

Interactive comment on “Discharge hydrograph estimation at upstream-ungauged sections by coupling a Bayesian methodology and a 2D GPU Shallow Water model” by A. Ferrari et al.

Anonymous Referee #2

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General comments

The manuscript applies a Bayesian geostatistical methodology to the solution of the inverse problem aiming to estimate the upstream flood hydrograph at an un-gauged river section. The downstream routing of the hydrograph is pursued by means of a 2D shallow water model. This leads to a computationally intensive problem, for which a parallel implementation is designed. The most computationally intensive operation (i.e.: the evaluation of the Jacobian matrix) is demanded to a multi-GPU HPC, and also the forward model exploits the opportunities of GPU-parallelization.

The adoption of two-dimensional hydraulic model represents a step forward compared

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with both the previous research developed by the Authors and with the state-of-the-art. The resulting complication arising from the increased computational effort is handled properly. Therefore, the research described in the paper appears to be sufficiently innovative, well-designed and of interest to the readers of HESS.

I am rather supportive of the publication of the manuscript, provided that the Authors put some additional effort in improving the quality of the presentation (especially of the English) and in addressing some issues in order to make their outcomes more conclusive. I provide in the following few specific comments to be considered in the revision, as well as some minor issues that could contribute to improve the quality of the manuscript.

Specific comments

- I appreciate that the presentation of the Bayesian Geostatistical Approach (BGA) is concise but complete of every detail: however I found it not very clear at some points, detailed below:
 1. The “prior mean” defined in eq. (9) should be better commented, explaining why the β vector reduces to “a single value” (do the Authors mean the same value for each parameter?), and why the matrix X reduces to “a single vector of ones”.
 2. The separation distance d should be defined explicitly.
 3. I wonder about the opportunity of defining Q_{ss} as $Q_{ss}(\theta)$ since the r.h.s. of eq. (6) does not contain θ .
 4. I could not find the definition of ξ appearing in eq. (9) and eq. (13).
 5. The Authors should better explain what they mean with “a flat solution”.
- In the scheme depicting the BGA in figure 3, I could not find the condition corresponding to the parameters convergence, which is claimed in the text. According

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to the scheme, the inner cycle terminates only when the maximum number of iterations N_i is reached. The Authors should clarify this point and modify accordingly the manuscript and/or the figure. Assuming that also convergence causes termination, the Authors should explain how did they check the convergence.

- The Authors should explain how the credibility intervals may be evaluated based on the results of the BGA algorithm, or at least provide a reference to previous literature.
- About the core of the research described in the manuscript, I am mostly concerned about three issues. They should hopefully be addressed in the revised version of the manuscript.
 1. Since the principal innovation comes from the adoption of a 2D forward hydraulic model, the improvement in terms of the quality of the estimated hydrograph deriving from the use of a more detailed (but also demanding) schematization of the hydraulic process should be explicitly assessed. For instance, how wrong is the estimated hydrograph if one uses a 1D model as the forward routing model in one of the presented examples?
 2. Could the Authors discuss (hopefully with the aid of some additional results) the effects of the resolution of the DEM and/or of the values of the roughness parameters on the estimated hydrograph?
 3. I understand the role of the simulations based on synthetic data-sets, with or without accounting for measure corruption in the validation of the procedure. On the other hand, as far as the “real field application” is concerned, I think that a different test case should have been considered, namely one for which the measured hydrograph was available, in order to compare the estimated with the actual one. This not being the case, the evaluation of the procedure performance cannot go further than the “credibility” (in a statistical sense), and the claims by the Authors in the comments (“This real field

application further confirms the capability of the proposed inverse procedure of estimating irregular inflow hydrographs in real rivers”) may sound excessive and not fully supported. Could the Authors take into consideration the addition of such an example?

- English should be carefully revised throughout the entire manuscript to match the standards of scientific communication.

technical corrections

- Please refer to eq. (5) and (6) as to linear or Gaussian variogram, just the way you did in section 4.2
- Probably in r.h.s. of eq. (14) a “+” sign is missing. Please check.
- Throughout the manuscript, “non linear” should better read “non-linear”
- Please note that actually the r.h.s. of eq. (12) is not a fraction, therefore referring to “denominator of Eq. (12)” makes sense if you are considering the discrete approximation of the Jacobian.
- The description of fig. 6 and the figure itself refer to four cross-sections along the river: an upstream un-gauged one (A), two intermediate (B and C) where water levels are measured, and a fourth one (D) for downstream boundary condition assignment. However, in the presented examples, only a single intermediate measuring cross section is used, so maybe the description and the figure should be consistently simplified.

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