Responses to the comments on the Manuscript “ A dataset for lake level changes in the Tibetan Plateau from 2002 or 2010 to 2021 using multi-altimeter data ”

Dear editor,

The authors would like to express thanks to the anonymous reviewers for their voluntary work and the constructive comments to improve this manuscript. All of the comments are of great benefit to us. During the past few days, we did much work to revise the manuscript according to the reviewer's comments. All of the comments have been addressed. Our revisions are as follows.

07 Oct 2024

Topic editor decision: Reconsider after major revisions

by Birgit Heim

Public justification (visible to the public if the article is accepted and published):

Dear Authors and Colleagues

Thank you for your contributions. Many thanks for the revisions and thanks for the authors for the replies and the edits in your manuscript.

The manuscript, the data description and data publication do not yet fulfill the requirements of ESSD and improvements are needed in form of a major revision of the manuscript and a minor revision of the dataset publication.

Please consider the reviewers comments and technically discuss all issues, specifically concerning the comments of reviewer 3. Please also consider the critical comments from the open discussion.

Minor revision of the PANGAEA data publication: please provide a technical read me, e.g. for users of the downloaded lake level data in the current format it is not easy to understand that the two subfolders represent two different time slices, you could e.g. attach a detailed technical product guide (e.g. also including figures and tables) in pdf format. Attention: in your data files the symbols for the unit of Water level, uncertainty (WL unc) seem to be corrupt. Could you please check and correct?

After reading the reviewers comments and concerns, we also suggest to add a quality information to each lake level data product.

Reply:

Thank you for the suggestion. In the last month, we have carefully revised our manuscript in response to the reviewers' suggestions. The dataset format has been updated: we consolidated all lake time series data into a single archive, dataset.zip, organized as 361 individual entities named by lake. The previous subfolder structure has been removed for clarity.

Additionally, we included two technical readme files to help data interpretation: readme_dataset_info.kml and readme_dataset_info.md. The .kml file can be opened directly in Google Earth Pro, providing an intuitive geographic view of the dataset, while the .md file lists detailed information for each lake in the title of each corresponding file, offering a detail reference to all included data.

Report #1

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

The authors have adressed all comments and proposals. The manuscript has been revised. I recommend the manuscript should be accpeted for publication in ESSD.

Reply:

Thank you for the suggestion.

Report #2

Submitted on 16 Sep 2024
Anonymous referee #1

Anonymous during peer-review: **Yes** No

Anonymous in acknowledgements of published article: **Yes** No

Checklist for reviewers

1) Originality	Excellent Good Fair **Poor**
2) Significance	
Uniqueness	Excellent Good **Fair** Poor
Usefulness	Excellent Good **Fair** Poor
Completeness	Excellent Good **Fair** Poor
3) Presentation quality	Excellent Good Fair **Poor**
4) Data quality	Excellent Good **Fair** Poor

For final publication, the manuscript should be

accepted as is

accepted subject to **technical corrections**

accepted subject to **minor revisions** (review by editor)

reconsidered after **major revisions**

rejected

Were a revised manuscript to be sent for another round of reviews:

I would be willing to review the revised manuscript.

I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

The authors have improved the manuscript. However, there are still some obvious typographical errors. These indicate that the authors did not read the manuscript carefully. In addition, this lake level dataset is not new now.

Main comments:

1) Why was ICESat-2 not considered?

Reply:

Thank you for the suggestion. The Icesat-2 does not have enough temporal resolution (91 days), the time period covered by Icesat-2 was also covered by Sentinel-3. Additionally, we are mainly consider radar altimeter data here.

2) Table 1: Please add some new studies for comparison, especially after 2019.

Reply:

Thank you for the suggestion. We added Luo et al. (2021) in Table 1.

3) Figures 2 and 5, can these two flowcharts be combined?

Reply:

Thank you for the suggestion. Sorry for this, it would be hard to combine together. Figure 2 is only for retracking, while figure 5 is to merge the multi-satellite time-series.

Specific comments:

L10: “Tibet Plateau (TP)” to “Tibetan Plateau (TP)”

Reply:

Thank you for the suggestion. It has been revised.

L50: “Tibetan Plateau” to “TP”, and other corrections are similar.

Reply:

Thank you for the suggestion. It has been revised, and also other similar place.

Table 2: How about Sentinel-6MF? See reference: doi: 10.1016/j.asr.2024.04.006

Reply:

Thank you for the suggestion. We do not use Sentinel-6 here, the dataset here only until 2021/07. Sentinel-6 only have no more than 1 year’s data, so we donnot consider it here.

L160: ICE-1 is ICESat-1?

Reply:

Thank you for the suggestion. Actually not, it is OCOG retracker, we change it already in revised version.

Reply:

Thank you for the suggestion. It has been revised.

Figure 3: longitude to Longitude

Reply:

Thank you for the suggestion. It has been revised and improved.

Figure 4: Some texts are too small, and Water Level to Water level

Reply:

Thank you for the suggestion. It has been revised.

L290: 6364km2 to 6364 km2

Reply:

Thank you for the suggestion. It has been revised.

Figure 10: area is Level?

Reply:

Thank you for the suggestion. This is not for mentioned Level, we are describing the relative proportions based on the lake areas in the basin.

Figures: All figures need to improve, some texts are too large, and some are too small.

Reply:

Thank you for the suggestion. All the figures has been remade and impoved.

Report #3

Submitted on 04 Oct 2024
Anonymous referee #3

Anonymous during peer-review: **Yes** No

Anonymous in acknowledgements of published article: **Yes** No

Checklist for reviewers

1) Originality	Excellent Good **Fair** Poor
2) Significance	

Uniqueness	Excellent Good **Fair** Poor
Usefulness	Excellent **Good** Fair Poor
Completeness	Excellent **Good** Fair Poor
3) Presentation quality	Excellent **Good** Fair Poor
4) Data quality	Excellent **Good** Fair Poor

For final publication, the manuscript should be

accepted as is

accepted subject to **technical corrections**

accepted subject to **minor revisions** (review by editor)

reconsidered after major revisions

rejected

Were a revised manuscript to be sent for another round of reviews:

I would be willing to review the revised manuscript.

I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

The revised version of the manuscript from Chen et al. submitted to ESSD has been quite improved and replied to some of my main comment. The addition of Table 1 comparing this study to ones previously published and the comparison of their new database with the ones from Dahiti, G-REALM and Hydroweb provides more insight in the novelty and robustness of this new database. I think that this manuscript and the associated database has the potential to be published in ESSD. However, I still think it is needed to improve the manuscript considering the following general comments:

- Even if it has been clarified in the abstract, I am still bothered that the title is 'A dataset for lake level changes in the Tibetan Plateau from 2002 to 2021 using multi-altimeter data'. 46 % of the lakes in the database (167 out of 361) have a time series covering the 2010-2021 time span and not the 2002-2021 time span. I would prefer to have a title like: 'A dataset for lake level changes in the Tibetan Plateau from 2002 or 2010 to 2021 using multi-altimeter data'.

Reply:

Thank you for the suggestion. We change the title into 'A dataset for lake level changes in the Tibetan Plateau from 2002 or 2010 to 2021 using multi-altimeter data'.

- The text needs to be clarified, as some sentences are difficult to understand, especially in sections 3.1.1, 3.1.2 and 3.1.3.

Reply:

Thank you for the suggestion. We improve the text and make it easier to understand and following. See more details in revised version.

- Section 5.1 presents trends computed over 2002-2021, but given that almost half of the lakes cover only 2010-2021, it seems to me that half of the trends computed and shown on the maps are not computed on the same period. This part should be revised (for more details, see one of my comment below).

Reply:

Thank you for the suggestion. We revised the text, figures, and tables in section 5.1, by only considering the analyze between 2010–2021-time span. See the details in revised version.

- I am still not convinced about section 5.2 ('Exploring the responses of the lake levels to river regulation'), which is quite qualitative and some assertions made are assumption that would require much more work to be validated. I think this section could be safely removed.

Reply:

Thank you for the suggestion. We remove this section in revised version.

Specific comments:

In the reply to my previous following comment: 'The intermission bias is a good way also to check if the different missions are observing the same target (if one mission provide water elevations multiples decametres above/below other missions, then they are not observing the same target).' Authors' reply concerning the bias is:
'we don't think this could check whether the different missions are observing the same target. There exists a system bias between different missions.'
The authors missed my point. I know that there is a bias between different mission. This bias is dependent of the retracker used, the technical characteristics of the altimeter, the way the waveforms are selected and if multiple waveforms are retracked if the median WSE is computed or not. However, this bias should be between few cm and few meters. If the bias is >10m, then the altimeter is not observing the same targets, such bias could not be explained with the sources of bias I listed previously. It could be specifically the case between missions in closed-loop tracking mode and the ones in open-loop tracking mode.

Reply:

Thank you for the comment. We understand that a significant bias (e.g., >10 meters) likely indicates that different targets are being observed, which cannot be fully explained by typical system or retracker biases.

In our previous response, we focused on known biases that can arise from differences in retracking methods, altimeter characteristics, and waveform selection, which we expect to be within a range of a few centimeters to a few meters. However, we agree that a bias exceeding this range could indeed indicate that the altimeter is observing a different target, particularly when comparing closed-loop and open-loop tracking modes. Actually, when we doing the fusion of multi-altimeter time series, the intermission bias analyze is also been used, since we are going to only merge the time series when $\overline{Series1_{ref}} - \overline{Series2_{ini}} < 10$, for those have too much bias we believe it comes from noise or other

bright target. We also added the info in Section 3.2.

In Section 4.3, we have now added a discussion on the specific challenges related to large biases in inter-mission comparisons and the possibility of such cases indicating different observed targets.

Authors' comment to one of my previous comment stated: 'In this paper, our main goal is to generate a dataset, the different between lidar and nadir radar is not an important part'
I don't agree with this statement. Such sensor differences will make the time series accuracy quite heterogeneous in time and could help to explain why some portion(s) of the time series is(are) noisier than others. That's why in section 2.2.1 ('Multi-altimeter data'), it should be clearly stated that ICESat-1 is a lidar altimeter, contrarily to other altimeters, that are nadir radar altimeters.

Reply:

Thank you for the comment. Yes, I agree with you. Different sensor could make temporal resolution heterogeneous in different period. We improve the sentence in section 2.2.1, to better describe the different between ICEsat used here and other radar altimeters.

Authors stated: 'Due to the influence OLTC, the waveform will show without any peaks (just like the noise signal), this could be distinguished by using the data quality flag and waveform classification'
I do not fully agree with this statement. If the tracking window is not observing any continental surface, then I agree that only noise is recorded and this case is easy to handle. However, sometime the tracking window can see another lake or river and therefore have a valid waveform, but it will not observe the wanted lake. How such case is handled with your processing chain? It is a source of errors in your database that might happen for some lakes. More generally, it is needed to include (for example in section 3.2) a paragraph on potential sources of errors or inconsistency in the time series in the database.

Reply:

Thank you for the comment. I agree that, sometimes, the tracking window may primarily capture signals from other rivers or lakes. However, the waveform range covers approximately 60 meters (0.4684 * 128), meaning that if the height difference between the target lake and nearby bright features is less than 60 meters, we can still obtain accurate information, even if the OLTC DEM is not entirely correct. Additionally, to minimize the chances of capturing incorrect lake signals, we use DEM to select height values, exporting only lake levels that fall within the range of $H_{DEM} \pm 20$ m. However, this approach is not fully correct, especially in regions where neighboring water bodies are within similar elevation ranges. Such cases could introduce inconsistencies in the time series for certain lakes, particularly where OLTC DEM values have changed over time, affecting the tracking window's focus.

But the case you mentioned, I also see once in Swiss Lake. For example, Mattenalpsee has an elevation of approximately 1800 meters, while Oberaarsee is around 2300 meters—a difference of over 500 meters. When using the same track observations, due to changes in OLTC settings, the OLTC DEM value was set to 2300 meters before 2020, making it possible to monitor Oberaarsee. However, after 2020, the OLTC DEM value was adjusted to 1800 meters, enabling monitoring of Mattenalpsee instead.

We have added a new section (Section 4.3) on potential sources of error to detail possible issues, providing clearer information for readers and dataset users.

Authors' replied to one of my comment: 'Actually, the ocean tide corrections are all zero in Tibetan Plateau lakes, we mentioned it here is for making this formula more complete.'
I suggest to remove the reference to ocean tide correction or state explicitly in the text it is equal to 0, otherwise it seems strange to have this type of correction on the TP.

Reply:

Thank you for the suggestion. We remove ocean tide correction in revised version.

Table 1: Databases from Li et al. (2019) and Zhang et al. (2019) are also public (like Dahiti, G-REALM and Hydroweb), so why did not you also compare your database to this two in section 4.2?
In my previous review, I suggested to reference the database from Luo et al. (2021) who studied 221 lakes in the TP, why did not the authors consider this article and database? I think you should include it.
Reference:
Luo S., C. Song, P. Zhan, K. Liu, T. Chen, W. Li, and L. Ke (2021). Refined estimation of lake water level and storage changes on the Tibetan Plateau from ICESat/ICESat-2. Catena, 200, 105177, https://doi.org/10.1016/j.catena.2021.105177

Reply:

Thank you for the suggestion. We added the comparison between our database and Li et al. (2019) in section 4.2 and Table A1. But we realize Zhang et al. (2019) is actually not available now, so we cannot do the comparison here, to make it clear, we change the table 1, Zhang et al. (2019) from public into not public. Also, Luo et al. (2021) has been added in Table 1.

Table 2, diameter of footprint for Envisat, Jason-1/2/3 and Saral are still wrong, see Steunou et al. (2015):
'the 3-dB footprint radius is about 4 km versus 15 km for Poseidon on Jason missions'
So, the diameter footprint for Jason-1/2/3 should 30 km and diameter footprint for Saral should be 8 km. Envisat radar altimeter diameter footprint should be similar to the ones for Jason, but I let the authors do their research for the value. For Sentinel-3A and Crysoat-2, where does the along and across track footprint diameter come from? According to https://sentiwiki.copernicus.eu/web/s3-altimetry-instruments, Sentinbel-3A/B radar altimeter 'Radar footprint diameter (LRM/P-LRM)' is ~15km, whereas 'Along-track footprint (SAR mode)' is ~330m. Please edit Table 2 'Diameter of footprint (km)' accordingly.
Reference:
Steunou N., J.-D. Desjonqueres, N. Picot, P. Sengenes, J. Noubel, and J.C. Poisson (2015). AltiKa altimeter: instrument description and in flight performance. Mar. Geodesy 38 (sup1), 22–42. http://dx.doi.org/10.1080/01490419.2014.988835

Reply:

Thank you for the suggestion.

LRM/P-LRM footprint diameter is $D = 2h * \tan(\frac{\theta_{3db}}{2})$

SAR/SARin footprint width across-track direction: $D = 2 * \sqrt{h \cdot \frac{c}{B}}$

SAR/SARin footprint width along-track direction: $\Delta x = h\frac{\lambda}{2 \cdot N \cdot v}PRF$

Envisat antenna beamwidth at -3 db is around 1.5 °, resulting $D_{Envisat} \approx 20\ km$.

Cryosat-2 $\lambda = 0.021m,\ PRF = 18181Hz,$

resulting $D_{Cryosat2} \approx 1.65km,\ \Delta x_{Cryosat2} = 0.3km$

Sentinel-3 $\lambda = 0.0221m,\ PRF = 17825Hz,$

resulting $D_{Sentinel3} \approx 1.75km,\ \Delta x_{Sentinel3} = 0.33km$

To distinguish the different between SAR and LRM/P-LRM. We added a table notes in revised version. "the footprint for SAR/SARin can be approximated by a rectangle given with the footprint width in across track and along track". And change the Diameter of footprint into width of footprint.

Reference:

1. https://earth.esa.int/eogateway/documents/20142/37627/CryoSat-Footprints-ESA-Aresys.pdf
2. https://earth.esa.int/eogateway/documents/20142/37627/ENVISAT-RA-2-MWR-Product-Handbook.pdf
3. https://sentinel.esa.int/documents/247904/4871083/Sentinel-3+SRAL+Land+User+Handbook+V1.1.pdf

l.135: 'the implementation of retracking correction', why do you write 'retracking correction'? It should be the retracking of the waveform, rather than a correction, should not it?

Reply:

Thank you for the suggestion. We revised it in revised version.

l.134: 'due to the potential interference or submergence of waveforms by signals from adjacent land areas' → this sentence is not clear, please rephrase.

Reply:

Thank you for the suggestion. We rephrased it into "due to the potential interference or submergence of water signals by those from adjacent land areas".

l.138: 'can get good results (Chen and Liao, 2020)' → 'good results' is quite qualitative. You should provide some metrics from Chen and Liao (2020) to provide more quantitative information to this statement.

Reply:

Thank you for the suggestion. We revised this sentence into: "In this study, we employed the automatic multiscale-based peak detection retracker (AMPDR) (Chen et al., 2021). The Jason-2/3, Sentinel-3A/B, and CryoSat-2 satellites are all suitable for providing precise measurements, with average accuracies of 0.18 m, 0.14 m, and 0.15 m when compared to gauges, respectively."

Figure 2 is not always easy to read. For example, what does 'For each track, determine the multiscale-based retracking level' or 'Determine the abnormal track by the DEM and the water level derived from the neighbouring cycle' mean? Some line does not have arrow, so it's difficult to know how these boxes are used in the flowchart.

Reply:

Thank you for the suggestion. The figure 2 has been improved, and "For each track, determine the multiscale-based retracking level" revised into "For each track, determine the multiscale-based retracking level from sub-waveform using threshold retracker.", "Determine the abnormal track by the DEM and the water level derived from the neighbouring cycle" revised into "Determine the abnormal track by the range of DEM ±20m and the water level derived from the neighboring cycle". The details info can be seen in next text within "Following this, a second run of AMPDR was performed to retrack abnormal tracks identified by checking if the current cycle's water level fell within the range of the Digital Elevation Model (DEM) ± 20 m and by comparing it with water levels from neighboring cycles, particularly when a significant discrepancy or abrupt change was detected in current cycles."

l.155 to l.163: Description of the procedure (and Figure 2) is not clear and needs to be rewritten. Why are you using also ICE-1 retracker in addition to the AMPDR? The benefits of using AMPDR instead of Ice-1 should be provided. The use of the DEM is not clear to me either.

Reply:

Thank you for the suggestion. The ICE-1 retracker is not used directly for retracking, it is actually to provide a possible water level into construction the point cloud of AMPDR retracker, this is because the multiscale-based adaptive threshold value would fail in some situations, such as sometimes, the signal from LRM mode altimeter is very noise, the adaptive threshold value may find too many noise which make it hard to create a good point-cloud. We revised this sentence to make it clear. "Additionally, a retracking point from the OCOG algorithm was incorporated into the AMPDR to assist in constructing the "point cloud" and CDF. This integration addresses specific cases where AMPDR's adaptive thresholding may encounter challenges."

l.169-170: 'Due to the use of a 1 km buffer to pick out the shape of the available footprints, there would be many noise footprints caused by the reflected signals of the terrain or by the scatter signals of the off-nadir points' You should define what is a 'noise footprint'. I don't understand what you mean, as a footprint is the observed area on the ground, it's not noisy by itself. A waveform could be noisy, but you should also define what you mean by noise. Multiple targets could send back energy to the satellite, which will be recorded in the waveform. These cases are difficult to retrack. This section describes how waveforms are selected within the polygon of the lake, whereas some other that are more difficult to process are excluded. So, it seems to me this section should be more labelled 'waveforms selection' or something like that.

Reply:

Thank you for the suggestion. This sentence maybe not clear enough. We are not picking up the footprints, we are selecting the observations available on the shape (with 1 km buffer), this could increase the number of observation, but also introduce uncertainty, the waveform of some observations could be noisy (hard to find the leading edge), which could be caused by the signals of terrain or complex signals of off-nadir observations.

We revised this sentence into "By selecting observations within a 1 km buffer around the lake shape, we capture additional data points. However, this approach can also introduce uncertainty, as some observations may contain noisy waveforms that complicate the retracking process. This noise can result from signals reflected off surrounding terrain or from off-nadir observations."

The section has been revised into ‘waveforms selection’.

In equation 2 and 3, the ‘i’ and ‘j’ indices and the variable ‘z’ must be defined.

Reply:

Thank you for the suggestion. t_i is the time of the i-th time step, j is the number of observations in given time, and z_i is a random noise term following a standard normal distribution. The definition has been added in revised version.

l.193: ‘According to the Laplace estimation, the mean value of the range was selected to represent the water level of the lake for each cycle’ → What is the ‘Laplace estimation’? Why does it imply to select the mean and not something else (for example the median)?

Reply:

Thank you for the suggestion. According to the process model and observation model in Eq.2 and Eq.3. The likelihood function of the true water level is non-Gaussian, so the Laplace approximation is used in “tsHydro”. More details can be seen in Nielsen et al. (2015).

But to make it clearer, we revised this sentence and remove Laplace estimation, which is not main point in the paper. The predictions of the true heights $\widehat{H}^{(true)}$ is the estimate of the water level of the lake for each cycle.

As there are some lakes with time series starting in 2002 and some other starting in 2010, on which period have the trends provided in the text and figures in section 5.1 been computed? If it varies depending of the time series, then it might be difficult to compare trends not computed on the same period. I think figure 8 to 10 and Tables 5 and 6 should be computed over 2010-2021, and other figures and tables should be added only for lakes with time series really covering the 2002-2021 time span. Besides, the equation and method to compute the linear trend should be provided. It should also be added if the trends are statistically significant and you should only consider lakes with statistically significant trends. For the unit of the trend, why using ‘m/a’ and not, for example ‘m/y’ (i.e. for meter/year) which seems to be more explicit to me?

Reply:

Thank you for the suggestion. We revised the text, figures, and tables in section 5.1, by emphasize considering the analyze between 2010–2021-time span, while for the period over 2002-2021, we added the summarize text, and also the Basin change info in table 5. See the details in revised version.

The equation and method to compute the linear trend was added in the beginning of Section 5.1.

The unit of the trend has been changed into ‘m/y’ in revised version.

Figure 8: I find it difficult to distinguish the different size of the arrows. It might be easier to have four different symbols for the four different ranges of trend.

Reply:

Thank you for the suggestion. The figure 8 has been improved by changing the different symbol.

Table 5: Does the ‘Annual rate of change’ correspond to the mean of all trends within the

considered lake area range? For this annual rate and the mean rate of rise/decrease, is the lake trend weighted in some way by the lake size or is it the arithmetic mean?

Reply:

Thank you for the suggestion. It is actually weighted by lake size. To make it clear, we added the info in revised version.

l.384: Why are you mentioning a 'modified waveform retracker'? You used the published retracker from Chen et al. (2021), did not you? Did you modified the retracker compared to Chen et al. (2021)?

Reply:

Thank you for the suggestion. We are using AMPDR from Chen et al. (2021), but when processing with Jason-1, Envisat, and SARAL, AMPDR could retrack fail in some cycles, this time we are going to do the second run of retracking, this can be seen more details in section 3.1.1. But you are right, in conclusion, using 'modified waveform retracker' is not clear, we changed it into "two-step AMPDR retracker".