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Discussions

Interactive comment on "Kalman filter approach for estimating water level time series over inland water using multi-mission satellite altimetry" *by* C. Schwatke et al.

Anonymous Referee #3

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The authors present an interesting approach for deriving water heights over inland water bodies utilizing the Kalman filter method and increasing the temporal resolution of inland water time series by combining multiple altimeter missions that cover a target of interest. The resulting time series for various inland water bodies are made available in the Database for Hydrological Time Series over Inland Water (DAHITI), maintained by the Deutsches Geodätisches Forschungsinstitut (DGFI). Generally, I like the idea to employ a Kalman filter technique in the processing of inland water heights and the increase in temporal coverage by combining different missions. However, I think that the proposed Kalman filter approach is quite simple and some details of the method-

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ology should be made clearer, especially why a detailed hexagonal grid is needed for the whole water body instead of just utilizing the measurement positions (or some fixed bins along the groundtrack) of the altimeter itself. Therefore, I generally recommend the paper for publication since the DAHITI database is an interesting addition to the available range of databases, but after addressing the major issues below.

GENERAL COMMENTS

The focus of the manuscript is unclear. The title suggests that the Kalman filter approach is new and the core topic of the paper, but in fact the authors describe their own database and its features all quite wordy while the core of the KF method is described relatively sketchy (apart from the general Kalman filter equations, which can also be found in a textbook). If the authors feel their method is innovative, they should put a stronger focus on it and provide more details on the method, motivations for choosing certain parameters and validations (and maybe shorten some other topics). See also below.

The core of every Kalman filter is the dynamic model, the propagation of which is merged with the data in a least squares sense. The authors assume an identity matrix for their dynamic model and introduce an a-priori error of $5cm^2$. This basically reduces the Kalman filter to a recursive least squares method. The choices for the model and the a-priori error, but also for the (optional) retracking procedure should be properly motivated in the text. Additionally, it could be validated whether the result is sensitive against e.g. the choice for the a-priori error or the choice of retracking algorithm, or the grid spacing, etc. . This would make the choices, which might also vary with respect to the target water body, more plausible.

The authors introduce their Kalman filter method utilizing a precomputed, hexagonal grid while assuming a constant lake level. I do like the idea of employing a Kalman filter, but I do not really see the necessity of the grid when all the grid points are assumed to be the same height (P4828, line 1). In the end all grid points over the lake are

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averaged into one final height output. I suppose, utilizing the actual altimeter locations (with the same assumption for constant heights and averaging) would yield nearly the same result? The authors state on P4825, line 8-10 that the observations are linked by A_k only to the nearest grid points. When computing the average, only the “best water levels” of all grid points of each epoch are selected (P4828, line 3-4). I assume, these should in general be the grid points very close to the actual altimeter groundtrack since only these will be influenced by the observations?

Furthermore, when using the grid, the individual grid points would be correlated, yet the authors assume uncorrelated system noise (P4826, line 5) and initialize their covariance matrix E_xx,k using an identity matrix (P4825, line 12). On P4824, line 10-11 the authors say that the grid is used to account for river slopes, but I think referencing the individual heights to a fixed point (e.g. by correcting for the differences in geoid height between the measurement point and the reference point) might also do the trick. The authors should better motivate these choices to make it more comprehensible.

Another point would be the applied retacking. Why are not all the lakes (rivers) retracked. This would be more consistent. Additionally, why use 1 Hz heights instead of 20Hz (or 18Hz or 40Hz) heights all the time over inland water bodies? Furthermore, the small threshold of the applied Improved Threshold Retracker of 10% might lead to the detection of small bumps in front of the desired leading edge in a sub-waveform, depending on the number of additional range gates considered before and after the detected sub-waveform.

Maybe the biggest problem is that no systematic analysis of the many choices is made in which the results differ. It is difficult for the reader to find out which of the choices actually leads to improvements and which is not really required. What I mean is something like, first, testing the effect of the SVR outlier detection (see also “specific comments” on this). Then investigate the effect of the additional threshold outlier correction, the Kalman filter, etc.

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As mentioned, parts of the manuscript are quite wordy, e.g. the description of the possible choices in the openADB database in the beginning of 3.1 (P4822). Additionally, some information is provided several times throughout the manuscript (see also the "specific comments" part). These could be shortened and more focused.

Command of language is a problem. I am not a native English speaker, but there are, e.g., a lot of commas missing in some parts of the text. Someone with better knowledge of this than me should proofread the manuscript later.

Why are there two sections containing pre-processing? (Sect. 2 and Sect 3.1)

I like the selection of the investigated water bodies, as they cover large lakes, smaller lakes, as well as rivers. However, some additional information on the chosen criteria for outlier detection etc. would be helpful.

For the results, the DAHITI time series are corrected for possible outliers while time series from other groups which might still contain outliers (e.g. GRLM) are not corrected. I think, it would be more consistent when the same outlier detection procedure is applied to the other time series, too. Additionally in my opinion, the correlation with an in-situ time series is more meaningful in combination with mentioning the number (or percentage) of removed outliers from the total number of points; after removing a lot of outliers, the correlation will always become better.

In the introduction, the authors mention that the determination of the accuracies will be covered in another paper. Why is this not included here, since the outlier detection and Kalman filtering are depending on the derived accuracies? Generally, I think this is an interesting and important part of the time series provided by DAHITI.

SPECIFIC COMMENTS

DAHITI The name of the database is introduced several times. At first in the abstract and again on page 4817, line 15, which is fine. But also on page 4821, line 26 or page 4837, line 2 or page 4838, line 8-9. I think one (or two) time(s) is fine.

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DGFI Similar to “DAHITI“, I think that introducing it once on page 4817, line 16 is sufficient.

— Abstract

P 4814, line 7-9: I think both sentences basically providing the same information. I would suggest writing something like “... approach incorporating cross-calibrated altimeter data from Envisat ...”

— Introduction

P 4815, line 18: The definition of “footprint” is not precise. The pulse limited footprint (which the authors talk about) is much smaller than the beam-limited footprint defined by the antenna beam-width.

P4815, line 24: How are the hooking effects treated? This is important, especially when small lakes or rivers are investigated.

P4815, line 27-28: I think this sentence might be misunderstood in the way that the curves are fitted to the final heights instead of the heights of one overpass over the target of interest.

P4816, line 1: When talking about ocean-like waveforms, one might refer to the Brown model (Brown, 1977). Similar on P4819, line 16.

P4816, line 4: Generally, all threshold retrackers are sensitive to the geometrical waveform shape, e.g. large peaks will lead to a larger amplitude etc. Therefore, I think “robust” should be rephrased. Same for P4819, line 20.

P4816, line 7-11: The first two sentences of this paragraph basically provide the same information.

P4817, line 22: “optional retracking”. Why is the retracking optional and not mandatory for all inland water targets. See also general comments.

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— Altimeter data and preprocessing

P4818, line 12-13: The first sentence repeats information already provided in the introduction. I'd suggest to simply remove this sentence.

P4819, line 3: "about 80km" → "about 80km at the equator"

P4819, line 3-5: Since Envisat stopped working in April 2012 and SARAL was launched in March 2013, there is a gap in the coverage.

P4819, line 18-20: Why use a threshold of 10%? Additionally, the algorithm might provide more than one sub-waveform and corresponding water height. Is the "correct" height selected by utilizing a reference height like in Hwang et al, 2006 or Guo et al., 2009?

P4819, line 20: How is it decided whether retracking is necessary or not? Is there a reference for the statement, that ranges from the 10% Improved Threshold Retracker are more reliable?

P4820, line 1: Suggestion: "altimeter range" could be renamed to "retracked altimeter range" to make it clear that the ranges are always retracked and not tracker ranges.

P4820, line 22: It would be nice if the authors could give more detail on the "certain thresholds" that were used.

P4820, line 25: "... and do allow to predict where ...". I think "... and do not allow to predict where ..." was intended.

P4821, line 1-4: Remaining uncertainties in the geoid might lead to significant height differences depending on the location over the lake, especially for larger lakes. So the assumption of a constant height level of the derived heights might not be satisfied.

— Kalman filter approach

P4821, line 22: Rephrase "hexagonal computation".

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P4822, line 14: Accuracy is not the same as precision.

P4822, line 15: The abbreviation SD for presumably “standard deviation” has not been introduced at this point.

P4822, line 16: The authors should elaborate why they use a “floating” box (and maybe rephrase it to “sliding”) with the size of 5 instead of just all the valid heights over the target of interest. Especially, since deriving the standard deviation reduces the degrees of freedom (dof) by two, leaving only 5-2=3 dof, which is quite a small number.

P4822, line 19: “without significant slopes”. As mentioned before, there might be a significant slope from uncertainties in the geoid correction. Therefore, what will happen in case there is a significant slope and the assumption is not satisfied?

P4822, line 21- P4823, line 4: I think it is not necessary to explain all possible choices for outlier detection in the software package, but rather focus on what was chosen for the water height derivation in DAHITI.

P4823, line 6-7: “ the SVR on [. . .] is applied.” → “SVR is applied to ...”

P4823, line 9: “a lake” → Which lake?

P4823, line 11-12: How have these thresholds been selected? Are there different thresholds for different targets (large lakes, smaller rivers, etc.)? When looking at Fig. 2, only a small number of points (green) are actually rejected by the outlier detection which would probably be also rejected by the selected threshold criterion. Maybe the authors could elaborate on why they need both stages of outlier detection?

P4823, line 14-16: The authors should mention this a little bit earlier in the before the example with the lake. Additionally, it is not mentioned in the results section which criteria for outlier detection were chosen for the different inland water bodies.

P4823, line 19: Suggestion: Maybe rephrase “heart of DAHITI”.

P4823, line 24-25: The Kalman filter only gives an optimal estimate for linear models

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and Gaussian distributed model states and observations.

P4824, line 23-24: I think the sentence might be a bit unclear. Suggestion: “Time intervals shorter than one day are precluded by assigning the individual measurements to full days”

P4825, Eq 2: I think the x_k vector is missing a minus sign in the exponent

P4826, line 5-6: Why 5cmˆ2? Why are the grid nodes uncorrelated (see also general comments)?

P4827, line 7-8: I suggest to rephrase it to “ . . . accuracies will become smaller within the updating step” since “reduced accuracies” implies that the accuracy becomes worse.

P4827, line 15-20: The information on the transition matrix and the assumed variances has already been provided on P4826, line 2-6.

P4828, line 6: Why 5-10cm? Is this value larger for smaller lakes or rivers? How is it chosen?

P4828, line 17-21 (Figure 5): How is the confidence interval “manually defined”? Is it different for each target? The dashed lines in Figure 5 are not described in the caption of the figure. Which lake is used for the example in Figure 5?

— Results and validation

P4829, line 14-15: The different target types have already been mentioned in the beginning of Sect. 4.

P4829, line 19-20: Suggestion: Remove that sentence, as it provides no new information.

P4829, line 23-25: This may be correct for the central parts of the lake but closer to the land influences will impact the waveform shape.

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P4830, line 16: “several lakes are frozen for several month” Which ones? Are all measurements during these month treated as outliers?

P4831, line 20-21: I’d suggest to remove the first sentence, as it repeats information that has been mentioned before.

P4832, line 5-6: Computing the relative height changes does not avoid all the uncertainties in in-situ gauge data. E.g. there could still be gross reading errors in manually operated gauges.

P4832, line 28 (Figure 7) Why is the year 2004 highlighted?

P4833, line 2: How have the time series been shifted? With respect to the mean or one (or more) selected cycles?

P4833, line 4: After applying two outlier corrections during the processing, one should in general not expect any more outliers.

P4833, line 5: Why use the 1Hz ranges instead of consistent 20Hz (or 18Hz or 40Hz) ranges for all targets? Small land influences on the trailing edge of the waveforms might still influence the results of the standard ocean retracker ranges, available in the GDRs. (See also general comments).

P4833, line 8-9: From Table 1, one can see that other stations showed larger RMS compared to Ontonagon and Grand Marais. Is this connected to the distance of the in-situ stations to the altimeter ground tracks?

P4833, line 9: In my opinion the correlation coefficient alone is not a good measure for the quality, since removing a large number of “outliers” automatically improves the correlation drastically. So additional information on the total number of measurements and the number of removed outliers from the final time series might be helpful here.

P4833, line 16-17: Have the results from the other databases also been corrected for possible outliers? E.g. the unsmoothed GRLM results which, judging from Figure

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7, are used here might still contain large outliers in some cycles, which will have a significant effect on the computed RMS. (see also general comments).

P4833, line 22: Figure 8 deals with a different lake so it does not show the “same time series” like Figure 7.

P4833, line 24: The GRLM time series for Lake Athabasca is no longer available on the GRLM website.

P4833, line 24-25: Figure 8 highlights the year 2004 (like Figure 7) and not 2010. Again, what makes this year special?

P4834, line 1: “very good” → The results for the Topex/Poseidon era seem to be more noisy compared to LEGOS.

P4834, line 2: Since “outliers” should have been removed during the processing, I’d suggest to maybe rephrase this.

P4834, line 2-7: Generally, the same concerns regarding mentioning the number of outliers and also applying the same outlier removal to the time series of the other groups.

P4834, line 9: Suggestion: Maybe remove the first sentence.

P4834, line 13: “only one mission” → Figure 9 includes results based on Envisat and SARAL. These are two different missions.

P4834, line 14-15: Similar to P4819, line 3-5, Envisat stopped working in April 2012 and SARAL was launched in March 2013, so there is a gap in the coverage.

P4834, line 9-24: What pre-processing criteria (mentioned in Sect. 3.1, e.g. SD thresholds) have been used? Why is the year 2010 highlighted?

P4835, line 7: “used points (No)” → Are the number of removed outliers included? (Same for Table 4).

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P4835, line 9-10: The smallest and largest RMS are not marked in color in Table 3.

P4835, line 15-18: Wind effects or other local influences might have an impact on the gauge station measurements. Again this raises the question whether stations closer to the track show better agreement.

P4835, line 19-27: In case the other products still contain outliers, the comparisons might need to be improved before a general better performance of DAHITI can be validated.

P4836, line 1-4 (Table 4): I think it would be helpful if the authors would include information on the utilized satellite data in each row (Maybe, e.g.: Station Name (J2) ...).

P4836, line 15-16: Is there a reference for that statement? Especially for rivers, the quality of the reprocessed water heights is quite depending on the surrounding terrain (in this case mainly rain forest?), etc. I think it would be nice to rephrase this statement to make it more clear.

— Conclusions

P4837, line 15: Related to the previous point, the authors could elaborate (either here or in the previous section) what is meant by “surrounding conditions”.

— References

- Well presented, except for a few missed “{}” (see “technical comments”).

TECHNICAL COMMENTS

I will only list a few things here, since the manuscript will probably be reworked anyway.

— Abstract

P 4814, line 2: “However, since some years” → “However, for some years”.

P 4814, line 16: “from available other altimeter ...” → “from other available altimeter ...”

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— Introduction

P 4814, line 26: “However, in the last years . . . “ → “However, over the last years ...”

P4815, line 27: “... fitting curves on the resulting ...” → “... fitting curves to the resulting ...”

Altimeter data and preprocessing

P4820, line 14: Missing words. → “Finally, each single altimeter measurement is corrected for its ...”

— Kalman filter approach

P4822, line 5: “data base” → “database”

P4822, line 14: “Due to lacking absolute ...” → “Due to the lack of absolute ...”

P4823, line 19-21: Just a suggestion: “... input heights by combining of the [. . .] at different locations (Kalman, 1960).”

P4824, line 15: “At the beginning an initialization ...” → “In the beginning, an initialization ...”

P4824, line 18: “contant” → “constant”

P4824, line 18-19: Rephrase: “That means that our system each time is updated if a new ...” → “That means that our system is updated each time a new ...”

P4826, line 15: “actual” → “current”

— Results and validation

P4830, line 21: To be consistent with the other times it is mentioned, write “Lago Argentino”

P4831, line 5-6: Maybe merge the two sentences to one.

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P4831, line 7: Suggestion: replace “wet” with “humid”

P4831, line 8: Suggestion: replace “show” with “reach”

P4831, line 10: Suggestion: replace “diversification” with “variety”

P4832, line 10-14: Suggestion: These 3 sentences contain the word “differences” five times. Maybe rephrasing some of it will make it more easy to read.

P4832, line 18: “disparate” → “divers”

P4833, line 21: “reaching” → “providing”

P4834, line 19-20: “... when taking the [. . .] into account” → “... when taking into account the ...”

— Conclusions

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— References

P4839, line 31: The names “ERS” and “ENVISAT” are in curly brackets.

P4840, line 33: The name “SARAL” is in curly brackets.

P4841, line 15: “Shum, C.” → “Shum, C. K.”

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