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Interactive Comment

Interactive comment on "Operational reservoir inflow forecasting with radar altimetry: the Zambezi case study" *by* C. I. Michailovsky and P. Bauer-Gottwein

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The authors would like to thank Dr. Schumann for his review of this paper. The following aims to address the comments provided.

- P9616, L5: please replace optimal with improved since an optimal forecast of flows is governed by optimal forecasts of initial boundary conditions to the model, such as precipitation, which in this case is independent of water level assimilation

This will be changed in the revised manuscript.

- P9616, L12/15: I feel these sentences are contradicting: first it is argued that radar altimetry is difficult to be used operationally (which I agree) but then the following sentence states that the use of radar altimetry can overcome this limitation. I imagine I know what the authors mean here but please consider revising this.

The point made is that while using radar altimetry brings specific challenges which are not seen for traditional gauge data, it can be used in different ways. The sentences will be rephrased to make the message clearer.

- P9616: Why was the initial performance of the model so low (NSE 0.2)? I assume this is because of the large error in precip forecasts? If so, this is worth emphasizing in the abstract.

Yes, this is because of large precipitation errors, in particular in 2005 where large differences were observed between different remotely sensed precipitation products. When the year 2005 is left out, the NSE at the outlet is of 0.44. The manuscript will be edited to provide more information.

- P9619, L9: Please write 'through storage and evaporation', flow dampening is primarily through temporary floodplain storage of water coming from the river

Yes, we agree, the manuscript will be edited.

- P9621, L12/13: is there a paper reference that could be cited here to back up the statement that groundwater parameters are the most sensitive ones?

As mentioned in the manuscript, the groundwater parameters were chosen as calibration parameters as they are the ones which are not initially determined based on the input datasets for soil and landcover as well as our own sensitivity analysis.

- P9623, L21-25: if the SRTM DEM is available, why not use the real floodplain geometry at each side of the reach thru a 1-D extended cross-section approach for instance?

This will be implemented in the revised manuscript. Due to the coarse spatial resolution of the reach set up in the model, floodplains need to be approximated by an equivalent floodplain extending along the length of the reach and one representative cross-section was therefore extracted per floodplain reach and the floodplain shape parameters fitted to match the data.

- P9624, L7-9: are these low and high flow widths from Landsat different enough? In some rivers in the world this approach would be limited since widths might not change much (e.g. Amazon or engineered river bank systems)

In rivers where widths do not vary (or not enough to be seen with Landsat), the method will simply yield a rectangular cross-section. The smallest width difference observed for our case study was of approximately 20m.

- P9626,L4/6: In these equations, I assume w (bottom width) is taken as the low water Landsat image-derived width; there is certainly a considerable amount of uncertainty in this assumption given that your bottom width might be significantly overestimating very low flow (i.e. at zero depth). How do you account for this?

Thank you for the comment; this was an oversight in the manuscript so far. This will be taken into account in the revised manuscript by using historical low-flow values in order to derive a low-flow reference depth following the method described in Michailovsky et al. 2012 (Michailovsky, C.I., McEnnis, S., Berry, P.A.M., Smith, R., and Bauer-Gottwein, P.: River monitoring from satellite radar altimetry in the Zambezi River basin. Hydrology and Earth System Sciences, 16 (7), 2181-2192,DOI:10.5194/hess-16-2181-2012, 2012.). The bottom width can then be obtained from the measured width, bank slope and the derived depth.

- P9626, L7-10: am I correct in assuming here that you adjusted the altimetry readings to your modeled depths? In this case, the observations are not really independent from the model anymore, how does this affect the assimilation? Would you make it all work in favor of the exercise so to speak?

It is correct that the mean of the modeled depths and altimetry readings are matched over the calibration period. This is done because it is necessary to have a common reference between model and measurements. The consequence of this is that while the altimetry measurements will effectively be able to improve water level variation, they will not be able to correct biases in the model if they are present in the calibration time period.

- P9630, L1-3: again, same comment as before: here it is worth briefly explaining why the prior NSE is so low (0.2)?

More information will be added to the manuscript.

- P9631, L25: Essentially, as is illustrated by watershed 2, if initial conditions are already quite good leading to acceptable prior model performances, assimilation does not help much. This is of course expected but I think is worth noting in the conclusion that assimilation is really powerful when initial conditions are poor (as illustrated by the case of watershed 1).

More emphasis will be added on this point in the conclusion.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 9615, 2013.