Replies to the comments by anonymous referee #1:

We would like to thank the reviewer for their interest and comments on the manuscript. Below the reviewer's comments are in italic and the replies in normal font.

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 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...).

We thank the reviewer for the summary and general comments about the manuscript.

Indeed the validation is challenged by the lack of concurrent in-situ data and only 6 in-situ gauges were located close enough to VS to ensure direct validation of the satellite performance. We expanded the validation by considering the hydrological patterns at additional stations with historical records. However, extracting the full Sentinel-3 dataset has the largest potential value in supplementing ground observations in poorly gauged catchments.

As the reviewer correctly points out an important aim of the study is to demonstrate the extraction of a catchment-scale WSE monitoring network from Sentinel-3 observations. To address these main concerns, we propose to rewrite the introduction and objectives of the study to more clearly reflect this (see also specific suggestions below).

The database will be publicly available in conjunction with the paper and a link will be provided in the reviewed version. The code will also be publicized. The purpose is not the specific Zambezi-database (although that is a non-negligible product of the study) but rather to demonstrate that by using the publicly available processing platforms, such databases can be created for any catchment globally. This will greatly facilitate non-experts in altimetry to utilize altimetry dataset. We believe that a framework to extract catchment-scale monitoring networks to suit specific study areas has a wide range of applications in hydrology, which is why we believe HESS is a good target journal for publication.

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.

We agree with the suggestions and will move the introduction of the processing platform names to line 4 and clarify the later statement, so there is no confusion: the datasets refer to their respective processing platforms. We will also clarify that both datasets provide unique VS, where useful data can only be extracted from one platform due to various processing options.

Line 8: Give the meaning of RMSD acronym.

Will be changed to RMSD (Root Mean Square Deviation)

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.

This is a reasonable concern and we propose to remove the sentence from the abstract and instead highlight that the S3A/B constellation provides a useful and unique spatio-temporal coverage of wetland WSE with important implications for future hydrology-oriented missions.

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.

We suggest modifying the part of the final sentence of the abstract referencing the SAR instrument, to instead reflect the reformulated objectives of the study: extracting a uniquely dense Sentinel-3 WSE monitoring network at catchment scale and the importance of considering the pros and cons of the processing options on publicly available data processing platforms.

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.

We agree.

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/).

Thank you for the suggestion and the additional databases – we agree that the references are not up-to-date, we will add the links as suggested.

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).

Yes, the citation will be corrected.

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).

Thank you for pointing out this detail – we propose to change the text to reflect this. Mentioning the effort put into updating the OLTC hydrology targets may be a more important point to be made here (e.g. by citing the paper by LeGac et al. suggested further down in the comments).

Line 62: "To allow continuation of the historical ERS/Envisat time series, the Sentinel3 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.

Indeed, the statement can appear unclear. In the mission summary, it is stated: 'The mission provides data continuity for the ERS, ENVISAT and SPOT satellites'; however it is true that the altimeter does not provide a direct continuation of the ERS/ENVISAT VS on land. We propose to modify the text to clarify this statement and include the comment about the higher spatial density compared to the Jason series.

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.

We propose to reorganize the text to introduce HBD earlier with the OLTC as suggested.

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).

We propose to expand this section and the altimetry processing section to better differentiate the two. To avoid confusion, we will use "range window" to refer to the on-board reception window.

The range window is the vertical window that is recorded by the altimeter whereas the receiving window is the matrix within which the pulses are temporally stored prior to processing. Shifts in topography may mean that the 128 bin radar window cannot store the elevation of all the samples in the echogram. Using a larger window to store the range samples ensures that the signals of all the echoes can be stored inside the same radar window and that the leading edge (which is later retracked to obtain the WSE) is not truncated. Examples and further details can be found in Dinardo et al. (2018 - Advances in Space Research 62 (2018) 1371–1404).

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

This will be added. In the Scihub dataset, the files contain the "corrected altimeter elevation from OCOG (ice-1) retracker" and only the geoid needs to be subtracted. In GPOD the instrumental corrections are applied already and only the geophysical corrections need to be handled and they are already aggregated. The corrections include:

- Instrumental corrections: USO drift correction, internal path correction, distance antenna-COG and Doppler-slope correction
- Geophysical corrections: GIM-derived ionospheric correction, model dry tropospheric correction, model wet tropospheric correction, solid earth tide height, geocentric pole tide height and ocean loading tide.

The corrections are also provided individually, but the formats above greatly simplify the task for the less experienced user.

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

Correct.

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.

The two DEM have the following expected errors:

- ⇨ MERIT DEM – 2 m or better vertical accuracy and based on SRTM (16 m – RMSD around 9-10 m)
- ⇨ ACE-2 – better than 16 m but large regional differences. Considering the quality filter, ACE-2 has an accuracy better than 10m at over half of the VS in the Zambezi basin. MERIT DEM is expected to have a 2 m or better vertical 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).

In this paper, we calculate the Stack Peakiness using the maximum and mean RIP, therefore no fit is applied to the RIP beforehand. We will remove "fitted" from the text too. Indeed, in Jiang et al. (2020) a Gaussian fit was used to smooth the 64 sample RIP. We did not do this here as the fitting is not very crucial, and we do not use the SP to classify the data only to describe it.

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?

We propose to expand this sentence slightly to better introduce the use of the untracked range. It allows us to track how the untracked range changes when new targets are uploaded and to identify whether the OLTC is at the source of problems with the data.

We use the DEM as a reference, but most importantly, plotting the untracked range can help explain why some VS fail. If we are very far from the DEM it is unlikely that the target was sensed at all. Of course there is a risk in some cases that the DEM is so wrong (e.g. due to dam construction) that we lose information due to this filtering. We do not expect this to be the case in the Zambezi basin. With regards to the untracked range, the examples where we see large errors, the untracked range is off by far more than the expected DEM error.

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

Indeed, it should be: "WRMSD (Weighted RMSD) by dividing the residuals with the in-situ standard deviation"

Equation 3 was modified in line with HESS recommendations to avoid abbreviations in equations – for clarity and because it is a widely used abbreviation, we prefer to retain WRMSD/RMSD in the text body but will use D_{RMS} when referring to the equation.

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?

The amplitudes are used for comparison with the ground observations – the section is indeed unclear, as it is the same bias correction mentioned in two different ways. This will be rephrased for clarity. We only correct the bias once by subtracting the mean WSE at overlapping sensing dates.

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.

Thank you for the suggestion – we propose to add the following flowchart to the paper to illustrate the processing steps, and to reorganize the methods section accordingly. For the particular section, we suggest to already mention in the methods that the NP is used as a selection criteria. The criteria are the same across datasets, however MP and SP are not calculated for the SciHub dataset as the RIP is not available.

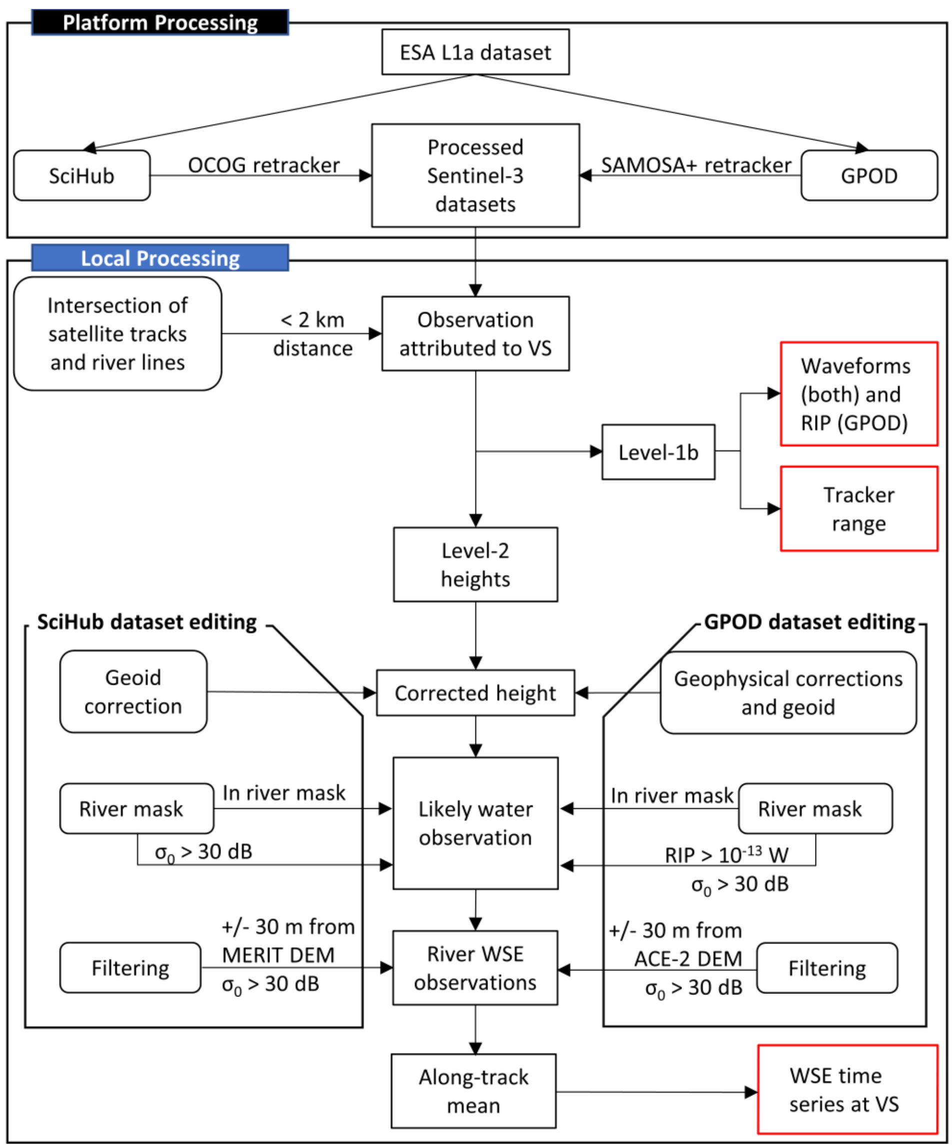

Figure 1 Data selection and processing flow chart. Highlighted in red are the output products used to assess the Sentinel-3 performance in the basin.

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?

As mentioned above, we will clarify section 3.1. Indeed multiple targets could be in the waveform, however that increases the risk of retracking errors if there are multiple high power targets. The missing data in Figure 2 is not due to this criteria as it is only based on what WSE observations could be extracted after corrections, water mask selection and filtering.
We base the evaluation on the along-track average number of peaks and select the 90th percentile as the selection criteria, meaning at least 90% of the data should come from single-peak waveforms. We did notice some small errors in the number of stations using this criteria. The error affects the number of valid stations but not the conclusions.

We also checked all rejected stations and found the following cases:

- Most have very little valid data at all, or several outliers (rejected on criteria of 80% data should be available). For some of these stations, dedicated (most likely manual) processing could help retrieve information if they were located in areas of interest
- Data loss due to OLTC update
- A few stations have seasonal water observations – but with a very wide spread – this is the case for VS on narrow river targets in wetlands.
- The stations rejected based on the single peak criteria mostly have very large across-track standard deviations, suggesting it is not unlikely that the waveform is contaminated by other bright targets and justifying the rejection of the VS.

Some of the retained VS might also require some degree of manual validation or outlier removal, however the proposed filtering greatly reduces the task (> 200 VS to check versus just over 100). It also allows users to group VS that they wish to further inspect and validate and to provide tools for pre-selection and evaluation. We propose to summarize this information in the revised manuscript.

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.

Several of the stations rejected had missing data in the GPOD dataset because the retracker failed to fit a model waveform to the observed waveform, suggesting the target is not a good water target. The OCOG retracker is less sensitive to the shape of the waveform. The higher rejection rate balances this.

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

We will modify the line in the table: it is the stations with data only after the OLTC update in March 2019.

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.

> We suggest removing the figure and state in the text that the OLTC update also improves the NP statistics (as shown already in Table 2).

*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' 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.*

> We thank the reviewer for the citation suggestions – we will incorporate them in the methods section as suggested. The update indeed only refers to the Sentinel-3A OLTC as the Sentinel-3B update as made prior to the beginning of the datasets considered. We will make sure this is clear in the manuscript.
>
> Indeed the targets mainly consist of HDB targets after the update – however they still rely on high resolution DEMs – as we understand, ACE-2 is still used to define many hydrology targets. We are very interested in further information if other high resolution DEMs are used instead of ACE-2 for the HDB targets.

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).

> Agreed – we will remove the lines, increase the difference between the two mission markers and refer back to 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.

> Based on the online OLTC webpage, the existing targets were only updated with no significant change in height, suggesting there is no point in splitting the time series in before and after the OLTC update.

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?

> Thank you for pointing this out. The difference is indeed only slightly better – we agree that the sentence could be misleading. We will nuance the sentence with the range of improvement from using Samosa+ versus OCOG and instead refer to GPOD and SciHub as the L2 WSE is compared in terms of platform, as it is not necessarily the retracker that drives the difference in performance. Because we use the processing options available on the two platforms, comparing the full processing packages and not the retrackers alone makes most sense in light of the objectives of the study.

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.

> Indeed, there was an error in the table as the values for Chavuma and Ngonye Falls were exchanged – thank you for pointing this inconsistency out. The correct RMSD relative to the yearly amplitude is 5.3% at Chavuma and 3.6% at Ngonye Falls.

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

> We will add detail and nuance the term "valid" – we consider the stations with single peak waveforms and a low degree of missing data as more reliable than those with multi-peak waveforms and a high degree of missing data.

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/wpcontent-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.

Of course, this is why this number can be increased this dramatically at catchment scale. We are fully aware that the goal of global database can not be to provide all VS for all catchments, therefore we see a value in providing tools and lessons-learned in processing the data at catchment-scale, as highly valuable data may be available. Data is also accessible sooner when new satellites are launched (e.g. S3-B,) allowing faster uptake of the data.

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.

The figure shows no data as no data could be processed before the update: i.e. due to no-data values, or too far from the DEM or very low backscatter. We will clarify in the text.

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.

We suspect it is due to too much weight being given to the next target thus over-smoothing the transition. The algorithm as we understand is developed for closed-loop mode, where you want a faster transition, which is what happens. In open-loop the fast transition already occurs and the algorithm introduces an artificial transition. We have been in contact with GPOD who confirmed that the results were as expected. The performance at this VS explains why processing options are significant at certain locations.

Thank you for the suggestion for the figure – we will redraw the "before" curves.

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

Indeed – it will be added.

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.

We will add detail about the VS considered. We noted in this context that a number of VS where erroneously left out from, so a slight renumbering is necessary. The shown height is the tracking window position – we will correct the y-axis label accordingly.

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

Actually, both options were chosen – the target was defined earlier, but for the GPOD dataset, an extension of the receiving window is still necessary.

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

This is a very good point – yes.

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.

Yes.

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?

The citation is a single example of course, we propose to increase the reference list, including by adding a paragraph in the introduction better showing this.

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.

This is a fair point and in light of other comments about the reference to the SWOT mission, we propose to instead focus the discussion on the benefit of the spatio-temporal coverage of the dual-satellite constellation for hydrological applications.

Line 430: Concerning SWOT mission, I think a better reference for the 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_D61923_SRD_Rev_B_20181113.pdf

We will correct.

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. . .

Same as above.

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.

We state it here as a concluding remark. The 70 stations are for each mission, totaling 72 S3B stations and 73 S3A stations which pass all criteria across datasets. The 169 stations are stations which pass the criteria in one or the other dataset. We will ensure that a more consistent reference to the number of stations is done in the revised manuscript.

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

In light of other comments in addition to this one, we propose rewriting this section and the conclusion in order to ensure perspectives are placed after the conclusion.