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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Spatiotemporal Assimilation/Interpolation of Discharge Records through Inverse Streamflow Routing" *by* Colby K. Fisher et al.

L. Read (Referee)

laurareads@gmail.com

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The purpose of the paper is to demonstrate a methodology for calculating 'continuous' streamflow from a network of point observations, with the targeted application to estimate global discharge in areas with sparse measurements. The topic and methodology are of great interest to the community as we are making progress on increasing access to observations worldwide and growing the remote-sensing based methods for estimating a host of hydrologic variables including river discharge. I have several main concerns regarding the manuscript and then list several minor ones for improvement:

Major: My primary concern is on the generalizability of this approach in other basins

with differing characteristics. Of course the method can be applied, but the performance seems highly unknown. For example, given that there are differences in performance of the method based on drainage area, I can imagine other characteristics - mean annual flow, degree of landuse/urbanization, management, etc., also leading to differences in performance. In my opinion since the method was established in a previous work (2013), this paper should include some sensitivity (besides the distribution of gauges, which was a very nice experiment) analysis, either using real data (preferred) or synthetic. Further, only one year (2009) is tested - why? Was 2009 particularly wet or dry? These could also impact the results.

The second major question is the relative performance of the statistical (kriging) method compared with the one presented. While I agree with the authors that a statistically based approach sacrifices mechanistic understanding, it is important to understand the context of where this work fits in and how it compares (as the authors mention in the introduction).

The third major point is on the application itself, where this case was presented for the Ohio river basin, which is a heavily gauged basin (making it great for an initial test, but perhaps poor for demonstrating how the method could be used for global discharge). In my opinion, the likely errors and missing data in the observations are a big concern. In the US we have a relatively low occurrence of missing obs compared with other places in the world where I imagine this could be applied, so the question is: at what point does this method break-down - what are the stress-test results (how many missing obs lead to performance worse than taking the mean or some other metric at the nearest gauge, or applying a statistical method)?

Minor: -Only NSE was used as a metric of performance, but there are other statistics that would be interesting depending on whether the user is interested in floods/droughs - 7Q10, bias, etc. -Suggest revising the language in the results that often repeats "we can see" (pg 8). -Avoid saying whether the method did a "good job". Allow the reader to determine that from the results. -Fig 5: make axes and legend font larger -Fig 9:

caption has a typo. Dived –> Divided?

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