

Interactive comment on “Assimilating SAR-derived water level data into a hydraulic model: a case study” by L. Giustarini et al.

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RESPONSE TO THE EDITOR

Firstly, we would like to thank Dr. Albrecht Weerts for the assessment of our study and also for his comments and corrections. We have incorporated the changes suggested by the Editor and the reviewers into our revised manuscript.

Editor:

All three reviewers have also some points of criticism “some assumptions should be better described and supported” (ref. Di Baldassarre), “some more discussion of chosen error distributions for the measurement uncertainty and specifically the impact of

any assumptions on the results” (ref. Neal) is necessary, “some minor corrections are required” (ref. Yamazaki).

Authors:

Concerning the points of criticism from Di Baldassarre, a more detailed and explicit description of the calibration process has been added to the revised manuscript. In particular, more details have been given about the hypothesis of no significant changes in riverbed geometry and the role played by the floodplains. Moreover, the calibration approach has been better described, explaining the reasons to carry out an optimization of 4 Manning’s values for the main channel. Neal’s comment about “some more discussion of chosen error distributions for the measurement uncertainty and specifically the impact of any assumptions on the results” has been addressed, adding some sentences to better clarify all uncertainties and/or assumptions affecting the hydrologic and the hydraulic modeling. In the answer to the specific comment on the chosen distribution a more detailed explanation of the performed tests with different distribution has been given. All the corrections suggested by the referees have been taken into account in the revised manuscript.

Editor:

Regarding the “wider context” (ref. Neal) the paper could benefit from discussing “scaling issues” especially if the ultimate objective is “forecasting applications” (ref. Neal). The paper could benefit from a discussion on operational aspects (also given the theme of the Special Issue) this could include a discussion on satellite measurement vs. ground based water level measurements used in state updating for operational hydraulic river or storm surge forecasting.

Authors:

On the topic of “scaling issues” especially if the ultimate objective is “forecasting applications” (ref. Neal), a discussion has been added in the revised manuscript. In fact,

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as suggested by more than one reviewer, we also expect the proposed Particle Filter assimilation scheme to perform better in longer river reaches with a larger upstream basin, where the effect of the inflow (and its correction) is less evident, due to the higher time correlation of water stages. The discussion on satellite measurement vs. ground based water level measurements used in a data assimilation scheme has been addressed in the response to Neal's review. However, as the topic of this research was to address the possibility of using satellite data when dealing with ungauged rivers, in the revised manuscript the results deriving from the assimilation of gauged quasi-continuous time series of water level have not been included in the paper.

REPLY TO G. DI BALDASSARRE (Referee)

First we would like to thank G. Di Baldassarre for his helpful review and for his recommendations and suggestions.

In the following we will address the specific comments outlined in the review.

Referee G. Di Baldassarre:

1. To calibrate the model, Giustarini et al. use contemporaneous measurements of water levels and discharge. In principle, I think that this can be a good calibration strategy. However, these measurements were performed in the period 2001-2009 and I presume that the geometry of the river has changed over this period. Thus, this calibration data is expected to be affected by significant noise because of changes in the river geometry and, in particular, of the cease-to-flow level. This is evident by observing the two measurements in Pfaffenthal (Fig. 7) at hydrometric levels around 60cm when the measured discharge decreases for a higher hydrometric level. Given that the calibration exercise focused on the highest flow measurements, this noise might be negligible, provided that for river discharge values sufficiently higher than the bankfull discharge, differences in water stage due to changes of river geometry actually tend to vanish. This is experienced in many alluvial rivers where changes in the river geometry mainly occur in the main channel and therefore do not have a strong effect

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on the flood hydraulics when the floodplain gives a relevant contribution to the flow (Di Baldassarre and Claps, 2011). I wonder whether this is actually case of this reach of the Alzette River. Anyhow, this issue should be discussed.

Authors:

1. The period to which the measurements of water level and discharge refer is rather long (1996-2010) and indeed during that time the geometry of the river might have changed. The geometry of the river described by the 144 channel cross sections refers to the year 2001. In this year topographic surveys were carried out and a LiDAR scan of the surrounding floodplain took place. It is important to note that the SAR-observed flooding event occurred in January 2003. All the cross sections with available measurements of simultaneous water level and discharge values were analyzed, comparing the cross section of the hydraulic model with the cross sections observed during each discharge measurement campaign. No significant differences were found, arguably due to the fact that the considered cross sections are located at bridges where the bed is generally stabilized. There is no evidence that points towards significant changes in riverbed geometry. Hence we adopted in this case study the assumption of temporally stable river geometry. With respect to this point a discussion will be added to the revised version of the manuscript. Concerning the comment on floodplain contribution, for the Alzette River and for the considered flood event, previous studies have shown that the floodplain does not play a significant role in the flood hydraulics (Hostache et al., 2009; Montanari et al., 2009)

Referee G. Di Baldassarre:

2. It is not entirely clear how the calibration was performed as the paper only states that "the calibration aimed to reproduce the highest measurements of discharge". Anyhow, by considering the measurements in Fig. 7, I guess that the number of data used for model calibration is very limited (4, 8 or 12 points?). Thus, I think that using 4 parameters with such a low number of calibration data (4 different Manning's coef-

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ficients specifically adjusted in each cross section to fit a few points) might not be a parsimonious approach. In other words, I feel that this parameterization might lead to overfitting. More specifically, it is well known that a unparsimonious model tends to capture relatively much of the idiosyncratic information in the calibration data (i.e. noise; Wagenmakers, 2003; see also above comment). In fact, by simply adding parameters it is possible to fit almost everything (Fig. 7). However, such a model might make poor predictions as its parameter estimates tend to be affected by a relatively high uncertainty (Burnham and Anderson, 2002). I wonder whether the use a single Manning coefficient for the main channel would not be more appropriate for this modelling exercise.

Authors:

2. In the considered case study, we first tested the assimilation of water levels into a model with the same Manning's values as in Montanari et al. (2009): one value for the channel and one for the floodplain. However, afterwards we decided to make the best possible use of the available data to reduce potential sources of errors originating from the model: structure errors, like 1D flow approximation and errors in the geometry, and parameter errors, like Manning's roughness value. We agree that our calibration approach with 4 different Manning values could suffer from equifinality but we considered it as an efficient way to reach a better local fit between simulation results and stream gauge measurements. As a matter of fact, an effort will be done to better clarify and support the calibration procedure in the new version. This point is also addressed in the answer to D. Yamazaki's review.

Referee G. Di Baldassarre:

3. The floodplain Manning's coefficient is taken from Montanari et al. (2009). Nevertheless, as far as I can understand by reading the two papers, that calibration exercise was completely different from this one. In particular, that value of the floodplain Manning's coefficient was associated to a different channel Manning's coefficients. Now, it

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should be considered that many studies in the scientific literature have shown examples of parameter compensation: decreasing floodplain Manning's coefficients can be compensated by increasing channel Manning's coefficients (e.g. Hunter et al., 2006; Di Baldassarre et al., 2009). Thus, the use of the floodplain Manning's coefficient derived by Montanari et al. (2009) should be critically discussed and commented.

Authors:

3. As already mentioned before, for this specific flood event the contribution of the floodplain is not particularly significant. Therefore, the use of the Manning's coefficient derived by Montanari et al. (2009) does not particularly influence the results. A discussion on this topic will be developed in the resubmitted version.

Referee G. Di Baldassarre:

4. In my opinion, the example application (Alzette River) is functional to facilitate the description of the proposed procedure. However, given the current existing availability of SAR data and the actual resolution and revisit time, I feel that such a procedure is presently more suitable for larger rivers. Also, it might be worth noting that Schumann et al. (2010) recently demonstrated that globally and freely available space-borne data sets (SRTM, ENVISAT ASAR in WSM) can be used to approximate flood levels on large rivers. This potentially might allow such technology to be extended to data scarce areas and developing countries. Hence, I think that the paper would benefit by exploring and/or discussing the possibility to extent the method to larger rivers where globally and freely available remote sensing data can be used to derive water levels.

Authors:

4. Thanks for the idea to extend the application to larger rivers and to test the applicability of the assimilation scheme to other case studies. In particular, we share G. Di Baldassarre's opinion that the proposed assimilation scheme has the highest potential for model improvements in large river systems that are poorly gauged. This idea will be

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mentioned in the new version, including a reference to Schumann et al. (2010). Hence it will be interesting to test the approach with freely and globally available space-borne data in order to check the performance of the proposed assimilation method in different regions and larger basins. In particular, we hypothesize that in larger river systems the dominating effect of the boundary condition is reduced and this would indeed favor more persistent model improvements through data assimilation procedures.

Referee G. Di Baldassarre:

5. The abstract (page 2104, lines 4-5) and the introduction (page 2105, lines 4-6) mention that SAR-data "can be used for updating hydraulic models in near-real time". However, the paper does not include references to previous works (e.g. Di Baldassarre et al., 2009) nor clear evidence of this statement. I feel that a reference is also needed when LISFLOOD-FP is mentioned (e.g. Bates and De Roo, 2000).

Authors:

5. Thanks for the suggestion about the references, which will be added to the introduction to make it more complete.

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REPLY TO D. YAMAZAKI (Referee)

First we would like to thank D. Yamazaki for his interest in our paper and for the comments, which will allow to improve the original version of the manuscript.

The reply to his comments can be found in the following.

Referee D. Yamazaki:

1. P.2105 L.14 "with a reliable observation uncertainty of 50 cm": The uncertainty of the SWOT observation is better to be mentioned with a spatial resolution. The uncertainty of 50 cm is the value for one pixel with its size of 50 m by 50 m, and the uncertainty of water surface elevation decreases when it is averaged for larger area [Lee et al., 2010].

Authors:

1. Thanks for the suggestion on how to better describe the expected SWOT data uncertainty: it will be corrected in the final version of the manuscript.

Referee D. Yamazaki:

2. The performance of the hydraulic model should be checked before the data assimilation experiments. It seems from Figure 5 that the observed discharge at the upstream boundary is available. Then using the observed discharge, it is better to show whether HEC-RES is able to reproduce reasonable spatio-temporal variations of water surface elevation when the realistic upstream boundary discharge is given as input.

Authors:

2. The observed discharge at the upstream boundary condition is available for the event of January 2003, through the application of a calibrated rating curve on the recorded water levels in Pfaffenthal. Considering the observed discharge as input, the performance of the model was assessed comparing the observed and the simulated hydrographs at all the gauged cross sections, thereby checking the capability of the calibrated model to reproduce the spatio-temporal variations of water levels. The Nash-Sutcliffe efficiency was computed at all gauged cross sections with recorded water stages and available rating curves, obtaining an average value of 0.84. This will be added and better explained in the resubmitted version of the manuscript.

Referee D. Yamazaki:

3. P.2118: The procedure of calibration is not clearly explained. - Which upstream boundary discharge was used as the input to HEC-RAS? - Was the roughness pa-

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parameter perturbed independently for each cross section within a single model run for calibration (i.e. in this case the model run should be repeated $31 \times 31 \times 31 \times 31$ times if the roughness coefficient is perturbed with the interval of 0.001 for the 4 cross sections)? Or, was the best parameter for each cross section derived from an independent model run (i.e. in this case the model run should be repeated 31 times)? The second approach is not strict because the stage-discharge relationship can be affected not only from the local water stage but also from the hydrodynamics within the surrounding reach (i.e. backwater effect should be considered). Given that the river bed slope of the study area is not so steep (approximately 70cm / 1km from Figure 3) for neglecting the backwater effect and the full-form of momentum equation with the backwater effect is used in HEC-RAS, I think the first approach should be taken.

Authors:

3. During the calibration the inflow discharge observed during the January 2003 storm event was considered as input data. The calibration approach was performed using multiple randomly generated roughness parameter sets. Each parameter set has 4 values for the channel roughness at the 4 gauged stations of Pfaffenthal, Steinsel, Hunsdorf and Lintgen (between the gauging stations, parameters are estimated through linear interpolation) and 1 value for the floodplain, as its contribution is assumed not to be relevant (see also Hostache et al. (2009), Montanari et al. (2009) for more details on the non significant floodplain roughness parameter sensitivity in the study area). The model was evaluated comparing the observed rating curves (points of contemporaneous measurements of water level and discharge) at the 4 cross sections with the internal rating curves of the model itself. The selected model set is the one with the best performance in reproducing the observed water level and discharge values. An effort will be done to better describe and support the calibration procedure in the new version.

Referee D. Yamazaki:

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4. P.2121 L.7 "The poor quality of the model results at this cross section could thus be explained with a badly calibrated model": The authors explain that the poor quality at Walferdange is caused by the model's uncertainty due to the insufficient parameter calibration, but another explanation can be made. I suppose it take a few hours for a floodwave to be transferred within the 19 km reach of the Alzette River. Given that the SAR observations are made near the inflow peaks (as shown in Figure 5), the error in the timing of peak upstream boundary discharge can also cause the situation that "the model results are good for some cross section but at the same time bad for other cross-sections". This possibility due to the error in inflow peak timing should also be discussed.

Authors:

4. The possibility of having poor model performances at a local level due to errors in the timing of the inflow peak is an interesting point of investigation. Based on the results obtained with some additional analyses, we will try to address this point in the final version of the manuscript, also taking into account the comment from D. Yamazaki in point 5 of his review.

Referee D. Yamazaki:

5. P.2111 L.1 "Model parameters, forcings and initial conditions of the hydrologic model were perturbed in such a way that the ensemble mean differs from the observation by a value that is equal to the time average of the ensemble spread (De Lannoy et al., 2006)." I do not think this assumption for ensemble spread is sufficient because, as discussed in the comment above, the timing of peak inflow also determines the spatial distribution of water surface elevation when SAR observations are made. It seems from Figure 5 that the timing of peak inflow is same for most of the ensemble members. The spread of discharge within a single time step is of course important, but I think the timing of peak inflow (i.e. response time between rainfall and runoff) should also has some spread due to the hydrological model's uncertainties. Some descriptions on this

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point are better to be included at least in a discussion part.

Authors:

5. We understand the suggestion of taking into account not only the spread of discharge within a single time step but also some differences in inflow peak timing. We will further investigate this and discuss it in the new version.

Authors:

All the technical corrections will be taken into account and added to the final version.

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Hostache, R., Matgen, P., Schumann, G., Puech, C., Hoffmann, L., and Pfister, L.: Water level estimation and reduction of hydraulic uncertainties using satellite SAR images of floods, *IEEE Transactions on Geoscience and Remote Sensing*, 47, 431-441, 2009.

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REPLY TO J. NEAL (Referee)

First the authors would like to thank J. Neal for his questions and comments, which enable us to improve the original version of the manuscript.

In the following we will address the specific comments outlined in the review.

Referee J. Neal:

P2104, L15: "significant reduction in the model forecast uncertainty", do you mean
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forecast here or that the uncertainty is significantly reduced at the analysis time?

Authors:

We refer to the reduction of uncertainty at the analysis time, which could also help to reduce the uncertainty in the forecast step. This will be clarified in the revised version of the manuscript.

Referee J. Neal:

L21-22: Might be worth saying why SAR is regarded as the most promising technology.

Authors:

Thank you for the comment: a sentence will be added in the revised manuscript to explain the utility of SAR technology for monitoring floods.

Referee J. Neal:

P2105 L7: You could be more specific here and refer to shorelines instead of inundated areas because this is the critical location for water level extraction.

Authors:

Thank you for the suggestion to use shorelines instead of inundated areas.

Referee J. Neal:

P17: add "as" after "However,"

Authors:

We will add "as" after "However", as suggested.

Referee J. Neal:

P24-27: Does this conclusion apply to the newer high resolution SAR's as well as ASAR or should this be instrument/resolution/polarization specific?

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Authors:

We refer to the work of Schumann et al. (2008) that investigated the different sources of uncertainties in the flood extraction algorithm from a high resolution ENVISAT ASAR image. Concerning other SAR products, with higher or lower resolution, we can only speculate on the relative importance of potential sources of uncertainty. We may only hypothesize that with very high-resolution satellite images the geo-location error can be reduced, as it should be easier to correctly georeference a more detailed image. Errors due to speckle are expected to increase in higher resolution images. In the revised version of the manuscript we will clarify that we are referring to a high resolution ENVISAT ASAR image.

Referee J. Neal:

P28-29: " In a data fusion..." I don't understand this sentence so it might need rewording?

Authors:

The sentence will be reworded in order to render it more self-explicative.

Referee J. Neal:

P2107: It might be worth saying why Neal et al. (2009) chose not to assimilate all the data because the reason relates to a data quality issues with remotely sensed derived water levels, rather than not wanting to use all the information available. Essentially they suggest a quality control step prior to assimilation is needed because some locations will obviously produce biased data (e.g. shorelines next to steep slopes and tall vegetation).

Authors:

Thank you for the suggestion. A sentence will be added in the new version of the manuscript to explain the reasons why Neal et al. (2009) used a quality control step in

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order to select a subset of data to be assimilated.

Referee J. Neal:

P2108 L16: do you need to say "actual"?

Authors:

We agree here and will remove the word actual.

Referee J. Neal:

P2110 L5: What happens in the case where an upper bound is under estimated at an upstream point... are all subsequent level distributions biased low until the upper level drops below the incorrect level or is there some procedure for spotting outliers. Further the uncertainty in the observations may also be underestimated which leads to too many particles being given zero/low weights. I'm worried that rather than removing poor quality data this approach has the potential to do the opposite and it would be better to use the data assimilation to filter the less certain observations rather than use a rule based system, especially as your results seem to indicate there is not enough uncertainty in the observations when assimilated globally?

Authors:

We agree with the reviewer about the fact that if the upper maximum (or lower minimum) is wrongly estimated, errors will propagate through all other cross sections. However, considering the efforts that were made to take account of the different sources of uncertainty in the water level estimation procedure it is reasonable to assume that the true water level is always included inside every interval of remote-sensing derived water level. We believe that the risk of under- or over-estimating the maximum and minimum water stages has been minimized. This hypothesis is moreover supported by the fact that all ground-surveyed measurements of water levels are included inside the above-mentioned intervals for both available satellite images (more details about this validation can be found in Hostache et al. (2009)). This will be highlighted and

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discussed in the revised article.

Referee J. Neal:

P2111: I think you mention it later but as your discussing the uncertainty and ensemble generation it would be good to state your assumptions about hydraulic model structural/parameter errors and the likely magnitude of these relative to other errors.

Authors:

We agree with the reviewer's suggestion to mention all uncertainties and/or assumptions affecting the hydrologic and the hydraulic modeling.

Referee J. Neal:

P2112 L8: A minor point but the EnKF doesn't necessarily give a Gaussian output... its an ensemble method. Rather the covariance matrix is assumed Gaussian.

Authors:

Thanks for the clarification. In the revised manuscript the sentence will be reformulated. Both the EnKF and the Particle Filter aim to approximate the posterior pdf by a set of random samples. In the EnKF the posterior pdf and the likelihood pdf are considered to be Gaussian, hence these pdfs are parameterized by the mean and the covariance, and the Monte Carlo approach is used to approximate the error covariance by the sample covariance. On the other end, in the Particle Filter the representation of the posterior pdf does not require a parameterization of the pdf. The latter implies the relaxation from the assumption of Gaussianity, allowing extending the Particle Filter to non-linear and non-Gaussian applications.

Referee J. Neal:

P2113 L2-4: Here I think it would be good to demonstrate that the uniform distribution is a better fit to the empirical data than say a normal or log normal distribution. I'd imagine there is some simple statistical test you could use for this. You argue quite correctly

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that the normal distribution makes assumptions that the data validates but presumably this doesn't mean its the worst distribution you could use or that the uniform distribution is better?

Authors:

Some statistical tests (Kolmogorov-Smirnov test, Lilliefors test, Jarque-Bera test, . . .) have been used to test the hypothesis of water levels having a normal standard distribution, before applying the hydraulic coherence concept. For the majority of the cross sections, we concluded that the distribution of water levels could not be approximated by a normal distribution function. After the hydraulic coherence concept was applied, we decided to use the uniform distribution, because only the maximum and minimum water level values are available for each cross section. In this case we assume that all water levels are equally likely.

We tested also another sort of distribution, assigning a zero weight to the extreme (maximum and minimum) retrieved water levels) and a maximum weight to the simulations whose predicted water levels was in the middle of the extremes (see Fig. 1). We obtained slightly better results with this last type of pdf. However, there is no available data at the moment that would help us to prove that the choice of this pdf is better than the use of a uniform one. Clearly, this would need to be tested with additional data sets. Therefore, the use of the uniform distribution seems preferable, as it does not identify some water level observations to be more likely than others. Some more comments and explanations will be added in the revised version of the manuscript.

Referee J. Neal:

L17-18: proposed SWOT data are very different to gauge data, so you might want to clarify that you don't expect the errors in these data to be the same as gauge data.

Authors:

We agree with the reviewer and the sentence will be removed in the revised manuscript

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to avoid confusion and also because the sentence itself is not relevant in the context.

Referee J. Neal:

P2115 L24: Could you state the expected impact of this rapid spread on forecast lead times. Given the reach length and a wave speed of something like 0.5-1 ms⁻¹ it will only take 15-30 mins for inflows at the top of the reach to arrive at the downstream boundary. This means the majority of any forecast and the uncertainty in the forecast will depend on the boundary update. I don't think this detracts from the main novelty of the paper (e.g. the application of the particle filter assimilation scheme with real data) but it does mean the example here is a far better estimator of river state at the time of the overpass than forecaster of future state. Explaining this and maybe even suggesting potential improvements may be a way of describing some of the future development that could improve the scheme implemented here.

Authors:

We share the reviewer's opinion about the predominant effect of the upstream boundary condition on the forecast. Given the reach length (19 km) and assuming a value for the celerity of 1 m.s⁻¹, the flood peak at the top of the reach needs several hours (5-6 hours) to propagate through the reach. Therefore, the quasi-immediate diversion of the ensemble spread is more evident at the upstream end, while near the downstream end it takes more time for the ensemble members to spread. For this reason we expect the proposed Particle Filter assimilation scheme to perform better in longer river reaches with a larger upstream basin, where the effect of the inflow (and its correction) is less evident, due to the higher time correlation of water stages. This aspect will be explained in more details in the new version of the manuscript.

Referee J. Neal:

P2117 L13: Is there a risk that narrowing the uncertainty in the water level distribution has caused too many particles to be rejected?

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Authors:

The risk of narrowing the uncertainty so that all the particles are rejected is indeed present. The way to deal with this issue is explained in the following point.

Referee J. Neal:

P2120 L29: If I understand correctly, the use of a uniform distribution with equation 3 means that if a particle is outside the uniform distribution at any location it is assigned a weight of zero, and that this may explain why more particles are retained when assimilating the more accurate ground data. Would it be possible to add this behavior and its implications to the discussion of the global results on the next two pages. In particular is desirable to have such a strict accept reject criteria for a global analysis given the limitation in the model and data you outline, where poor prediction at a single point can lead to a zero weight? It might also be worth integrating this point with conclusion 3 should you agree with it.

Authors:

The strict accept / reject criterion given by Eq. (3) was first developed for the synthetic experiment in Matgen et al. (2010). Bearing in mind that this paper was a proof of concept dealing with synthetic satellite observations, that were assumed to be normally distributed, it never happened that the weight of a particle became zero. However, when dealing with real observations, that are supposed to be uniformly distributed between a maximum and minimum value, it can indeed happen that with the global weighting procedure a particle falling outside the interval of possible water stage values at a single location will be attributed a weight of zero. This was done on purpose, as we consider as not acceptable (i.e. weight zero) a model that is not able to produce water stages that fall inside this (rather large) interval. However, different global weighting criteria could be applied and tested: instead of a multiplication of the local weights, a mean value of them could be used to compute the global weight for the whole water surface line. This adaptation is required when dealing with very accurate

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measurements, as our assimilation procedure would presumably lead to the rejection of all particles.

Referee J. Neal:

P2127 L1: Am I correct in thinking the time series data from the gauge was not assimilated as that this could be done in theory?

Authors:

In situ observed data were assimilated only at the 2 time steps of the satellite overpasses assuming a Gaussian probability distribution of the measured water levels. Our analysis can be considered as a benchmark test that enables contrasting the performances obtained when assimilating, respectively, very precise but poorly distributed ground-surveyed information and spatially distributed but highly uncertain satellite data. As suggested by the reviewer, the assimilation of gauged quasi-continuous time series of water level is feasible and yields the best overall results. This result is not surprising as it corresponds to a typical operational application in a well-gauged river network. However, it was not the topic of this research to find out if remote sensing-derived water elevation data can provide model improvements in well-gauged areas in an operational context. For this reason these results have not been included in the paper.

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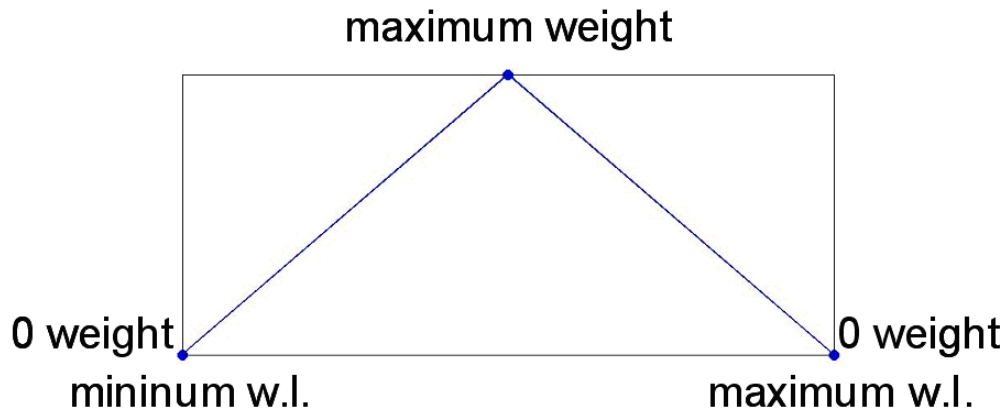


Fig. 1. Example of tested triangular pdf to assign weights in the analysis step.

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