

Interactive comment on “Hybridizing sequential and variational data assimilation for robust high-resolution hydrologic forecasting” by Felipe Hernández and Xu Liang

Anonymous Referee #1

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This study presents a hybrid approach to data assimilation by combining parts of sequential and variational algorithms, and demonstrate its applicability to hydrologic forecasting. The topic is interesting and appropriate for the journal. However there are certain problems with both the experiment design and the results. The design of the experiment is a bit confusing, no explanation is really given on the choice of the parameters etc., while the “scenarios” are not really explained well. In many of the comparisons the proposed algorithm degrades the performance when compared with the “default” model, which begs the question on why that is. Would a simpler data assimilation technique yield the same results? Given the increased complexity of implementing the OPTIMISTS algorithm compared to say an EnKF, I believe including the results

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from implementing a simpler assimilation algorithm should be included. In addition, the description of the algorithm is somewhat confusing and doesn't answer the basic question of how this algorithm addresses the limitations of the sequential and variational approaches. Overall, I'm not convinced by the presentation or the results that this method can be superior to other data assimilation approaches as is stated in the Conclusions section.

p. 1, l. 22, “growing complexity”: perhaps add a reference. p. 2, l. 30: what are these disadvantages? p. 3: does an observation need to be available for each assimilation step (seems so from l. 8)? In that case, wouldn't the assimilation step be limited by the observation time step? I suggest rewording l. 5 to clarify that. p. 3, l. 15: state variables can be output values too. I think using the term “predicted measurements” would be more appropriate. I have to admit I was confused by Section 2. The proposed algorithm seems like a mishmash of ideas from other algorithms, and there is no clear explanation what limitation the authors were trying to address by each choice they made on their algorithm. For example, why is it more advantageous to generate random samples to supplement the root samples with? Couldn't Steps 2-5 be replaced with one of the evolutionary algorithms that the authors are already using? What led the authors in choosing a complicated optimization algorithm that combines a GA and Metropolis-Hastings sampling instead of something simpler? Why isn't resampling adequate to account for sample impoverishment? I strongly suggest the authors rework the entire section by splitting the algorithm into its components and providing an explanation under each step describing what it does, what was the issue with current state-of-the-art approaches and how their proposed algorithm solves the issues. It would probably be useful to add a flow diagram describing the algorithm. With all the approximations made to make the algorithm computationally tractable, what is the advantage gained in comparison to simpler assimilation approaches? p. 8, l. 29: it's not clear why the experiment were configured the way they were. What was the rationale behind the choices in the parameters? For example, w_{root} is set to either a value or a range of values, but nothing really has been said about its significance or the possible

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values it can take. When its value is within a range, did those values represent one of the factors in the factorial experiments? What were the discrete values used, if that is the case? Also, it might be good to reformat the table and use vertical lines to partition the columns. p. 8, l. 21 “To assess the performance of OPTIMISTS, three forecasting scenarios were selected...”: what are these scenarios? p. 9, l. 1-2: were the meteorological forecasts sampled from a long-term climatology, i.e. ESP? p. 9, l. 4: this should be Section 4. p. 9, l. 13: why not add a table with the summary performance metrics? p. 9, l. 28-29: how much was the error reduced during the training period? p. 10, l. 6-8, “If these results cannot be generalized to challenge this entrenched assumption, they at least indicate that OPTIMISTS can efficiently encode the state probability distribution of complex models using relatively few particles” which directly translates to better scalability to higher-dimension applications.”: I don’t think that’s necessarily true, since this assumes that there is no dependence on the sampling strategy to estimate the PDF. I suggest either augmenting this to strengthen the statement or restating. p. 11, l. 5: what is the ACF of soil moisture? p. 11, l. 22-24, “our analysis demonstrated the benefits of judging candidate initial state assignments, not only for their ability to reduce the observational error but also, for their consistency with the model’s physics”: wouldn’t that be moot if the state was always generated by the model? p. 11, l. 25-27 “While a formal comparison with 4DEnVar (Buehner et al., 2010) or a similar hybrid method deserves an investigation on its own, this feature adds to a set of characteristics that makes our approach superior, at least conceptually”: I’m having a hard time with this statement, especially given the results and the lack of any comparison with other DA approaches. p. 11, l. 30: unless I’m missing something, the proposed method reduced the forecast accuracy in a number of cases and not just “one of the studies scenarios”.

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