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Discussions

Interactive comment on "Variational Assimilation of Streamflow Observations in Improving Monthly Streamflow Forecasting" *by* Amirhossein Mazrooei et al.

Anonymous Referee #2

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This is well-organized paper analyzing the effectiveness of variational data assimilation approach to improve monthly streamflow forecast. Although the authors provided a comprehensive discussion on the strengths of the variational data assimilation approach in improving streamflow forecasts, the way they implemented the variational data assimilation is not consistent with its definition. Additionally, I have several other serious concerns about this study. All in all, I do not find this study novel nor provides insight/unique findings that makes it publishable in HESS.

Major Comments

Page 1, line 7: It is not clear what does the frequency of Data Assimilation (DA) appli-

cation mean? The length of the assimilation window (t) is the time interval for which the variational cost function is minimized, and its frequency depends on the entire period of study, as it is calculated by (entire period, T)/(assimilation window, t). Therefore, this is a bit vague as the authors defined it as one of the "independent" parameters of variational DA approach.

Page 2, line 16: Please include hydrologic studies, such as drought monitoring and flood forecasting, as well.

Page 3, line 8: In situ streamflow observation generally contains substantially lower measurement errors compared to satellite retrievals. Please include a reference for this statement.

Page 3, lines 8-10. Yes, this is true for hydrologic data assimilation based on lumped- or semi-distrusted hydrologic models. However, for fully distributed hydrologic models, such conclusion is rather speculative and less conclusive, as the impact of assimilating satellite soil moisture versus streamflow observations into fully distributed hydrologic models has not been fully explored according to the literature.

Page 3, lines 20-24: It is unclear how the authors believe assimilating point-measurement, such as observed streamflow at gauge, into a gridded hydrologic model (i.e., Variable Infiltration Capacity, VIC) using a variational DA assimilation is essential, knowing that many studies have already used the ensemble DA approaches such ensemble Kalman filter (EnKF) or Particle Filter as a more efficient approach under similar conditions. I suggest the authors use more encouraging and tenable explanation to justify the necessity of for this study.

Page 3, line 29: After reading the introduction section, I am still not sure why variational data assimilation approach is being used in this study.

Page 5, section 2.4 and 3.1: The spatial downscaling and temporal disaggregation of precipitation forecast data along with calibrated model parameters for the Tar River

Basin were directly borrowed from authors' other studies.

Page 6, section 3.2: Equation (1) shows that the authors used strong-constrained 4DVAR assimilation approach where B (background error covariance matrix) and R (observational error covariance matrix) are the only error covariance matrices. In the strong-constrained formulation, we do not have model error covariance matrix (Q). This means that the model error covariance matrix is zero, unlike the week-constrained formulation that includes all three error covariance matrices, B, R and Q. With this introduction, the equation (1) should be used for the synthetic case where the model error (Q) is zero (perfect model assumption). However, the present work is based on a real case, which is inconsistent with the definition of the variational data assimilation approach implemented.

Page 6, lines 25-29: It is not clear what approach was used for the minimization of the cost function, as the tangent linear and adjoint versions of the forecast model is not available.

Page 7, section 3.3: As highlighted in this section, the goal of this study is to correct the VIC model's initial state to improve monthly streamflow forecasts. To accomplish this though variational DA approach, the cost function should include background error covariance matrix (B) (along with other matrices if necessary) as it represents the uncertainty in the initial condition. However, the authors excluded this matrix from the cost function (J) for the sake of simplicity and low computational load. I am not sure then, how the authors are addressing the uncertainty in the model states (soil moisture) while equation (2) only has observational error covariance matrix that represents the uncertainty in the USGS gauge observation.

Page 7, lines 27-28: To minimize the cost function (J), the VIC model should be initialized with the background (prior) state variable, which is calculated as Xb = x(initial guess)+Epsilon and Epsilon belongs to N(0,B), where B is the background error covariance matrix. The result (optimized initial condition), is the X analysis (or let's say Xa).

The authors are using a "k" (!?) factor to generate the analysis state variables and use them to initialize the VIC model during the optimization process. This is inconsistent with the cost function definition in the variational DA approach.

Page 15: line 21: 7-days was identified as a more effective assimilation window size to implement the variational assimilation approach for streamflow forecasting. Please provide a reasoning for this choice or back up your claim with a previous study which has done this.

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