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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Using modelled discharge to develop satellite-based river gauging: a case study for the Amazon Basin" *by* Jiawei Hou et al.

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General

The paper describes a method to use Earth Observations as way of estimating river discharge in ungagged basin. The method is applied to the Amazon river basin, and performance evaluated with a set of independent river gauged records.

The paper is very well written and clear, and aims to propose a solution to the challenge of lack of hydrological measurements in many parts of the world. The solution makes use of satellite imagery, which is great as more data are becoming available from the

EO community for hydrological applications. My recommendation is for publication after clarifications on a few points.

Major comments

Overall, the methods description is clear, and results show promising potential. However, I would have liked to see a commentary on the limitations of the method, in particular related to the use of hydrological model simulations to train the derivation of 'Satellite Gauging Reaches' SRGs. This is particular important in regions with no river gauged observations, as lack of observations can dramatically reduce hydrological modelling performance as no calibration can be undertaken. I believe the authors should also clarify their comment line 334-335 as this seams a circular argument, with hydrological simulations used to derive SGRs, and then SGRs used to calibrate hydrological models.

Another point of clarification regards the justification of the optimisation/ training strategy: SGRs discharge estimates are based on a built relationship between water extent and modelled channel storage, and then another transformation from water extent to river discharge. I am not sure I follow why they are two independent methods, and why the water extent cannot be directly trained using simulated river discharge.

Whilst promising, the authors only found that the methods could be applied and evaluated over less than 1/3 of gauged rivers (and 1/6 for the GFDS method). It would be insightful to have a commentary on the overall applicability of the method, and ways of improvement.

Minor comments

- The Data and methods section does not contain information on the data used for the hydrological mode, in particular the source of rainfall and potential evaporation time series. A commentary on the calibration of the model, especially if it used any of the 31 river gauges considered in the study, would be an important addition.

- The description of the GFDS dataset is not very clear, in particular regarding the time step of the time series (4days or 1 day?).

- line 143: change ‘resampled to 8-day averages’ to ‘averaged to 8 days’.

- Section 3.1: it would be useful to know on how many points the evaluation is conducted over (for fig 2 and 3).

- Lines 222-225. Can you please provide a more quantified metrics, for example the number of false attributions in relation with the window size?

- lines 231-232: The last sentence is presumably referring to GFDS: please clarify.

- Section 3.2: can you justify the use of a correlation threshold of 0.6? Please also remind the reader that the vertical axes in fig 5 are not the same for observations and simulations. It would also be important to comment on the relatively low number of sites where the method is judged ‘applicable (about 1/3 for MODIS, and only 1/6 for GFDS). As GFDS shows a relatively better performance in reproducing river discharge time series than channel storage (fig 5), it might be useful to consider a slightly lower threshold for the overall performance analysis.

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