Hydrol. Earth Syst. Sci. Discuss., 9, C5777–C5780, 2012
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Hydrology and Earth System Sciences Discussions

Interactive comment on "A method for low flow estimation at ungauged sites, case study in Wallonia (Belgium)" *by* M. Grandry et al.

Anonymous Referee #2

Received and published: 11 December 2012

General comments: This paper presents a case study which combines frequency analysis and the (regional) regression approach to estimate MAM7 low flows for different return periods T. The goal is to fit a model which can be used to predict MAM7,T at gauged and ungauged sites in Wallonia. While the study presents some interesting material and has potential for a nice study, the paper has several weaknesses as detailed below. This would require carefully re-editing of the paper in combination with revised and additional analyses:

1) The innovation of this paper is unclear. The literature review does not explicitly tell us what prior studies achieved and what is missing (I suggest that you rephrase it accordingly). It remains unclear what is actually the research gap that this paper wants

to fill. Is it only “completeness” in the sense that all the stated methods are tested on one region – this would be not sufficient.

2) The presentation needs to be improved. The paper reads like a report, it should be more concise, while giving all necessary information. Some parts are really narrative, carry little information, and are a bit sloopy (e.g. p 11585, line1: The knowledge of river behaviour during low flows is really important nowadays. . . - and many other sentences). The literature review in the introduction is written in an unusual way, studies are cited by telling what was missing there (e.g., Chen (2006) did not, Hayes (1991) did not, . . .). I suggest that you first say who, where, what was achieved, and than what you think is missing.

3) Soundness / state-of-the-art of methods

a) The approach of frequency analyses is a very classical one (and a bit dated): Try any distribution and take the best fit. I strongly argue that this is not sate-of-the-art, e.g. Tallaksen and van Lanen (2004), Pacheco, Gottschalk & Krasovskaia (2006) and many others recommend letting the choice of the distribution be guided by statistical judgement in combination with knowledge of the phenomenon studied. The danger of goodness-of-fit selection is the overfitting to the short samples.

b) I am not sure about the fit and evaluation of the models (p. 11596 and Fig. 2) because of several strange performances indicators: - The R2 are large for the validation set (short records which where deemed inappropriate for the model) than for the “calibration” data set, what is unusual. Moreover as the validation set are short records which where deemed inappropriate for fitting. (You could use cross-validation instead). - There is a huge bias for large values (overestimation) and also for small values (underestimation). There is a lack of fit which can be due to several reasons, one is the leverage points visible in Fig. 2 (and also in the regional models shown in Fig.4). There are several ways reported in the literature to work with messy data as it is commonly the case for extreme flows. I suggest that you try to refit the models. And I

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would like to see the scatterplot of predicted vs. observed values. - The relatively large R^2 values are related to the leverage points... I understand that the modelling exercise is not easy because of the rather limited data set but this is a necessary step for sound conclusions and predictions.

c) Before starting a regional regression approach, it should be tested if the region is heterogeneous. If the regional regressions do not perform better than a global regression, is it worth to do the more complicated regional regression approach?

Specific comments:

p 11588, line 26: you describe the estimation of posterior probabilities – did you apply a Bayesian approach of the maximum likelihood method?

p. 11590 percolation – how defined, how derived? (same for recession coefficient – define exactly as it is important for the study)

p. 11592, line -2 before results: Relationship b/w regression coefficients and return period – is the approach new or taken from the literature? (If this is new, it would be the innovation of the paper and should be highlighted as such).

Section 3.2 it would be useful to interpret the first 2 principal components – what are the latent variables?

Section 4 – Discussion – should be rewritten in a more concise style

Section 5 - Conclusion - should be rewritten in a more concise style, should transport the main findings of the paper.

References: Pacheco, A., Gottschalk, L. & Krasovskaia, I. (2006) Regionalization of low flow in Costa Rica. In: Climate Variability and ChangeâĂŤHydrological Impacts (Proceedings of the Fifth FRIEND World Conference Havana, Cuba, November 2006. IAHS Publ. 308, 111-116. IAHS Press, Wallingford, UK

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