

Interactive comment on “Data-based discharge extrapolation: estimating annual discharge for a partially gauged large river basin from its small sub-basins” by L. Gong

Anonymous Referee #3

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This paper describes an approach to upscale streamflow from a group of smaller basins to a larger basin, by up to 2 orders of magnitude, where smaller subbasins are a subset of the larger basin having similar hydrological and meteorological characteristics. Two sets of small subbasins are used in the method, one set of 100 for validation (covers the large-scale basin) and another set of 51 for Monte Carlo resampling. Climate grids, whose resolution determines the lower size limit on the smaller basins, are used to extract precipitation and potential evapotranspiration that are used to somehow estimate streamflow; I would assume their relationship to streamflow data. It appears as though the author allows the sample size of small basin climate cells to vary in the resampling,

and then compares their large-scale flow estimates to the large basin validation set so that the method can be tested for accuracy using a mean-standardized RMSE. The minimal number of small basins needed for accuracy is found upon convergence of the RMSE.

While this method seems like a novel approach worthy of publication, the paper is vague in its lack of definitions, details of estimations including mathematics, and overall seems to be written to a very targeted audience. Defining terms and fully explaining procedures not only broadens the target audience but also benefits the author greatly as it increases the paper's potential for citation. It took me a couple of readings to get a general idea as to the method, and yet, I'm still not seeing the technical details of estimation, evaluation and inference, which are important in a methods paper.

The structure of the paper, i.e. sections and their order, is largely intact; however, for better story continuity and readability section 4.3 on Budyko's equation should go in the theory section and the theory section should come after the introduction.

Aside from grammatical errors, of which there are many, some problems need brief mention prior to any rewrite: (1) the paper as a whole is wordy and could be more terse than its current form, (2) potential evapotranspiration should replace potential evaporation (i.e. PET) throughout, (3) a bit more detail in discussing figures is needed to get inference across, and (4) the figures need careful attention to details and their captions are not always clear.

Since the RMSE is the sum of the variance and the square of the bias, the author assumes an unbiased estimate of their model. Is this a reasonable assumption? Moreover, the variance used in the SRMSE is of biased form having n rather than $n-1$ degrees of freedom in the denominator. Both of these points can be easily proven. Or is this form of SRMSE commonly used in practice?

The method has potential and would be useful to the hydrologic community. I am inclined to suggest this paper be accepted for publication, but not until a substantial

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rewrite. I will review again once these issues are addressed. A final suggestion to the author is to please allow someone who is not familiar with your project or your area of study to proofread your paper. When I do so with my own work, I always get great feedback from folks. Good luck on the rewrite.

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