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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A topological restricted maximum likelihood (TopREML) approach to regionalize trended runoff signatures in stream networks" *by* M. F. Müller and S. E. Thompson

Anonymous Referee #2

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The manuscript entitled "A topological restricted maximum likelihood (TopREML) approach to regionalize trended runoff signatures in stream networks" is a nice study that is bridging recent geostatistical regionalization work in terms of Top-kriging (Skoien et al) and Restricted Maximum Likelihood (Cressie et al as well as Ver Hoef & Peterson). The text is well written and the TopREML approach is presented is a very clear fashion. In my review of the work, I think the single result of Figure 5 (c) showing the Monte Carlo-based verification of the TopREML's improved representation of estimation uncertainty is the core publishable contribution. The relative differences of the best estimator methods (e.g., Figure 4) is less compelling as would be expected from the

prior literature that has shown the mean or median estimates are generally less sensitive to methodological choices. Overall I appreciated the authors efforts of motivating their methodological strengths and weaknesses in the linearity assumptions implicit to the signatures being estimated. Overall I think this work is very publishable in HESS and will constitute a nice contribution. I do however have the following comments, which in my view if addressed well would significantly improve readers' ability to really understand the core contributions and implications of the work.

Major Comments

1. Generalize the Case for the Computational/Methodical Enhancements: In the Introduction lines 16-28 on page 1359, later in Section 2.6 as well as Section 5.1 lines 13-19 the authors discuss the computational challenges of Top-kriging, the implementation of TopREML, and make an offhand remark regarding substantial computational savings (15% of relative runtime). Overall the topic of the TopREML implementation and its general computational complexity (both numerical and ease-of-use) is handled with a lack of rigor relative to the other portions of this work. This is unfortunately hiding a core benefit and major contribution if the authors can substantiate formally why they get the savings and how it should scale with problem complexity. It may seem that the PUB science is disconnected from this, but as the authors themselves argue the complexity of the approaches can restrict the scope of science questions or applications. The authors hint at some of the broader implication of this in Figures 6 & 7. I would suggest a figure of the general computational complexity of TopREML that results from its algorithmic support as well as its data requirements would fundamentally improve this work.

2. Related to my comment above, Figures 6 & 7 start to frame the broader implications of the work and its relative strengths/weaknesses. Overall this portion of the paper's exposition seems less mature. Sections of text where more analysis could be helpful include Section 5.1 lines 14-22 and Section 5.2 overall. I suspect if you can make a case for the general computational and mathematical weaknesses and benefits of

TomREML more formally (my comment #1) than the implications of this work for PUB can be sharpened significantly.

Overall I think the authors have done nice work.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 1355, 2015.