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Hydrology and Earth System Sciences
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Discussions

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

Anonymous Referee #2

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My main concern is in regards to the transferability of this method to the remote basins with sparse data, extensively discussed in the conclusion. Given the experiments run, I'm not convinced this method would do any better than the simpler statistical methods typically used for ungagged basins. Line 18-21, page 6, you write "Based on the previous work of Pan and Wood (2013), the wave velocity parameter and the smoothing window for the Ohio River basin were set at 1.4 m/s and 70 days, respectively." In that previous work, Pan and Wood set the values based on a streamflow model calibrated to the gages. How would you get these parameters without having already had a good working model with a lot of data in the area? In my experience, models are very sensitive to wave celerity values, which vary with discharge in non-linear models. So to

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leave a value constant through time, especially with no model/data to back it up and also considering interannual variability: this all seems quite risky. I feel this paper could be greatly improved by backing down the conclusions and instead perhaps alluding to the possibility that such conclusions will be tested in future work somehow (at least comparing the method to other methods in such basins). Or, as the previous reviewer suggested, more sensitivity studies should be presented, in regards to the velocities assumed and the period of record tested on, since the Pan and Wood (2013) paper has already presented the novel method of inverse routing.

1. Does the paper address relevant scientific questions within the scope of HESS? Yes.

2. Does the paper present novel concepts, ideas, tools, or data? Somewhat. This paper is an extension of the ideas in the 2013 paper by Pan and Wood, that was published in HESS. That paper was much more novel. I find this paper interesting, but mostly in the concept that was already introduced in Pan and Wood. I am not convinced that the claim of the method being better that traditional methods in remote basins without good data is an accurate claim, so I'm not convinced of the novelty of the idea extension.

3. Are substantial conclusions reached? Yes, but I'm not sure they are justified.

4. Are the scientific methods and assumptions valid and clearly outlined? Yes, this paper is extremely well written.

5. Are the results sufficient to support the interpretations and conclusions? The interpretations are supported but not the conclusions.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? Yes.

11. Is the language fluent and precise? Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No.

14. Are the number and quality of references appropriate? Yes.

15. Is the amount and quality of supplementary material appropriate? Yes.

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