Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-165-RC1, 2020

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Sentinel-3 radar altimetry for river monitoring – a catchment-scale evaluation of satellite water surface elevation from Sentinel-3A and Sentinel-3B" *by* Cecile M. M. Kittel et al.

Anonymous Referee #1

Received and published: 11 June 2020

The manuscript from Kittel et al. presents a validation study of Sentinel-3A/B (S3-A/B) SAR altimeters measurement over the whole Zambezi basin. Time series at 175 virtual stations data have been extracted on the river network, floodplain, reservoirs and wetland. Only 6 in situ gages could be used to validate this database, showing a RMSD between 3 to 31 cm. However, no direct validation can be done for the remaining 169 VS and especially over wetlands (except that the seasonal cycle is well captured and coherent with past in situ observation or nearby VS). Some discussions on the benefits and drawbacks from 1. the open loop tracking mode and 2. the different processing

Printer-friendly version

Discussion paper

available on two platforms (SciHub and GPOD) to process S3 data complement the manuscript.

General comments: It should be noted the important work done by the authors to extract this unprecedented database of WSE time series over the whole Zambezi basin and the interesting discussion on the open loop mode and the SAR altimetry processing. However, the authors should better highlight the new discovery from their manuscript and why it has to be published in HESS and not in a more specific remote sensing journal. This is my main concern and the reason why I suggest major revision. As stated by the authors: "The objectives of the study are to evaluate the density of valuable observations and establish a WSE monitoring network. Additionally, we demonstrate the potential application of Sentinel-3 for monitoring river interactions with wetlands and floodplains." The issue is that validation and discussion on SAR and open loop mode have already been done in Jiang et al. (2020) over rivers in China. The submitted manuscript confirms some conclusions from this paper over another basin, but does not bring new information concerning S3 measurements, nor on the hydrology of the Zambezi basin. The application of radar altimetry for monitoring interactions between river, floodplains and wetlands has already been investigated by other studies with different radar altimetry missions. Another previous study from this group (Michailovsky et al., 2012), also studied the Zambezi basin with the Envisat radar altimeter and derived discharges from these WSE with different methods. The main benefit of the submitted manuscript is the important database of WSE over the Zambezi basin derived from S3 missions. So, according to me, the submitted manuscript is a database presentation paper, but the database does not seem to be freely accessible, like other global altimetry database (e.g. Hydroweb, DAHITI...).

Specific comments:

Few clarifications are needed in the abstract. For example, give the name of the two datasets the first time you mention them (line 4). Especially, the sentence "Additional VS are available in both the Copernicus Open Access Hub and GPOD", seems to

suggest that these two datasets are different from the two platform mentioned on line 4, which is not the case. That's why, when reading only the abstract, this sentence is confusing, especially the term "additional". It is not clear from which dataset the Copernicus Hub and GPOD provide additional information.

Line 8: Give the meaning of RMSD acronym.

Line 17: I have some doubts about using S3A/B as a SWOT surrogate. SWOT will do quasi global observations over two swaths, providing not just WSE, but also water extent and surface water slope, which could not be derived from S3A/B only. Besides, the temporal/space resolutions are coarser for S3A/B.

Line 18/19: This sentence is quite general. Similar conclusions were also reached in Jiang et al. (2020) for rivers in China. Besides, in the submitted manuscript, there is no comparison with other mission that does not have SAR mode. So it is difficult to conclude from this manuscript only that SAR mode brings more information than mission with LRM mode.

Lines 27-30: References provided here correspond to only few studies linked to these subjects. That's why I suggest to add "e.g." before the references in brackets.

Line 36: Getting "up-to-date" reference for the databases is very difficult (for example Cretaux et al., 2011 corresponds to the old "lake" version of Hydroweb). To overcome this issue, you could rather point out to the web link for each database. It's just a suggestion, so I let the authors decide if they want to do that or not. There are other altimetry databases than the ones cited in this sentence, like HydroSat (http://hydrosat.gis.uni-stuttgart.de/php/index.php) and GRRATS (Coss et al. 2020, https://doi.org/10.5194/essd-12-137-2020; https://podaac.jpl.nasa.gov/dataset/PRESWOT_HYDRO_GRRATS_L2_VIRTUAL_STATION_HEIGHTS_V1). And for lakes, there is the G-REALM database (https://ipad.fas.usda.gov/cropexplorer/global_reservoir/).

Line 47: For S3 mission, you should rather cite the S3 mission requirements document (S3 MRD), available at http://esamultimedia.esa.int/docs/GMES/GMES_Sentinel3_MRD_V2.0_update.pdf, rather than Jiang et al. (2020).

Line 44: “Sentinel-3 mission is a marine and land mission” This sentence is of course true, but it could give the feeling that both ocean and land requirements are considered equally, which is not the case for the altimeter part of the mission. Indeed, it is worth pointing out that for the topography component of S3 mission “Altimetric gauging of river and lake water levels is a secondary mission objective [. . .]. This requirement shall not compromise the ability of the altimeter to meet the primary ocean and ice topographic mission objectives.” (section 4.4.2 in S3 MRD).

Line 62: “To allow continuation of the historical ERS/Envisat time series, the Sentinel-3 orbit is similar to the orbit of Envisat” This sentence is confusing, as S3A/B cannot continue VS from ERS-1/2 and Envisat, as the orbit and its phasing is not the same. You can argue that S3 provide more spatial sampling than some other missions (e.g. Jason series), but it is not a direct continuation of previous ERS and Envisat ones.

Line 64-67: These sentences are confusing for people who know nothing about the OLTC. It should be clearly stated that the “on board Hydrology Database (HDB) targets” is part of OLTC table. It should be introduced earlier, in the OLTC description section.

Line 137: Could you provide more information on this receiving window? The explanation provided in the current manuscript is interesting, but it is still difficult to understand clearly what this receiving window is and why it is needed. In the manuscript, it is written that it should “not to be confused with the on board reception window”, but it’s not clearly defined. It is important to better explain it for readers not familiar with SAR altimeter processing (and even more, for those not familiar with altimetry at all).

In section 2.3.3, please cite briefly the corrections taken into account in the two datasets.

Line 170: According to Jiang et al. (2020), the RIP is in Watt, so please indicate the unit in "(>10^-13)"

Lines 174-175: Could you provide some estimates of the two DEM errors (provided in the DEM reference paper or in the DEM quality matrix, for the Zambezi basin). It would help the reader to assess if the 30m threshold is much above the DEM accuracy.

Line 178: According to Jiang et al. (2020), the fit is a gaussian fit, isn't it? It could be worthwhile to mention it (and maybe to add a sentence to explain why the fit is needed).

Line 195: "Retrieving the untracked range gives an assessment of whether the expected WSE was within the on board reception window", I agree, but this statement is very general and you could better explain how you will use this information. If you don't know the expected WSE (which is the case for 169 of your VS), I don't see how you can really make use of this information. Will you compare it to DEM?

Line 211: "WRMSD (Weighted RMSD) by dividing with the residuals with the in-situ standard deviation" it not clear, please rephrase

Equation 3, to be coherent with the text change D_{RMS} with RMSD

Line 217: "We correct for datum shifts by using the WSE amplitude and therefore expect a bias of 0 cm." You need to provide more explanation. First, how did you use the amplitude and to compute what? Second, I don't understand why you need to correct datum shifts, as you already removed to time series "mean level at overlapping sensing dates is subtracted". So why is it needed to add any other bias correction?

Lines 244-251: All the criteria used by the authors are not easy to follow, as they depend of the dataset and the product level. It should be better explained in the methodology section, with a clear flowchart of the process and a more in depth explanation of all the criteria used.

Line 250: "we use NP alone as the L1b selection criterion" but how did you use NP? Using which threshold? It is somewhat difficult to understand why NP is a good criterion,

as multiple targets could be in the waveform and it does not mean that the altimeter is not observing the target of interest. It is especially true for small tributaries, where there are a lot of missing data on Figure 2. The missing data could also be due to your criteria. Could you discuss it in more details?

Line 254: “The rejection rate is higher in the SciHub dataset, with rejected stations throughout the basin.” This sentence seems to meet the concern expressed in my previous comment.

Table 2: What is the line “OLTC” in Table 2? It is not explained in the table legend, nor in the text.

Figure 4: This figure does not seem useful, except to state that after OLTC update there is mainly 1 peak in the waveform. But as the NP before the update is not provided, it is difficult to estimate the improvement.

Lines 266-267: “The OLTC contains targets based on elevation information from hydrology databases (e.g. Hydroweb), virtual stations networks and the global ACE2 DEM (Altimeter Corrected Elevations v.2 Digital Elevation Model)” Actually it depends of the OLTC version you are considering, as stated later in your paragraph. According to https://www.altimetry-hydro.eu/ here are the different OLTC table versions over inland waters: - For S3A: * DEM: v5 (Date start: 2019-03-09) * DEM: v4_2 (Date start: 2016-05-24, Date end: 2019-03-01) * DEM: v4_1 (Date start: 2016-04-18, Date end: 2016-05-24) - For S3B: * DEM: v2_0 (Date start: 2018-11-27) * DEM: v1(tandem) (Date start: 2018-06-06, Date end: 2018-10-16) Especially, on the https://www.altimetry-hydro.eu/ you can see that ACE2 DEM is heavily used in v4_2 for S3A, but not used at all in v5 over the Zambezi basin, as shown on Table 3 but not clearly stated in the text. Besides, at line 268 and in other part of the manuscript, it is written that the table has been updated in March 2019. It is true for S3A, but not for S3B, which has been updated sooner (after the end of the tandem phase in November 2018). The OLTC versions are given in Table 3, but never really explained in the text. A good reference for OLTC tables’

Printer-friendly version

Discussion paper

generation is (with some validation): Le Gac S., F. Boy, D. Blumstein, L. Lasson and N. Picot (in press). Benefits of the Open-Loop Tracking Command (OLTC): Extending conventional nadir altimetry to inland waters monitoring. Advances in Space Research, https://doi.org/10.1016/j.asr.2019.10.031 I think putting a table to summarized all these OLTC versions and dates could be useful in the manuscript, with some information on OLTC generation (see Le Gac et al., in press). These information should be put somewhere in section 2.

Figure 5: On the map, the black line (sub-basin boundaries?) are not defined, does not seem to be useful and make the map difficult to read. I suggest removing them. Where they are close, S3A and S3B VS are difficult to differentiate. Maybe use different color or level of grey between the two missions. On the sub-plots, write when it is S3A or S3B. In the legend, write to refer to figure 2 for the location of the map within the Zambezi basin (blue polygon on figure 2).

Line 280: “no new targets were uploaded to the OLTC in March 2019 near the two S3A VS” Just to be sure, even if no new targets has been added in march 2019 near these VS, it does not mean that the OLTC table has not been updated in March 2019 for these VS. Is it the case? From figure 5 even if it is the case, the updated value should be pretty similar, as the time series seems pretty stable before and after March 2019.

Line 288: “Samosa+ retracker outperforms the OGOC retracker”, first replace OGOC with OCOG. Second, from this sentence, I was expecting much better results with Samosa+ than with OCOG, whereas on table 4, SAMOSA+ is better only by few cm (or %, even most of the times few tenth of %). So I would encourage the authors to add this quantitative information to alleviate this sentence. Besides, Samosa+ comes from GPOD, whereas OCOG comes from SciHub, and processing between these two platforms are different (not just the used retracker, but also the data selection and probably other processing, corrections...) as described on sections 2.3.1 and 2.3.2. How these differences could impact the results shown on Table 4?

Table 4: For Chavuma station, Samosa+ RMSD is equal to 15.8cm and 3.3%, whereas for OCOG the RMSD is 25.6cm and 3.6%. How an almost 10 cm difference in RMSD between Samosa+ and OCOG only translates into 0.3% increase? I think there is an issue with the % computation (or with the RMSD value). Besides, the 9th column entitled "Relative RMSD" corresponds to WRMSD in the text, please replace "Relative RMSD" with "WRMSD" for consistency.

Line 315: "If we consider the stations, which are valid across datasets", how do you define "valid" here? Could you recall the criteria here?

Line 316: "The number of VS is quadrupled compared to using the global database Hydroweb", it is impressive. However, it should be noted that all Zambezi VS on Hydroweb have an "expert validation criteria" (see http://hydroweb.theia-land.fr/?lang=en&basin=ZAMBEZI and https://theia.sedoo.fr/wp-content-theia/uploads/sites/2/2020/04/Handbook_Hydroweb-V2.0-1.pdf). Are all the 145 VS being individually checked and validated (coherent seasonal cycle and amplitude from upstream to downstream VS)? Coherent amplitude and seasonal cycle has been shown only for 10 VS (and compared to in situ gage data only for 6 gages) in the manuscript.

Line 320: "At four stations in the Upper Zambezi, there are no valid observations at any of the VS prior to the OLTC update (Fig. 8)" I don't see how figure 8 shows that there is no valid observation before OLTC update, as figure 8 is only showing data after (S3A) OLTC update.

Figure 9: Concerning the zoom on the WSE vs. latitude plot (between -12.01°N and -11.81°N), it might be because of the color code, but it seems "after Schihub" WSE is in between 1050m and 1100m, whereas "After (GPOD/3x" WSE is in between 1000m and 1050m and "After (GPOD/2x" WSE is below 950m. I don't understand why there are not more consistent. I understand it is near the transition, which affect GPOD when changing the receiving window, but why is it that different, especially why "GPOD/3x"

is above “GPOD/2x” and not below (by tripling the window, you should have more data after the transition)? Besides, on the WSE vs. latitudes plots, I would suggest to draw all “Before” curves with dashed lines, to make them easier to differentiate with the “After” curves.

Line 350: “According to the OLTC website,” please give the URL of this website (I guess it is https://www.altimetry-hydro.eu/)

Section 4.2, which VS is considered here? A86 VS on figure B1? Besides, it to better see the impact of the platform processing versus the OLTC value, it could be good to show the OLTC table value (i. e. position of the tracking window), rather than the retracked value converted to WSE. It would help to show the transition and why you need to extend the receiving window with GPOD and then you can discuss the difference between the two platforms.

Lines 370-372: The second option is the one chosen during the March 2019 update, isn’t it?

Line 389: “This is likely due to the frequent cloud cover over the floodplain.” Or maybe due to vegetation cover masking water?

Line 412: the references provided here are just examples of studies using altimetry to calibrate and update hydrology model, so I suggest putting “e.g.” before the references.

Lines 423-428: There is not just Park (2020) and your study which investigated connectivity between river and floodplains. Could you increase your references list?

Line 429: “The cross-sections extracted over floodplains are similar to observations expected from the future SWOT” I disagree with this statement. Even if Sentinel-3 mission provides much more spatial observations than other altimetry missions (like Jason series), it is not comparable to SWOT measurements, which will provides images of WSE. So rephrase this sentence accordingly.

Line 430: Concerning SWOT mission, I think a better reference for the in-

terested reader will the SWOT Science Requirements Document (SRD) rather than Domeneghetti et al. (2018). SWOT SRD could be accessed with the following link: https://swot.jpl.nasa.gov/system/documents/files/2176_2176_D-61923_SRD_Rev_B_20181113.pdf

Line 432: “Similar information can already be extracted from the Sentinel-3 dataset in selected locations” Similarly to my previous comment, I think this sentence should be rephrased. S3 is providing WSE, but not water mask and of course slope could be computed between close VS, but it is far from being the one expected from SWOT images. . .

Lines 445-446: “We extract over 360 virtual stations from each satellite of which over 70 are validated based on the waveforms and temporal coverage for each Sentinel-3 satellite” Why stating this in the conclusion and not in the core of the manuscript? In the abstract 170 VS are mentioned. The same goes for the 70 validated VS.

Section 4.4 and 5: I find it strange to have perspectives before conclusions...

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-165, 2020.

Printer-friendly version

Discussion paper