say that their method follows the “black-box approach which means that the internal parameters *can* have conceptual interpretation and *thus* also be physically meaningful”. Unfortunately, I have no access to the book cited in this paragraph (Dooge and O’Kane, 2003) so I cannot verify the definitions reported in it; however, the black-box approaches usually refers to methods in which the variables are not (directly) related to a physical process (“black” stands for a relationship which is not visible).
- A scatterplot of observed versus predicted quantiles, which I consider a very important (although very simple) outcome, is missing. Such kind of graph is essential to visually check the results and cannot be replaced by summary statistics on residuals as table 1 or figure 11.
- Please review the grammar of the phrase "This option ... solution techniques" (line 9, page 6333).
- Please clarify how the “accuracy” cited in line 22, page 6338 is quantified.
- The author should give more support to their results in the conclusions section to guide the user in apply the model.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 6327, 2013.