Review1

Most of the comments from review 1 were technical comments and were corrected accordingly in the text. You can find below the detailed list of the corrections that were made and the answers to a specific question about the recession law. The reviewer's comments are in green to differentiate them from the authors response.

Page 4480, line 16, the reference (Papa et al., 2012). There is confusion in the year of publication. In the reference list, page 4508, lines 18-20, the year indicated is 2010. Please correct to year 2010.

This was corrected in the text, p.4 l.97.

* ECMWF page 4481, line 12 appears for the first time in the text. So abbreviation has to be defined.

This was added to the text, p.3 l.129.

* Page 4482, line 4, replace "he" by "they". Same page line 12, "qmodeling". I think q has to be deleted. Same page line 22, the sentence: "The domain study area.....
"Make it two sentences, and avoid too many use of the word "which".

The sentence war rearranged and split in 2 parts as asked and can be find on p. 4 from line 186 to 191:
The domain study area is the trans-boundary Niger basin
(Fig. 1) which crosses a large part of the Sahel and is a

critical source of water in this semi arid region. The West African region is also characterized by an increasing popu-

lation, putting larger pressure on the already limited fresh-water resources.

* Page 4483, line 9. The sentence "The confined aquifer improves the recession law ..." What is the recession law? Not clear.

The recession law is the function describing the curve of the decreasing flow during the dry season. This was precised in the text on p.4 l.214.

* Page 4484, line 20, "the Basin has ...." Change "Capital B to small b".

This was corrected in the text.

* Page 4489, line 11, sentence "First, a realistic modeling of the studied basin in needed ..."

Replace "in "by "is".

This was corrected in the text.

* Page 449, line 3, sentence "....the satellite starts its first orbit on 1 January 2002)". Change "starts to "started".

This was corrected in the text.

* Page 4503, line 18, the word "scenarii" before the sentence hat starts by "Thus, the estimation of ....". It is not clear what it means.

The sentence was re arranged and the word 'thus' was removed. Here, the word 'scenarii' refers to different data assimilation configurations in which different sources of uncertainty are considered. The use of an ensemble DA methodology could allow to take into account these different sources of uncertainty. p. 11 l. 971 :
“It could be potentially
interesting to perform the assimilation on an ensemble of perturbed runs in order to take into account several uncertainty
scenarii and the estimation of the background modeling matrix could be done using an ensemble method (Evensen et
al., 2004)
”

* Page 4520, Fig.8. The legend is not clear for the 1 day and 3 day subcycles. It says "straight line" for the 1 day subcycle and "dashed line" for the 3 day subsycle. But there are no dashed or straight lines, but colored liCones. Provide also title for the vertical axis.

The legend was corrected according to the review's comment (see Fig.8) The x axis represents the number of assimilation cycle and the y-axis is relative error as expressed in the legend.

* Page 4530, Fig.18. Provide title for the axes.

Fig. 18 is now Fig. 19. The x-axis is the time in days and the y-axis represents the relative storage in the different reservoirs.

Review2

The abstract was shortened accordingly to the reviewer's comment. Also, Fig. 9 was added to show the spatial distribution of the Manning coeffifient before and after the assimilation. You can find below the detailed list of the corrections that were made and the answers to a specific question about the recession law. The reviewer's comments are in green to differentiate them from the authors response.

> The abstract is too long and has many unnecessary information. For example, I think the following parts can be removed:
- which are typically employed in Land Surface Models (LSM) for global scale applications.
- a trans-boundary river, which is the main source of fresh water for all the riparian countries. In addition, geopolitical issues in this region can restrict the exchange of hydrological data, so that SWOT should help improve this situation by making hydrological data freely available. In a previous study, the model was ïnˇA¸rst evaluated against in-situ and satellite derived data sets within the framework of the international African Monsoon Multi-disciplinary Analysis (AMMA) project.

The abstract was shortened accordingly to the reviewer's comments.

> The word “indeed” appears repeatedly (16 times in the manuscript), and sometimes it is not necessary. Please remove the word when not needed.

This was corrected in the text.

P4478.L15: “which might have locally significant errors.”
The term “locally significant errors” is ambiguous. It’s better to say “significant errors at local scale”.
Furthermore, the scale indicated by “local” may be different for different researchers. Please clarify what the “local” mean in this study. It can be read as “basin-scale” or “reach-scale”.

This was corrected in the text, p.2 l. 18 : “which leads to significant errors at reach and large scale.
”.

P4479.L8 & Section 5.4: Continental reservoir

It's not clear what the "continental reservoir" means. It might be misunderstood as large lakes or any kind of large water bodies.

This was corrected in the text : "... shows skill in simulating the maxima and minima of water storage anomalies, especially in the groundwater and the aquifer reservoirs." P.11 l.925.

P4479.L9: "Results obtained in this preliminary study demonstrate SWOT potential for global hydrologic modeling, especially to improve model parameters."
> This statement is generally true, but I think it's too optimistic given that this manuscript only performs Observing System Simulation Experiment. It's better to say that further studies (e.g. considering multiple error sources and difference between synthetic and real observations) are obviously needed to achieve the SWOT's application stated in the abstract.

This was changed to "The application of the assimilation method in the framework of an Observing System Simulation Experiment allows to evaluate the skill of the EKF alogorithm to improve hydrological model parameters and demonstrate SWOT promising potential for global hydrology issues. However, further studies (e.g. considering multiple error sources and difference between synthetic and real observations) are needed to achieve the evaluation of the method." P.2 l.41.

P4480.L8: "1-dimensional"
What the 1-dimensional means here? Is it 1-spatial-dimension along river stream, or a point data with a time-series dimension?

1-dimensional means that the discharge gives a 1-spatial-dimension information in the direction of the river stream flow.

P4480.L9: "but such data do not give any information about runoff and lateral inflow."
> It's better to say "any DIRECT information" because we can guess the amount of runoff or lateral inflow from gauged discharge.

This was corrected in the text on p.3 l.88.
Indeed, the total runoff can be guessed from gauged discharge but it seems difficult to differentiate every component of it only from this 1d observation.

P4480.L19: "current remote sensing technology spatial resolution does not resolve small scale land water dynamics"
> It's better to say "current satellite altimetry" because high-resolution observations of water area are already available (i.e. LANDSAT, Synthetic Aperture Radars, etc).

This was corrected in the text on p.3 l.102.

4480.L29: "Several studies are thus currently being performed over geographically

diverse basins”
> Please put references for the “several studies”.

This sentence was removed because it did not add any relevant information according to the author.

P4481.L26 “the Brahmaputra river” and related parts
> Please use large capital for an individual river name (i.e. the Brahmaputra River, the Niger River).
P4482.L12 qmodelling.
> Please correct the typo.

These were orrected in the text.

P4483.L15: might not give the best results locally (for a particular basin).
> This is true, but some studies showed that empirical equation does not work well even within one basin and significant error can be found at sub-basin scale or reach scale (e.g. Miller et al., 2014, Yamazaki et al., 2014).

This comment is useful and was added to the text on p.4 l.225.

P4483.L29: “the reason will be explained”
> Please clarify in which section “the reasons are explained”.

The reasons are explained in “Section 4.1”. This was clarified in the text on p.4 l.249.

> It is stated in the previous sentence that the modelling complexity comes from different climate zones existing in the Niger Basin, therefore I think it’s better to write that infiltration and evaporation from floodplain is also very important in addition to rainfall for modelling the Niger River.

This was corrected in the text on p.5 l.285.

P4485.L18: a saturated fraction “fsat”
P4485.L26: a simple groundwater reservoir “G (kg)”
P4486.L3: a prognostic flood reservoir, F (kg),
> If the symbols for variables (e.g. fsat and G, F) are not used in other parts of the manuscript, these symbols don’t have to be shown. Same correction may be needed for other variables.

These technical issues were corrected.

P4486.L2: TRIP RIM
> Does this mean “TRIP RRM”?

Yes, this was corrected in the text on p. 5 l.329.

P4486.L13: The TRIP schematic concept is presented on Fig. 2 and more details can befound in Pedinotti et al. (2012).
> It's better to move this sentence at the start of the explanation of TRIM RRM in ISBA. Readers may understand the model easier by reading sentences with the Provided Figure.

The sentence was moved to the begining of the explanation of TRIP RRM (p.5 l.320).

P4487.L25: Remote sensing opens the possibility of estimating the river width by direct measurements and the critical bankfull height by indirect algorithms.
> Better to provide references such as [Pavelski and Smith, 2008; Yamazaki et al., 2014; Durand et al., 2010].

These references were added on p.6 l.394 .

P4488.L23: wider than 100 m (requirement)
> Better to say "(mission requirement)" instead of "(requirement)" for non-expert.
P4489.L11: in needed
> Should it be "is needed"?

Those technical comments were considered and corrections were made in the text.

P4490.L9: absolute water level
It's difficult to guess the difference between "absolute water level" and "free surface water elevation". May be it's better to say "water depth".

This was corrected in the text on p.7 l.487.

P4490.L9: Thus, in real-time DA applications, the direct comparison between SWOT and ISBA-TRIP water levels will not be straightforward and will need further investigation.>
Please make a discussion about the impact of this assumption. It can be a limitation of applying the method developed in this study to a real-observation case.

For DA applications in real conditions, the direct comparison between SWOT and ISBA-TRIP water
levels will not be straightforward and will need further investigation. Indeed, the SWOT satellite measures free surface water elevation which can not be directly compared to the ISBA-TRIP outputs which are stream water absolute depths in the river channel.
The assimilation then requires to find a way to compare these two different variables in order to perform the DA. For example, they can be compared in terms of anomalies relative to a mean value over a long period of time instead of absolute water elevations.
This method allows to remove the bias due to different reference values of the level where the water elevation is zero. However, in the framework of an OSSE, the same model is used to generate the apriori and observed water levels and this issue can be evaded.

P4491.L18: has a significant impact on the hydrological variables over the Niger basin.
> It's better to clarify "impact on SIMULATED hydrological variable" given the impact assessment on actual hydrodynamics is still difficult due to uncertainties in model physics and parameters.

This was corrected in the text.

P4492.L25: requires a bigger storage capacity
> It's better to say "disc (or memory, or computational) storage capacity". River model also has storage component, thus storage capacity might be misunderstood as model variable.

This was corrected in the text on p.8 l.595.

P4493.L11: a Gaussian distribution, centered in 0 with a standard deviation, _bt of 20% of the average value of the Manning coefficient over the river.
> I'm not sure whether this assumption for the initial prior Manning's coefficient is feasible. Because there is no ground-truth for Manning's value, the initial value may be totally different (can be biased globally and/or locally) in the real situation. Please at least discuss the possible impact of the initial Manning's value estimation, and if possible please do additional experiment to check its impact on assimilation results.

The EKF filter makes the asumption that the modeling error vector describes a gaussian distribution centered in 0 and this is why the modeling error is described this way in this study. However, the limitations of this asumption are described later and it is suggested to use a particle filter in order to consider more realistic modelling errors. The standard deviation of 20% derives from the sensitivity tests which were done in Pedinotti et al. (2012) and showing the impact of the Manning coefficient on the simulation. Moreover, Decharme et al. (2011) estimated that it was a resonnable range of uncertainty for this parameter.

P4493.L19: H = SoM
> What is "o" between S and M?

The "o" in algebra describes the combination of two functions. For any variable x, SoM(x) is equal to S(M(x)).

P4496.L5: the Manning coefficient relative error (averaged over the river)
> Is it reasonable to average relative errors over the basin? Is some point has positive relative error and another point has negative ones, they are cancelled out.

The expression of the relative error is wrong in the paper. The relative error is : |n-ntruth|/ntruth. This relative error, described as is, can not be negative. This was corrected in the text.

P4496.L25: a noise with a frequency of about 20 days
> This is obviously the signal from orbit cycle, thus it's strange to call it "noise". It's better to use another word (such as jump?)

This was corrected in the text p.9 l.746.

P4497.L12: reaching up to 9 m at Lokoja (for an 8 m averaged river depth).
> I cannot get this message. It seems two gauges are lacking in Figure 11.
Thank you for this remark, it seems that two figures disappeared while editing the article in the discussion version. The fact that is pointed here is that the correction of the water level due to DA can be considerable since it is higher than the mean river depth over the river.
P4499.L15: the model simulates ïn floodplains (25%)
> It's difficult to guess that the sentence means. Please say, for example, "flooding in 25% of the grid area".

This was corrected in the text on p.10 l.848.

P4501.L15: This study is promising since, to our knowledge, no large scale assimilation applications exist
> This statement is generally true, but please note that further studies on model physics and parameter retrieval are needed to apply the developed method to "real observation" further than OSSE.

The application of the developped method requires further investigation on the assimilation filter and a better representation of observation and modeling errors. However, it is not sure wether the model physics must be improved or not for its application with real data. Moreover, in real conditions DA methods could be used as indicators of missed crucial processes (mostly related to water levels) in the model. It is thus really difficult to make a pronostic about the level of physics that is needed for real conditions applications.

The retrieval of several parameters through single DA application could be possible assuming that the correlations between all the variables are well represented in the model and that all related main physical processes are considered by the model. Of course, further investigation is needed to confirm or not this asumption.

Figure 2 Caption:
> The figure shows "water flux calculation in TRIP RRM in ISBA", but not for "The TRIP model" itself.

This was corrected (see legend of figure 2).

Figure 4
> Can you also show the prior Manning's value before assimilation and posterior Manning's value as well as the true value?

Fig. 9 was added and shows the spatial distribution of the Manning coefficient for the truth, for the background and for the two simulations with assimilation. We see that the DA allows to retrieve the general patterns of the Mannin coefficient, especially for the extreme values of the background. Moreover, the values downstream of the river seem to be better corrected which can be expected due to the cumulated corrections upstream of the river.

Figure 7: Caption
> Please describe what the colored line (black and blue) represent.

This was corrected on Fig7.

Figure 11:
> Two gauges are missing.

This was due to a compilation problem with latex and was corrected in this new version.

Figure 16b
> Please clarify that the blue and red lines are not shown because flooded fraction is zero
The caption of Fig 16 was changed accordingly to this comment.

Figure 18 Caption
> Please use “floodplain” instead of “flood”. Flood is too ambiguous.
Corrected.

Tables 2 and 3.
> Please use the consistent effective digits.

Corrected.

[References]
Durand, M., E. Rodrigues, D. E. Alsdorf, and M. Trigg (2010), Estimating river depth from remote sensing swath interferometry measurements of river height, slope, and width, IEEE Geosci. Remote Sens. Lett., 3(1), 20–31, doi:10.1109/JSTARS.2009.2033453.
Miller, F. M., T. M. Pavelsky, and G. H. Allen (2014), Quantifying river form variations in the Mississippi Basin using remotely sensed imagery, Hydrol. Earth Syst. Sci. Discuss., 11, 3599–3636.
Pavelsky, T. M., and L. C. Smith (2008), RivWidth: A software tool for the calculation of river widths from remotely sensed imagery, IEEE Geo-sci. Remote Sens. Lett., 5(1), 70–73, doi:10.1109/lgrs.2007.908305.
Yamazaki, D., F. O ’ Loughlin, M. A. Trigg, Z. F. Miller, T. M. Pavelsky, and P. D. Bates (2014), Development of the global width database for large rivers, Water Resour. Res., 50, doi:10.1002/2013WR014664.

Review3

The comments from referee 3 are useful and mostly concern technical issues that were corrected according to the remarks. Some explanations were also required by referee 3 about the DA methodology and the authors focused on answering these questions as clearly as possible. You can find below the detailed list of the corrections that were made and the answers to a specific question about the recession law. The reviewer's comments are in green to differentiate them from the authors response.

-Abstract
p. 4478 l. 13-15: Please rephrase "might have locally signi?cant errors". The problems of parameter estimation occur across all scales and these errors rather "typically" occur.

Text corrected p.2 l.18.

p. 4478 l. 18: replace "so that" with "and

This line was removed as asked by referee 2.

p. 4479 l. 1: please replace "leads" by "led"

Corrected in the text.

p. 4479 l. 4: I would recommend removing: "despite potential problems related to equi?nality" as this is vague and no particular problem with equi?nality were identi?ed in the study.

This part was removed.

p. 4479 l.6-9: "prediction" see general comments

The word “prediction” was replaced by "simulation".

-Introduction

p. 4479 l. 14: "such" is not appropriate here since no impact studies are mentioned previously.

This was corrected in the text.

p. 4479 l. 15: I suggest you replace "over and under" by "above and below"

This was corrected in the text p.3 l.82.

p. 4479 l. 16-19: Please rephrase. This sentence is a bit confusing/long.

Replaced by "At regional or global scales, realistic representation of major surface hydrologic and hydrodynamic processes is very challenging and requires the use of computationally efficient, easily parameterized, comparatively simple and physically based routing methodologies". P. 2 l.57.

p. 4479 l. 20: insert "the" before "huge"

Corrected.

p. 4479 l. 23: replace "but also" by "and"

Corrected.

p. 4480 l. 10: Please simplify the sentence; the verb does not match the structure of the sentence. I suggest something like: "However, hydrologically complex areas such as wetlands or floodplains are better represented as three dimensional processes and cannot be adequately. . ."

Replaced by " Yet, more hydrologically complex areas, such as wetlands and floodplains are three-spatial-dimension processes, cannot be adequately resolved using one-spatial-dimension observation". P.3 l.88.

p. 4480 l. 12-17: The link between these sentences should be made clearer. Perhaps specify that you are writing about different types of surface water monitoring?

This pararagraph was rearranged (p.3 l.81).

" Hydrological models require information about continental water dynamics and storage variations above and below the surface for calibration and evaluation of the simulated water budget. To this end, diverse types of monitoring data are needed. In situ discharge data, for example, give 1-spatial-dimension information which quantifies water fluxes
in a specific river channel, but do not give any direct information about runoff or lateral inflow. Yet, hydrologically
complex areas, such as wetlands and floodplains which are three-spatial-dimension processes,

cannot be adequately resolved using one-spatial-dimension observations (Alsdorf et al., 2007). Spatially distributed observations are required, such as those provided by satellites which give 2-dimensional information about surface water dynamics. Recently, efforts have been made to build global maps of floodplains variability and extent, providing an additional metric for CHSs evaluation (Papa et al., 2010). Nadir altimetry has also constituted a valuable progress for the monitoring of surface water dynamics and elevation (TOPEX-POSEIDON, ENVISAT, JASON 1 and 2; Baup et al., 2007; Santos Da Silva et al., 2012). "

p. 4480 l. 25: Replace "concerning" by "For"

Corrected in the text.

p. 4480 l. 29: Remove "thus"

Corrected in the text.

p. 4481 l. 2: "geomorphologies"

Referee 2 suggested to remove those lines.

p. 4482 l. 8: replace "he" by "they"

Corrected in the text.

p. 4482 l. 12: typo "qmodeling"

Corrected.

p. 4482 l. 17: Input data such as precipitation should be included in the list as it tends to be one of the major sources of uncertainty.

The precipitation data were added to the list.

p. 4483 l. 5-7: Please specify what type of data was used to evaluate the model and whether the model was calibrated.

'The model parameters were estimated using geomorphologic relationships to characterize the river characteristics. The modelling evaluation showed that the model was able to reasonably reproduce the major hydrologic and hydrodynamic processes. The model outputs were compared to in-situ discharge as well as satellite derived flood extent, total continental water storage changes and river height changes.' (added in the text)

p. 4483 l. 10-11: Consider merging the two sentences.

Corrected.

p. 4483 l. 17: "spatially distribute" awkward, please rephrase

Replaced by "Such data can potentially be used to estimate spatial parameters..."

p. 4483 l. 18-20: The results sensitivity analysis would be relevant here, especially in order to justify the stated objective of improving Manning"s coef?cient in the next paragraph.

Added :" These tests have shown that the model was sensitive to modifications of some key river parameters (river height and depth as well as Manning coefficient) and that a good estimation of those parameters was required to optimize the simulation errors. "

-Study domain and model description
p. 4484 l. 24: replace "thus" with "and"

Corrected.

p. 4485 l. 23: missing "resolution" at the end of the sentence?

Corrected.

p. 4487 l. 9: replace "this" with "the ?"

Corrected.

p. 4487 l. 22: "could be considered" is not strong enough in my opinion, the other effects are very important and "should be considered" would be more appropriate

Corrected.

p. 4487 l. 25-27: please provide references.

References added :"Pavelski and Smith, 2008; Yamazaki et al., 2014; Durand et al., 2010"

-Satellite observations
p. 4489 l.5-8: I recommend removing "Indeed, here" and merging the two sentences: "It is assumed that the state of the system as well as the error statistics of the model and observations are known which will not be the case. . ."

Corrected.

p. 4489 l.8: remove "also"

Corrected.

p. 4489 l.9: reorder words: "since it allows for the quantification . . . modeling before launch"

Corrected.

p. 4489 l.11: typo: "in" -> "is"

Corrected

p. 4489 l.11: remove "indeed"

Corrected.

p. 4489 l.18-20: Missing verb. Perhaps "The . . . results are generated . . ."

The verb here is "to result from". The sentence explains that the background simulation is obtained from a different simulation in which the Manning coefficient was perturbed.

p. 4489 l.23: "and/by assimilating" rather than ", assimilating"

Corrected.

p. 4490 l.5: I think that ", in addition to" should simply be "and" if I understand the sentence correctly

Corrected.

p. 4490 l.9: The problem of water level/surface elevation/depth will be present for all applications using real data, not only real time. More discussion should be included on the impact of neglecting this.

For DA applications in real conditions, the direct comparison between SWOT and ISBA-TRIP water
levels will not be straightforward and will need further investigation. Indeed, the SWOT satellite measures free surface water elevation which can not be directly compared to the ISBA-TRIP outputs which are stream water absolute depths in the river channel.
The assimilation then requires to find a way to compare these two different variables in order to perform the DA. For example, they can be compared in terms of anomalies relative to a mean value over a long period of time instead of absolute water elevations.
This method allows to remove the bias due to different reference values of the level where the water elevation is zero. However, in the framework of an OSSE, the same model is used to generate the apriori and observed water levels and this issue can be evaded.
This discussion was added to the text.

p. 4490 l.12: Is this out of the scope of an OSSE or simply of this study?

The framework of the OSSE allows to evade this issue as direct comparison between apriori and observed WL can be done. The sentence was modified to make a clearer point.

p. 4490 l.20: "earth"s rotational speed"

Corrected.

p. 4491 l.7: repetition, replace with: "investigated within the DA framework"

Corrected.

-Data assimilation schemes
p. 4491 l.19: remove "indeed" and commas

Corrected.

p. 4491 l.25: remove "indeed". "Hydrodynamic models" cannot be the subject here, "modelers" could.

Corrected.

p. 4492 l.6: I think there are more problems than just a scale issue. How accurate is this linear relation to width? I think it is important to clearly acknowledge that the initial value of the roughness used would be very uncertain in a real case (especially for an uncalibrated model).

Replaced by : "These geomorphologic relationships are used to obtain the spatially distributed Manning coefficient which provides a 'global' fit or best estimate. However, the accuracy of these relations can be very uncertain due to the significant heterogeneity of the river and land properties, especially in uncalibrated models"

p. 4492 l.15-18: I do not understand the relevance of the variability of the correction of the coefficient in the context of this study: since the SWOT observations are generated using a constant "true" Manning, the assimilation-estimated Manning would have no reason to bene?t from this. What was the reason for the choice of the 2-day time window?

A longer assimilation window requires a larger computational and storage capacity than available for this study. As the principal scope of this work is to present and assess the DA methodology, it was decided to use a simple configuration with an assimilation window of two days. Since this study was performed, the computational capacity was improved and the impact of the window's length was investigated. A DA simulation was done with a 22 day assimilation window and showed a quicker and better convergence of the Manning coefficient. This will be

the subject of a future article which will also discuss the impact of considering other sources of model uncertainties and non gaussian errors.
In real conditions, it might be relevant to investigate the possible time variability of the Manning coefficient especially in areas where flooding events usually occur.

p. 4493 l.12: Is 20% error Manning"s realistic considering typical variations between streams?

The authors are not sure to understand the question but this 20% error is issued from the sensitivity tests performed in Pedinotti et al. (2012) and Decharme et al. (2011). According to these studies, 20% constitutes a realistic (or assumed) range of error on this parameter.

-Results
p. 4496 l.5: an absolute relative error should be used in order to carry out averaging.

This is correct and the absolute relative error is actually what was calculated to compute the average. The expression of the calculation is wrong. This was corrected in the text.

p. 4497 l.15: typo? "differs"

Corrected.

p. 4497 l.25: Is there any proof for the physical link between the width and the in?uence of the roughness? I can think of two other possibilities:
- the hypothesis of linear relation between width and roughness means that the 20% std will lead to a larger absolute error on the roughness for wider rivers
- it is not necessarily related to width as any errors (occurring or corrected) upstream will have an impact on the downstream portion of the river as well (and rivers just
happen to typically be wider downstream)

The sentence was replaced by :"The improvement is larger for stations that are located downstream of the river, possibly because of the cumulated corrections upstream of these stations. Moreover, the hypothesis of linear relation between width and roughness means that the 20% sandard deviation will lead to a larger absolute error on the roughness for wider rivers."

p. 4498 l.14: Please include some discussion of why the impact on flow is smaller than for levels. Shouldn"t this be expected since the Manning"s roughness is in fact updated through level measurements?

This is indeed expected since the Manning' roughness is updated through level measurements. The improvement of discharge can be seen as a secondary effect of the improvement of the Manning coefficient, although the discharge-Manning or discharge-level relationships are non linear.

p. 4498 l.15: I disagree that a "seasonal variability" is seen for the assimilation results, rather it appears that the open loop run is closer to the "truth" during the dry months
leaving little room for improvement (this is more or less what is then written on line 20 regarding sensitivity to Manning"s roughness, but the separation of this in 2 paragraphs is confusing)

The sentence about seasonal variability was removed.

p. 4498 l.19: It is unclear what you mean by "noisy", is this the same 20-day "noise" from the level observations?

Yes, it is the same noise than observed for water levels and this precision was added in the text.

p. 4499 l.13: Please rephrase to avoid using the word "results" 3 times

Corrected.

p. 4500 l.19: list what you mean by "continental reservoirs" here to avoid confusion.

Replaced by :"regional to large scale continental reservoirs including river, groundwater, aquifers and floodplains"

p. 4501 l.2: I agree that there is typically a lack of data for monitoring of these storages. However, I think it should be pointed out that the physical representativeness of the modeled values is not guaranteed, specifically because of the lack of monitoring data.

This comment is relevant and was added to the text.
The physical representativeness of the modeled values is not guaranteed, specifically because of the lack of monitoring data. Here, the values have simply been bounded to be within a reasonable range (based on rivers similar to the Niger and the scale of TRIP).

-Discussion

Considering that the assimilation corrects the Manning's number I recommend discussing the improvement on this parameter before the impact on levels.

Corrected.

p.4502 l.9: "degradation of the error estimates": please explain what you mean. Isn"t the discussion here about improving these estimates? The fact that they are no longer
Gaussian is not a degradation. Or do I misunderstand your meaning?

The "error estimates" refers to the relative errors that are calculated for the evaluation of the assimilation. The introduction of non gaussian observation errors in the assimilation method

would require the change of the assimilation filter since the EKF makes the hypothesis of gaussian errors. The sentence was changed to :"However, their introduction in the system is not obvious and the use of a different assimilation filter due to the aforementionned Gaussian issue."

p.4503 l.8 : what would be the impact of assimilation windows of different lengths?

As said previously, an ongoing study is investigating the impact of the assimilation window's length on the assimilation performance. Preliminary results showed that a longer assimilation window improves the performance of the model which is expected because for each correction, there is more information available about the water dynamics in space and time. This will be the subject of a future article.

p.4503 l.17-20 : Is this not a repetition of the discussion from p.4503? This paragraph would fit better after the discussion of the Gaussian observation error and other sources (f.ex. precipitation etc.).

Corrected

-Conclusions
I think the conclusion would benefit from having some information removed. For example it is unnecessary to repeat why Manning"s number was the chosen parameter (p4503 l. 24-p.4604 l.4)

The conclusion was shortened as advised by referee 3 :
'This study presents a simple method for assimilating SWOT virtual water level into a large-scale coupled land-surface hydrology model (TRIP-ISBA) in order to improve estimates of the required global hydrological model input parameters. In this case, the assimilation is used for the correction of a single parameter which is the Manning coefficient. To accomplish this, an Observing System Simulation Experiment (OSSE) was performed, using virtual SWOT observations of water levels. Two orbits, with different subcycles but with the same 22 days repeat period, have been considered to generate the observations (1-day and 3-day subcycles), each one providing a specific spatial and temporal coverage of the domain. Uncertainties on the estimation of the Manning coefficient are assumed to be the only sources of modeling errors. The Extended Kalman Filter (EKF) algorithm was applied every 2 days (the length of the assimilation window) to
compute an optimal Manning coefficient (analysis). The Manning coefficient globally converged
for both orbital subcycles to the same average value, the convergence being faster for the 3-day subcycle orbit.
The method leads to a global reduction of 40% of the Manning coefficient error over the river. This correction
significantly improved the water levels (the error has been reduced by 30% for the river) and, to a lesser extent, discharge (7% of reduction of the error which can be significant for the Niger river in terms of water resources considering that its mean annual discharge is

$6000m^{3}.s^{-1}$). Moreover, the biggest improvements were
observed downstream of the river (Lokoja), which is a valuable result for climate applications which require estimation of the discharge at large rivers mouths.

This method gives a promising perspective for global scale applications, and it could be extended to other large basins. However, several relatively simple hypotheses have been made, and these should be addressed and refined in future studies. The context of the OSSE allows the evaluation of the model but does not guarantee the physical representativeness of the corrected values obtained in this study. Moreover, other sources of uncertainties should be assumed for the assimilation, such as rainfall errors and/or river bankfull depth. Modeling errors such as those from the ISBA land surface parameterisation should be considered, such as that pertaining to runoff. It was also considered in this work that observation and modeling errors were not correlated in space and time which might not be realistic.
The use of more realistic errors simulated by Lion (2012) in the framework of the SWOT mission pre launch investigatiions will be considered in future studies.

Another perspective consists in the application of this method to other TRIP parameters, or several parameters at a time. Correction of ISBA parameters, such as those controlling sub-grid runoff for example, is also planned but must be considered carefully as the impact on the river is less direct. Before the satellite launch, the AirSWOT airborne campaign will provide SWOT-like datasets of water level, which will enable studies using a more realistic SWOT DA application, instead of the Observing Simulation System experiment presented here. Even if this airborne campaign will not cover the Niger basin, it will potentially provide a better observation error model. Yet, using more complex observations and model errors might require a modification of the assimilation scheme to overcome extremely strigent EKF filter asumptions of Gaussian unbiased errors. Possible assimilation techniques to test are the Ensemble Kalman Filter
or the particle filter.'

p.4505 l.10: "Another perspective: : :": the organization here is confusing as the previous sentence is about results and not perspectives. The conclusion should be reorganized so that this follows the suggestion of further work with different error models, and so that the first part of this paragraph directly follows the assertion that the assimilation experiment provided good results (somewhere around p.4504 l.17).

Corrected

p.4505 l.21: "the/run" typo?
-Figures
Fig.4: typo in legend "input"
Fig. 8, 13 & 18: missing y axis labels
Fig.10: y axis should be unitless
Fig.11: appears to be missing two locations

Fig. 14: Please correct numbering

All corrected

Editor comments answers

The author's response to the referees were rearranged. Colors were used to distinguish between the referee comments and the author's response. The line and page number were provided as often as possible.

1. The typesetting of authors' response document is really awkward. Please modify it and provide a general description for your revision.

The author's response to the referees were rearranged. Colors were used to distinguish between the referee comments and the author's response. The line and page number were provided as often as possible.

2. Ln35: valuey, value??

Corrected.

3. Ln54: CHS is a special model or a type of model, please clarify.

CHS stands for Continental Hydrologic System and is used to refer to the general coupled CHSs are generally made of a Land Surface Model (LSMs) which computes the water and energy budget at the surface-atmosphere interface, coupled with a routing model which distributes the runoff to the river and the soil storage components.

4. L90: Why say wetland and floodplains are three-spatial dimension, while satellite observation is 2D? In my mind both are 2D. Please clarify.

Discharge is generally used to evaluate hydrological models but discharge gives an information only along the river flow direction ie a 1D information. Satellite informations such as flood fraction data (Papa et. Al, 2010) provide a 2D information about the surface water. In my opinion, wetlands have to be seen as 3 dimensional processes since they generally fill wetland areas by flowing in 2 directions but also contribute to vertical flows such as the infiltration to the soil.

5. Ln200: Give the full name of ISBA when it first appears.

Corrected.

6. Ln246: The Manning roughness coefficient is considered to be a key parameter. Could you provide more evidence? For example, from your own sensitivity analysis or the existing literature.

Thank you for this comment.
Unlike river depth and width, which can be estimated through direct measurements, Manning coefficient can be estimated only indirectly, using bathymetry and flow velocity measurements.

Several studies discussed the importance and difficulty to estimate the Manning coefficient (Ven Te Chow et al., 1988; Argement et al., 1989; Bates and de Roo, 2000).

The sensitivity of the Manning equation to several river parameters including the roughness coefficient was investigated by Pistocchi and Pennington (2006). In addition to the concern about estimating accurately the Manning coefficient value, they highlighted the importance of considering its spatial distribution instead of a unique value as it is done in some hydrological models (Arora and Boer, 1999).

Moreover, the Manning coefficient is often used as an adjustement variable for models calibration which can lead to additionnal errors (e. g. Biancamaria et al., 2009). Hunter et al. (2007) indicated that very frequently in models errors on topography and roughness dominate errors from equation approximation.

The estimation of the Manning coefficient is thus considered by litterature as one of the major issues limiting the performance of hydrological models and, to the authors knowledge, there have been very few attempt to evaluate the potential of satellite data to correct it. Therefore, it was chosen as the main parameter to be investigated in the DA study presented in this study.

7. Ln317: A more recent a??

This was replaced by “More recently, a 0.5....”.

8. Ln329: RRM??

RRM stands for River Routing Model. This is precised in the introduction and at line 313.