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Interactive comment on "**Large scale hydrological model river storage and discharge correction using satellite altimetry-based discharge product**" *by* **Charlotte Marie Emery et al.**

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We would like to thank the reviewers for their very constructive comments. Please find below our replies.

1. In the introduction, the third paragraph is about the resources of model errors. In my opinion, a description of the accumulation of model errors and other uncertainties in model's prediction which can lead to the collapse of the model should be added. I think this part can be regarded as a part of the explanation of the necessity of data assimilation. I recommend to shorten the

section on model error sources and add some sentences on the impact of accumulation of uncertainties.

Authors' reply: Following your comment, we propose to reduce this paragraph by shortening the list of model uncertainty sources (which are the lack of knowledge on the real physics, the numerization/discretization induced errors and the uncertainties in the inputs parameters and forcings). Also, we will add an ending remark stating that the accumulation of all these uncertainties could lead to the model collapse if they become too important. Therefore, we propose to rephrase this part of the introduction as follow:
"However, even if hydrological models become more and more accurate, inherent model uncertainties are unavoidable. They originate from several sources: the lack of knowledge in the real physics, the numerization/discretization-induced errors and the uncertainties in the inputs parameters and forcings). All these uncertainties impact model's outputs. In the worst case, all those uncertainties could accumulate and result into the collapse of the model. The model gives therefore an approximate view of the system real state."

2. In Lopez Lopez (2016) discharge data was assimilated into a model for the Rhine basin. I think this paper should be added to the introduction. (López López, P., Wanders, N., Schellekens, J., Renzullo, L. J., Sutanudjaja, E. H., and Bierkens, M. F. P.: Improved large-scale hydrological modelling through the assimilation of streamflow and downscaled satellite soil moisture observations, Hydrol. Earth Syst. Sci., 20, 3059-3076, https://doi.org/10.5194/hess-20-3059-2016, 2016)

Authors' reply: Thank you for this suggestion. We will definitely add this citation in the

7th paragraph of the introduction, next to the citation of Trudel et al. (2014).

3. Page 3, in last second paragraph, the aim of this study only mentioned that EnKF is applied and also said something about the model and observations. From my point of view, the using of localization methods should be mentioned. The localization methods are the crucial key to this case study and without localization, the academic value of this research will mostly be as a case study of an otherwise known method into a new geographical area.

Authors' reply: We totally agree with the reviewer's suggestion. We are going to formulate the paper aims differently. The modified aims will state the following scientific question: how can we use remotely-sensed data at a river reach scale in order to improve a large-scale model. Because of the "local" information provided by the satellite and the model errors (and the difficulty to estimate them), the use of localization method is needed. Therefore, in the same paragraph, we will add some sentences to explicitly mention the need of "localization methods" in the quick method description. We propose to reformulate the objective as follows:
"The objective of the present study is to investigate the contribution of remote-sensed data that provide local information to improve a large-scale RRM via DA. The scale difference between the observations and the model lead to also study the need to use localization methods within our DA framework. We used an Ensemble Kalman Filter, to which we added a simple localization module, to assimilate discharges derived from ENVISAT water surface elevation measurements. These observations are used to correct the state of the large scale Total Runoff Integrated Pathways (TRIP, Oki and Sud, 1998) RRM version included in the land surface modelling platform "Surfaces Externalisées" (SurfEx, Masson et al., 2013), and developed at the Centre National de Recherches en Météorologie (CNRM, France). This particular version is de-

noted by the CTRIP acronym hereinafter. CTRIP is coupled with the Interactions-Soil-Biosphere-Atmosphere (ISBA, Boone et al., 1999) LSM at a resolution of $0.5° \times 0.5°$."

4. In section 2, I think the description of the CTRIP RRM is excessive. It would be better to make the context of this model shorter and simpler. Maybe the authors can reference one of their earlier papers on the model and point to that for the model description.

Authors' reply: As proposed by the reviewer, we will reduce the size of the CTRIP model description. First of all, we will remove the model equations along with the description of its parameters, as they are already described quite well in previous papers. Instead, the reader will be directed to these papers (especially: Decharme et al., 2010; Decharme et al., 2012). We will keep the description of the ISBA model as it is, as it is already quite synthetic.

5. In section 3, it has the similar issue just like section 2, the explanation of the fairly standard EnKF is too long.

Authors' reply: Similarly to the previous comment, we will reduce the length of the subsection by getting rid of the first paragraph that focuses on the classical Kalman Filter. So this subsection will directly start with the EnKF description and equations. We will also remove Eq.15, as the matrix in the equation is already described in the preceding text.

6. *Section 3.2.4 (Localization) does not belong to "3.2 Generating the ensembles". In my opinion, the following structure of section 3 is better. Firstly, introduce the control variables and observations separately. And then, give a short introduction to EnKF theory. Following, present how to implement the EnKF with localization specifically. Last part includes diagnostics and experiment set up.*

Authors' reply: Thank you for this suggestion, which will make the paper easier to read and highlight the localization method. We propose the following reorganisation of the third section:

- 3. Method

  - 3.1. Data assimilation variables

    - 3.1.1. Control variables
    - 3.1.2. Observations variables

  - 3.2. EnKF for ISBA-CTRIP state estimation

    - 3.2.1. EnKF theory
    - 3.2.2 Localization
    - 3.2.3. Generation of ensembles

  - 3.3. Assimilation diagnostics

7. *In page 14, section 3.2.4, the second paragraph, it said that there are three localization methods. I prefer to state that there are two common localization methods, namely local analysis (R localization) and covariance*

localization (B localization) . These two methods can be found in following two papers. Balance and Ensemble Kalman Filter Localization Techniques (doi:10.1175/2010MWR3328.1) -Relation between two common localisation methods for the EnKF (doi: 10.1007/s10596-010-9202-6).

Authors' reply: Thank you for the suggested publications. We will include them and present the two common localization methods in section 3.2.4.

We suggest the following changes to the section:

"It exists two types of localization techniques (Greybush et al., 2011; Sakov and Bertino, 2011). The first one is called B-localization. It is based on explicitly modifying the background error covariance matrix $\mathbf{P}^b_{e,k}$. The latter is multiplied by a correlation matrix generated from a radial function, namely a function of the two/three spatial dimensions which monotonously decreases with the distance between control variables (Hamill et al., 2001; Houtemaker and Mitchell, 2011; Houtemaker and Mitchell, 2005). A sparse matrix $\tilde{\mathbf{P}}^b_{e,k}$ is therefore computed, with non-zero elements centred around the matrix diagonal. This modified matrix replaces $\mathbf{P}^b_{e,k}$ in the calculation of the Kalman gain matrix $\mathbf{K}_{e,k}$. The other common localization technique is called R-localization or Local Analysis. This one consists in proceeding the analysis step into characteristic sub-spaces of the overall problem space."

8. *The names of the different localization schemes should align with the common names in the data assimilation field. I recommend the terms in those two papers in point 7, above. In table 3 and corresponding parts in the main body of this research should change "-local suffix" to "Local analysis" or "R localization" and also replace "-diagonal suffix" with "covariance localization" or "B localization".*

Authors' reply: Given our understanding of Greybush et al. (2011) and Sakov and Bertino (2011) articles, it seems that we applied a B-localization method for both the experiment with suffix -diag and -local. The difference between these two experiments is the "localization matrix" multiplied (via the Schur product) to the background error covariance matrix. More explicitely, to apply the localization, we used the same equation than Moore (1973) and Biancamaria et al. (2011):

$$\mathbf{x}^a = \mathbf{x}^b + \left[\mathbf{S} \times \left(\mathbf{P}^b\mathbf{H}^T\right)\right] \left\{\mathbf{H}\left[\mathbf{S} \times \left(\mathbf{P}^b\mathbf{H}^T\right)\right] + \mathbf{R}\right\}^{-1} \left(\mathbf{y}^o - \mathbf{H}\mathbf{x}^b\right),$$

where $\mathbf{S}$ is the localization matrix and "$\times$" is the Schur product.
We will clearly state in the manuscript that we applied B-localization methods. As both methods are B-localization and we will explicitly mention it in the manuscript, we prefer not to change the names of the experiments.

9. On page 16, the part before 3.3, it describes how to get the localization matrix. In this study, the author used localization matrix to multiply covariance matrix directly. This way is not wrong but it differs from the most common way to implement the localization methods. Can you use equations to display the formulation of localization matrix? This is helpful for readers to understand your localization methods.

Authors' reply: We apologize for not giving the equation in the previous version of the manuscript. The equation we used (as mentionned in our previous comment) is taken from Moore (1973) and Biancamaria et al. (2011):

$$\mathbf{x}^a = \mathbf{x}^b + \left[\mathbf{S} \times \left(\mathbf{P}^b\mathbf{H}^T\right)\right] \left\{\mathbf{H}\left[\mathbf{S} \times \left(\mathbf{P}^b\mathbf{H}^T\right)\right] + \mathbf{R}\right\}^{-1} \left(\mathbf{y}^o - \mathbf{H}\mathbf{x}^b\right),$$

where S is the localization matrix and "×" is the Schur product. We will add this equation in the new section 3.2.2 ("Localization").

10. *In table 1, the size of the ensemble is 101. The authors do not justify the choice of exactly 101 ensemble members in the paper. It is not possible for the reviewer to see if the ensemble size represent the distribution of model states properly? Could the authors use some figures or the rank histogram of the ensemble to show the gaussianity of the ensemble? Otherwise, could the authors justify the choice for 101 ensemble members?*

Authors' reply: In our data assimilation platform, the ensemble size of exactly 101 instead of 100 is for implementation convenience. In the literature (e.g. Greybush et al., 2011) an ensemble size of 100 is often chosen to have a large ensemble, while maintaining a reasonable computational time of the data assimilation experiment. The suggestion of adding ensemble histogram is a very good suggestion, however, because the paper is already quite long, we prefer not to add such histograms in the present study. However, we will add this explanation in section 3.2.3 ("Generation of the ensembles") such as "To get a large ensemble, while maintaining a reasonable computational time, the ensemble size has been set to 101 members."

11. *In data assimilation applications with localization usually a common localization function is used. Common example is a fifth order function of Gaspari and Cohn. I didn't find the description of the localization function in this paper. If I missed it, please point out its location. If the authors didn't use it, could the authors explain reasons and considerations?*

Authors' reply: You are right, in the present study, we did not use such radial functions. The localization matrices we built only contain 0 or 1. The resulting "localized" error covariance matrices contain the exact same values as the error covariance matrices before localization, but only at elements corresponding to a "1" in the localization matrix. In a way, the localization only allows to suppress spurious effects of far away cells. The reason why we did not use functions such as the fifth order Gaspari function is that, in the literature, we only found use of this function in atmospheric and oceanographic applications where the study domain are full 2D/3D domain. It has also been used, to our knowledge, in river hydraulic model applied to a single reach (1D) river network with no modeled tributaries between the beginning and the outlet of the river reach (e.g. Biancamaria et al., 2011). What we want to highlight here is that the case of a complete river network is different from the atmospheric and oceanographic framework, as for this case discharge within the river network is not a 2D/3D field. It is a "multi-1D ramified network". Therefore, it appeared to us that we could not directly use the same localization method as in atmospheric and oceanographic applications (applied on a sphere or disk of influence). Using such kind of radial function is tricky, as they could only be applied locally over the different 1D river reaches and the case of pixels corresponding to connections with another tributary or with the mainstream is not trivial. That's why, we decided, as a first step to develop a localization method for a basin-scale river network application, to have a very simple localization function: the first developments of our localization method only select elements of the error covariance matrices but do not modify their value. Further development of the localization method may consider the use of such function within the river network. We discussed this difference between 2D/3D model atmospheric and "multi-1D" river model in the description of the localization method we used for our study (in section "Localization of the error covariance matrices").

12. *It is common that localization methods can cause imbalance. The analysis of imbalance can show the performance of localization method in specific application. I recommend adding the imbalance analysis. If the authors think it is unnecessary, could the authors explain the reasons?*

Authors' reply: Thank you for this suggestion. Greybush et al., (2011) clearly show the interest of an imbalance study. However, we do not think it is necessary for our present study. Indeed, the localization method do not use radial function (see our reply to the above comment), but just select elements in the error covariance matrices. Therefore, non-zero elements in the matrix are not modified. Moreover, the CTRIP model is a relatively simple model compared to complete atmospheric model used in Greybush et al., (2011). Indeed, CTRIP is based on a linear reservoir model. Its only prognostic variables are the water storage in 3 reservoirs and the discharge is deduced from the storage at each time step (diagnostic variable). Therefore, we think that the issues discussed in Greybush et al., (2011) should not be as critical in our study.
That's why we decided not to add the imbalance analysis. However, we suggest to raise the imbalance question in the paper perspectives.

13. *I am a bit confused about the chosen localization scales. The "diagonal error covariance matrix" in this paper is to apply B localization method to form the localized covariance matrix. In this paper (Relation between two common localization methods for the EnKF, doi: 10.1007/s10596-010-9202-6), the author used a figure to show the influence of B localization (covariance localization) method to the error covariance matrix. The result of this localization method is mentioned in the paper. It said that "with non-zero elements centered around the matrix diagonal"(page 15, the*

first two lines). You only keep the elements in the matrix diagonal which means you only use the fixed localization scale. And also, in your "-local suffix " case, if I understand correctly, this is the "Local analysis" or "R localization" in data assimilation. When you design and set the influenced areas, you still used the fixed localization scale. Could you explain the reasons why you only use a fixed localization scale in your experiment set-up? Can you also explain how this localization scale was chosen? In the results part, the "local" case has a better performance compared with the "diagonal" case. Can the authors elaborate on the impact of different localization scales on the performance of DA?

Authors' reply: What we wanted to do with the "-diag suffix" experiment is to test the behavior of the data assimilation platform in the extreme case of an error covariance matrix with a zero-correlation length (diagonal matrix). This experiment has the objective to illustrate the case when the model cells are completely independent from any other cells in the catchment (we only correct at the location of the observation). This experiment is then the opposite of the "-direct" experiment, where the correlation length can be considered as infinite (because there is no localization). From these two "extreme" experiments, we wanted to build an intermediary case: the "-local suffix" experiment. The local experiment can be seen as a "proof-of-concept" experiment, to show benefit of using observations to correct surrounding pixels. It should avoid the spurious correlations observed in the "-direct" experiment (due to the generation of the ensemble) and should improve results from "-diag" experiment, as observations correct more than one pixel. We chose a fixed localization scale for simplicity and as a first step in the feasibility study of the development of a localization method for a hydrology application.

The correlation lengths were determined with respect to the averaged flow velocity in the river. From the averaged flow velocity, we can deduce the traveled

distance over one assimilation window (which is one day here) and express it as a number of model grid cells. Therefore, for any given cell and over a day, the "area of influence" represent the set of upstream cells whose water flow will pass through the given cell during the day (which will therefore contribute to the discharge at the given cell) and the set of downstream cells that will receive the water from the given cell during the day (which will therefore receive discharge from this given cell).

Concerning the last question, to our understanding, both experiments (i.e. "-diag" and "-local") correspond to B-localization. In the local-experiment, the localization mask is more realistic as there are more than one cell impacted by the modification of a given cell. Therefore, the data assimilation results are logically better. We could add one or two sentences about this point in the discussion section. Finally, the main perspective of this study is to develop more elaborated correlations lengths (as it is discussed in the 4th paragraph of the paper conclusions). These more elaborated correlations lengths should be built by trying to localization method used in the literature, adapted to the specific case of a river network at a basin-scale (see our reply to comment number 11).

14. In page 24, the last paragraph, the authors state that there are two ways to improve DA. A more realistic ensemble method to generate ensemble and observation correction algorithms can help to get better performance. These two conclusions are right. But, in your analysis part, you didn't compare the situation with specific ensemble generating method and the situation with generating ensemble randomly. In my opinion, no evidence in this paper can support this conclusion. Similarly, the second conclusion is not conclusive. Can you rephrase these two conclusions and make them open?

Authors' reply: You are right, we should rephrase our last paragraph to make these two points more open. We will change the paragraph as follow:

"To improve these DA results, several aspects could be investigated. For example, it could be studied if a more realistic ensemble method generation could be helpful. In the present study, only the model initial condition and the precipitation forcing are perturbed to generate the background forecast ensemble. More uncertainties in this ensemble could be added by also perturbing CTRIP parameters and/or ISBA outputs. Another DA aspect to look into is the potential use of a smoothing data assimilation algorithm, such as the Ensemble Kalman Smoother (Evensen and Van leeuwen, 2000). A smoother could help to have less "variability" in the corrected discharge. Finally, the assimilation scheme presented in this study could be applied to other river basin in the world, as ISBA-CTRIP is a global LSM. However, more work is needed to apply the DA platform at global scale."

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