Hess-2019-242-RC2

Reply to Claire Michailovsky's comments

General Comments

This paper presents a data assimilation experiment using synthetic observations from the planned Surface Water Ocean Topography (SWOT) mission to update the Manning roughness parameters of a large scale routing model. The assimilation method used in the Asynchronous Ensemble Kalman Filter (AEnKF) with a 21-day time window (one orbit repeat cycle) which allows for measurements at different locations acquired at different times to be included in a single assimilation step.

The application of the AEnKF is logical considering the specificities of the data and this is a useful contribution to the work preparing for SWOT.

The authors would like to thank referee Claire Michailovsky for her detailed and very helpful comments. She raises very pertinent points that helped us better articulate our purpose. Please find below our replies and associated modifications to the manuscript with which we hope to address her concerns. To easily address all comments, we numbered each comment: they are referenced with a "M" for the major comments and with a "m" for the minor comments.

We kept the reviewer's comments and question in bold while our replies are in italic. When the associated manuscript's modifications remain small, we inserted the modified paragraph in our reply in plain text: the black text corresponds to the original unmodified text, the crossed text corresponds to deleted text and the blue text corresponds to new text.

*The abstract, the introduction (the latest paragraphs) and the discussions sections have been more updated due to several remarks from all reviewers. Therefore, we attached to our reply separate files with the new version of these sections. In these rewritten sections, we used a color code to differentiate which reviewer made the comment and suggested a modification: comments from reviewer 1 (Hessel Winsemius), 2 (Claire Michailovsky) and 3 (Paul Bates) are in purple, orange and green respectively. Each modification is also referenced by a code in bracket indicating the reviewer (R#***) and the type and index of the comment (M/m#***) such as: "[R#3-M#1]".*

I recommend publication with major revisions, including a thorough review for improving language.

Reviewer #1 had a similar remark. Therefore, while preparing the replies to the reviewers, we submitted the manuscript to an independent English-speaking proofreader to improve the English after taking into account your own corrections.

MAJOR COMMENTS

My main comment relates to the specificity of the study as a synthetic experiment and with how some of the simplifications/assumptions are presented. I recommend further discussion on the impact of these on a study using real data, and on the estimates of uncertainties which are crucial to any assimilation experiment.

Thank you for this important remark. Reviewer #1 raised similar issues and also asked for more discussions regarding the impact of the OSSE's assumptions going towards more realistic *experiments. Therefore, the "Discussions" section will be profoundly updated to take into account all the points raised by the reviewers. Please find below our more detailed reply.*

These are specifically:

M#1 - The assimilation of depth rather than elevation

We agree that SWOT will provide water surface elevations and not water depths. The assimilation of water depths in the first experiment serves as a reference or benchmark, as we expect it to perform well. Then, going towards more realistic experiments and directly use water elevations as observations, the first challenge would be to deal with the change of reference between the water depths simulated by the model and the water elevations measured by the satellites. To do so, we need to know the elevation of the bottom of the river bed referenced to the same geoid as the observations. Yet, this information is generally not well known. Therefore, we chose to use water surface elevation anomalies as observations because this type of variable does not depend on a reference or a bathymetry.

This essential point (use elevation anomalies to overcome the issue of unknown bathymetry) constitutes one of the main motivation for this study. Still, we realized it was not clearly stated neither in the abstract nor the introduction. Therefore, we modified these two sections.

- *Modified abstract >> see attached document: Hess-2019-242-corrected-abstract.pdf*
- *Modified introduction >> see attached document: Hess-2019-242-corrected-introduction.pdf*

M#2 - The fact that the truth is generated by the same model:
- no model structure error
- there is a "real" Manning to converge to, which might not be the case with real data
- even in PE3, the bathymetry is not significantly changed, only the river bed elevation.

To our knowledge, the model structure error is still a challenging error to estimate and most of data assimilation studies assume no model structure error. However, when using ensemble-based model, a possibility to deal with this error is to enrich the background ensemble by considering more uncertainties from variables that are not necessarily included in the control vector (including errors in forcings or parameters from both the LSM and the RRM). The capacity of such ensemble to deal with the model structure error can then be tested in a framework where we still use synthetic observations but generated from a different model. We added those remarks to the Discussions.

We agree that bathymetry errors are more complex than just errors on river bankful depth Errors on widths and also representativeness errors due to the use of a simplified bathymetry are also important. With the presence of such errors, one could expect to generate updated Manning coefficient distribution with unrealistic values that translate the incoherence between the simulated and observed bathymetry. We added a paragraph in the Discussions dedicated to these errors.

- *Modified Discussions >> see attached document: Hess-2019-242-corrected-discussions.pdf*

M#3 - Assumption of perfect forcing

Reviewer #1 had a very similar remark. We are aware that the "perfect" forcing assumption is a very strong simplifying assumption as the forcing control the amount of water entering the system. We do not plan on correcting the forcing with data assimilation here but rather calibrating the model given *the current forcing. However, to smooth the effect of forcing errors on the assimilation, it is possible to include them in the generation of the background ensemble. A dedicated paragraph in the Discussions section has been added to the manuscript to discuss these aspects.*

- *Modified Discussions >> see attached document: Hess-2019-242-corrected-discussions.pdf*

MINOR COMMENTS

m#1 - P6, l.21: this is brought up later when the anomalies are assimilated, but more focus should be placed here on the fact that depth is assimilated while SWOT will produce elevations. The conversion from level to depth is one of the big issues with using altimetry in hydrological studies. Does the SWOT simulator directly produce depths? You are assuming known river bed elevation, and this should be clearly specified. (I can see this is mentioned p11, l.26 but that is too late in the paper).

You are right. This aspect is not clearly set when describing the observations variables. We modified the corresponding paragraph accordingly.

- *Modified manuscript in section 3.2.1 (p.6, l.19-27)*

In the present study, the observed variables are water depths issued from a simplified SWOT simulator. Note that this simulator will produce water depths while the real SWOT satellite will provide water elevations. As in Biancamaria et al. (2011) and Pedinotti et al. (2014), our~~this~~ SWOT simulator replicates SWOT spatio-temporal coverage. At a given date, the simulator selects the ISBA-CTRIP cells contained (at least 50% of their area) in the SWOT ground tracks. Figure 2 shows some selected ISBA-CTRIP cells under the real swaths over the Amazon basin. The true run is used as a basis to get the true water depths Ytk. Then, to generate the observation vector yok from the extracted true water depths, each of them is randomly perturbed by adding a white noise characterized by a standard deviation σ_o such that:

Equation (6)

Using water depths observation is a strong simplification of the real SWOT product. Therefore, in order to take into account that SWOT will provide water elevations and not directly water depths, this study will look at the assimilation of both water depths and water anomalies. The method to generate the anomalies will be further detailed in Section 4.2.

m#2 - P7, l.9: similar issue as previous comment, elevation and not depth is required at 10cm accuracy. The depth accuracy will be much lower. You assume no representativeness error due to scale, but how about level vs. depth?

It is true that the 10cm vertical accuracy corresponds to water elevations measurements. We should expect higher errors on water depths, as we do not know the bathymetry. Still, it is complex to quantify such bathymetry errors and they only apply to water depths assimilation. As water depth assimilation is considered as a benchmark, we are still considering 10 cm vertical accuracy in our study for water depth. For water elevation anomalies, the error might even be lower, but as we are only considering white noise, we keep 10 cm for anomalies. We added a few remarks to acknowledge this aspect in the manuscript.

- *Modified manuscript in section 3.2.1 (p.6, l.27-p.7, l.9)*

The observation error is the addition of the measurement error and the representativeness error. The first is associated to inherent instrumental errors when processes are observed and the second represents the error introduced when the observed and simulated variables are not exactly the same (in nature or scale). Following the SWOT uncertainty requirements (Esteban Fernandez, 2017), SWOT-like water surface elevation measurements have a vertical accuracy of 10 cm (when averaged over a water area of 1 km2). This uncertainty accounts for measurement errors due to the remotely-sensed acquisition such as instrumental thermal noise, speckle, troposphere and ionosphere effects. Moreover, we omit error correlations along the swath so that observation errors follow a white noise model. Accounting for spatially-correlated observation errors is an active research area in the field of data assimilation (Guillet et al., 2018) that is beyond the scope of demonstrating the feasibility of assimilating SWOT-type data. ~~Finally,~~ In the framework of OSSE, observed and simulated water depths have the same scale as the ISBA-CTRIP model is used to generate both. Therefore, in the following, we assume there is no representativeness error related to scale in the system. However, it is worth acknowledging that we should expect higher errors on water depths, compared to water elevations, as we do not know the bathymetry. Assimilation of water depths is performed as a benchmark, against which assimilation of water anomalies will be compared to. Ultimately, σo is chosen equal to 10 cm for all observed variables (i.e. both water depths and water elevation anomalies).

m#3 - P8, l11: Would this be necessary if you had a better representation of your measurement error (re: previous comments)? What is the magnitude of this additional error?

Not necessarily. In our study, we are using a stochastic version of the EnKF method that is, each member of the background ensemble is updated using the analysis equation 12 (in opposition to deterministic version like the Ensemble Transform Kalman Filter, where the observations randomization is not necessary as the analysis equation is only applied to the ensemble mean and the covariance matrix is estimated from a transformation of the prior covariance matrix). According to Burgers et al. (1998), the randomization of the observation vector should be used so that all the variables within the EnKF are random. Without this, the observation vector is deterministic, which conflicts with the EnKF analysis scheme where the ensemble covariance is used instead of the error covariance. Moreover, we know that the stochastic EnKF tends to under-estimate the analysis error. Therefore, we generate different realization of the observation vector so that the analysis ensemble retains enough variability from one assimilation cycle to another and avoid "ensemble collapse" (when all ensemble members are identical).

This randomization is specific to the algorithm itself, not the data. Still, the magnitude of the randomization is generally fixed by the observation error (hence, the randomization will be minimal if the observations are very accurate). Therefore, the observation randomization is done by adding to each observation a random perturbation following a white noise with a standard deviation σ^o=10cm.This is now specified in the manuscript.

- *Modified manuscript in section 3.3 (p.8, l.11-13)*

To avoid ensemble collapse, the observation vector in Eq. (5) is randomized by adding a supplementary white noise with the same observation error standard deviation σ^o=10 cm (Burgers et al., 1998) such that

$$\forall\, j = 1 \dots n_y, \forall\, l = 1 \dots n_e, y_{k,j}^{o,[l]} = y_{k,j}^{o} + \epsilon_j^{o,[l]}, \epsilon_j^{o,[l]} \sim N(0, \sigma^o).$$

An observation ensemble is generated

$$Y_{e,k}^{o} = \left[y_k^{o,[1]}, y_k^{o,[2]}, \dots, y_k^{o,[n_e]} \right].$$

m#4 - At some points vague language is used (f.ex: "without really converging"), please be more precise.

Thank you for this advice. We carefully went through the manuscript and focused on correcting this language.

m#5 - P7, l.25: It is not clearly explained if xk are the Mannings coefficients themselves or the multiplying factors as described above. This issue is repeated elsewhere in the paper and while I understand xk does refer to the factors, please make sure this is clear throughout.

Reviewer #1 had similar comment, this section was confusing. Therefore, we modified the manuscript to clarify this aspect.

m#6 - Figure 8: the black lines cannot really be seen, perhaps increase the width?

Thank you for this suggestion. We modified Figures 8 and 10 such that the different curves can be seen more easily.

ADDITIONAL COMMENTS FROM SUPPLEMENT

m#7 – P2, l.9: it is not unavoidable that reductions in structure uncertainty are linked to increase in parameterization.

Following the reviewer's recommendation, we re-wrote this sentence.

m#8 – P2, l.27-28: This sentence feels odd right after the mention of important vertical errors.

Yes, this is true. In the previous sentence, we implies that the signal with high vertical error was then ignored, diminishing more the spatio-temporal coverage of the observations. To be clearer, the part "or with important vertical error" was deleted.

m#9 – P5, l.3: in time I agree, but is it not usually spatially variable?

To our knowledge, there are models where a spatially-constant Manning coefficient is applied such as Biancamaria et al (2009), Beighley et al.,(2009) and other with a spatially-distributed Manning coefficients such as Decharme et al. (2012). However, it is true that the most recent model developments use a spatially-distributed coefficient. Therefore, this sentence was added to the manuscript.

m#10 – P10, l.10: this is confusing, is this study a standalone PE? This paragraph is unnecessarily complicated.

You are right; the term "standalone" is confusing. We withdrew it from the manuscript.

m#11 – P11, l.26: this should be mentioned way earlier in the text.

Please see reply to m#1.

m#12 – P12, l.8: isn't it river bed elevations you are talking about?

Yes, this is right. We changed the term in the manuscript.

m#13 – P15, l.1-5: I don't see a clear distinction between the behavior of these 2 groups in the figures

This is true. Besides, for the interpretation of the results in the next paragraph, zones 1 and 9 are interpreted along with zones 2, 3, 4 and 5. Therefore, this item is deleted and zones 1 and 9 are now cited in the first item.

m#14 – P16, l.1-2: It is overstating it that the bathymetries are different, the shape itself remains the same, only the bed elevation is shifted.

This is true. We re-adjusted this sentence in the corrected manuscript and worked on the paragraph.

- *Modified manuscript in section 6.2 (p.15, l.32 – p.16, l.1-2)*

In the PE2 experiment, there is no difference of bathymetry between the simulated and observed water anomalies while the river bankful depth is different in the PE3 experiment.

m#15 – p17, l.2-6: I find it odd that this is how you begin the discussion. If that is why assimilation is introduced, then the natural conclusion would be to update the river bed elevation.

It is true that this sentence was confusing. This part has been rewritten.

m#16 – P17, l.14-15: This will help only if width, bank slope etc are also correct or corrected

You are right. Following previous reviewers' comments, this point was covered elsewhere in the Discussions and the current sentence does not appear anymore in the manuscript.

Additionnal references

Sylvain Biancamaria, Paul Bates, Aaron Boone, Nelly Mognard. Large-scale coupled hydrologic andhydraulic modelling of the Ob river in Siberia. Journal of Hydrology, Elsevier, 2009, 379 (1-2), pp.136-150. doi 10.1016/j.jhydrol.2009.09.054

Beighley, R. E., Eggert, K. G., Dunne, T., He, Y., Gummadi, V.,and Verdin, K. L.: Simulating hydrologic and hydraulic processes throughout the Amazon River Basin, Hydrol. Process., 23, 1221–1235, doi:10.1002/hyp.7252, 2009