Comment on tc-2022-132

Anonymous Referee #1

Referee comment on "Ice thickness and water level estimation for ice-covered lakes with satellite altimetry waveforms and backscattering coefficients" by Xingdong Li et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-132-RC1, 2022

Ice thickness and water level estimation for ice-covered lakes with satellite altimetry waveforms and backscattering coefficients.

by Xingdong Li, Di Long, Yanhong Cui, Tingxi Liu, Jing Lu, Mohamed A. Hamouda, and

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The paper titled "Ice thickness and water level estimation for ice-covered lakes with

satellite altimetry waveforms and backscattering coefficients" by lead author Xingdong Li and co-authors explored radar altimetry data to infer lake ice thickness. By conducting the study over 6 lakes, the authors improved existing methods of ice thickness estimation using the radar altimetry and reported improved accuracy of their retrievals by comparing altimetry-derived ice thickness estimation and those from the in situ gauge records and from the modelling. The authors have done an considerable efforts of describing the details and nuances of the radar signals scattering within the ice and how it matches the assumptions they applied in the modified methods.

My major concern with the paper is the lack of clarity and consistency in presentation of the method and results. While the theory of the methods were well described, the implementation routine sometimes is unclear.

The supplementary material provides an excessive theoretical information and can be reduced to several lines describing how exactly the inter-satellite bias was calculated (along-track point-by-point approach or on the cycle basis, the length of the tandem mission phase for each tandem, and the obtained bias for each tandem - average or median bias plus the standard deviation or the range. This information is crucial for evaluation of the results. The authors claims several times in the texts that the presented method does not take the in situ observations, while in the Methods section the calibration of several parameters of equations was mentioned. More information on this calibration is required (period of calibration, values of calibrated parameters, uncertainties).
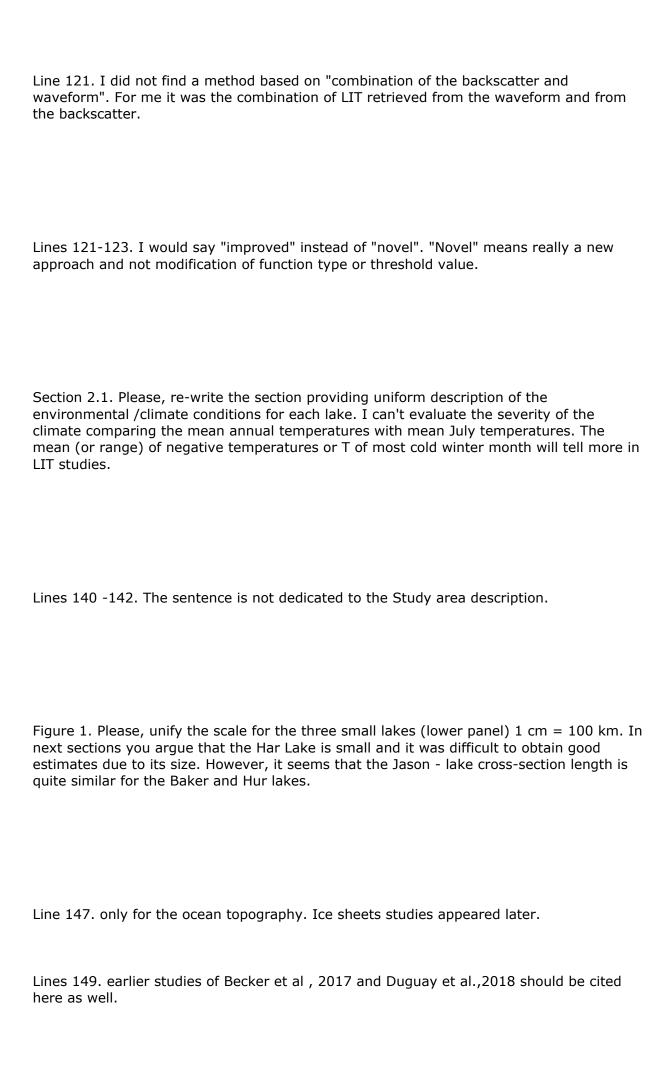
Please see below my comments. I would recommend the authors provide less theory and more implementation techniques in the Methods section; better prove the findings providing consistent (the same) set of statistics across the text in the tables for each lake; reduce the Supplementary Materials to several lines and introduce them into the main text.

Overall, I think the paper needs a major revision.

General comments.

For lake surface height retrieving the authors tested two thresholds and removed the systematic bias between the heights obtained using these thresholds and calculated for the open water period. Then, finally, they found that the winter water heights obtained with 0.1 threshold are lower than the heights obtained with the 0.5 threshold (fig.6), which contradicts with the theory well illustrated in the Figure 3a. The explications provided in the Discussion section 5.1 are quite interesting. However, to be convinced I would like to see an example of quantitative evaluation for one case: compressed pulse length/gate width/LIT/snow depth/ values of open water bias etc.

I would expect that the summer height bias due to the thresholds applied is the value variable from cycle to cycle as it depends on the leading edge width, which is the proxy of the surface roughness (wave height). The leading edge width (LEW) of a specular waveform sampled during calm water conditions may be similar to the LEW of a waveform over the 20-40 cm ice with some snow cover. The open-water bias should be deduced from these open-water specular waveforms. Otherwise, the solid proofs are necessary (seasonal plots, or the bias value for waveforms of different peakiness, for example). And again, the tables of estimates with sets of descriptive statistics will help to follow and understand the authors' logic.

The subsection Uncertainties and Limitations is only qualitative and somewhat naive: a short paragraph on what the authors expect from the snow effect on the waveform and consequently on the LIT retrievals will be beneficial for the subsection.

Specific comments.

Line 83. Why the approach developed in Becker et al., 2017 for SAR waveforms is not compatible with the conventional altimetry waveforms?. SAR waveform is more specular, however the approach remains the same: retracking of the sub-waveforms.

Line 107. The word "paradox" is not good here. "Discrepancy? "

Line 121. I did not find a method based on "combination of the backscatter and waveform". For me it was the combination of LIT retrieved from the waveform and from the backscatter.

Lines 121-123. I would say "improved" instead of "novel". "Novel" means really a new approach and not modification of function type or threshold value.

Section 2.1. Please, re-write the section providing uniform description of the environmental /climate conditions for each lake. I can't evaluate the severity of the climate comparing the mean annual temperatures with mean July temperatures. The mean (or range) of negative temperatures or T of most cold winter month will tell more in LIT studies.

Lines 140 -142. The sentence is not dedicated to the Study area description.

Figure 1. Please, unify the scale for the three small lakes (lower panel) 1 cm = 100 km. In next sections you argue that the Har Lake is small and it was difficult to obtain good estimates due to its size. However, it seems that the Jason - lake cross-section length is quite similar for the Baker and Hur lakes.

Line 147. only for the ocean topography. Ice sheets studies appeared later.

Lines 149. earlier studies of Becker et al , 2017 and Duguay et al.,2018 should be cited here as well.

Lines 160-162. SGDR Jason product contains as well the range retrieved with Ice (OCOG) retracker widely used in inland water studies. (see S. Calmant, F.Frappart; S.Biancamaria etc. articles).

Lines 169-170 . please, check the prepositions in the sentences. Move the information on location of LIT in situ stations into the Table 1. In the Table 1, provide consistent with LIT station coordinates (in decimals). In this lines it would be better to mention that the in situ LIT is observed near the coast, where the ice growth and snow-on-ice conditions can differ from open area of the lakes (especially GSL). See different studies of C.Duguay team for details.

Table 1. Please summarise here all data used for validation, water level, LIT gauges, model simulation.

Line 185. Please, give here 2-3 lines about 1-D lake ice model developed by Li et al.,(2022). What does it mean "remote sensing" for this model? What is the accuracy of the LIT simulations for the lakes selected for this study ?

Line 217. Better to speak "delay-Doppler" or "SAR" altimetry when meaning SC2 or Sentinel-3.

SAR altimetry can be seen as the beam-limited only in along-track direction.

Line 220. and in other places : replace wave for waveform.

Line 240. Check c and c_i in formulations; provide the reference on c in the ice.

Line 253. Delete "quite"

Line 269. Delete "A possible reason". This is the main reason. Provide a reference.

Line 272. replace "increases(decreases)" on " is high(low)"

Line 280. Explain STD, variability in spatial domain?

Line 285. I am not agree. The Sig0 rise during this phase is due to water-on-ice appearance or decreasing of the penetration depth (volume scattering) caused by high water content of the melting snow, unless you prove your statement with the solid references.

Line 320. "I_1 does not change with LIT" is also only the assumption. If I understood I_1 is the surface echo plus snow volume echo. It can change during the winter due to the changes in the snow (densification, redistribution) and ice (thermal or mechanical deformation resulting to changes in ice surface roughness).

Equation 8. Explain, please, why in the Eq2 "-2kHi" appeared.

Equation 10. Insert space between equations

Line 361 replace the word "paradox"

Line 369. Please, rephrase the sentence in more scientific manner. Phrase "investigate LSH for ice-covered lakes" is scientific slang.

Line 374. All LSH **retrievals**...

Lines 377-381 These lines describe the results. Move them to the Result section. See my general comment dedicated to the evaluation of the open-water altimetric range bias obtained with 0.1 and 0.5 thresholds.

Lines 383. What does it mean "more robust performance"? Please, illustrate with the statistics comparing with corresponding reference time series. Remind in the text what Davis (1997) investigated: inland waters, which retracker (OCOG) ?.

Lines 385-387. Not clear how it was implemented: by concatenation of [$LSH0.5_{openwater}$; ($LSH0.1_{ice}$ - bias)] ?

Line 423. The subsection 4.1 was also based on altimetric measurements. Change the title of the subsection 4.2 for more specific.

Line 424. Please, provide here the uncertainties found in the cited study. Not clear why you did not compare your waveform-based retrievals with the in situ observations. Did you use exactly the same areas, same tracks, same codes for selection of the sub-waveforms as in the Li et al., 2022 or you used LIT provided by these authors ?

Lines 426-430. Summarise the statistics for each lake in a Table. Be consistent providing coefficients of correlation or determination.

Line 432. Why your method (backscatter-based in logarithmic approximation ???) does not depend on availability of in situ observations. If I understood from the lines 339-351 you calibrated the parameters K, A and C ?.

Section 4.2 In the Figure 3 only the equation for GSL was provided. Please, in the section 4.2 give the table with 1) the K, A, C parameters for each lake, 2) with the period used for calibration, 3) period used for validation, 4) accuracy of the LIT for validation period (RMSE, correlation coef.).

Lines 440-445. Some reasoning based on demonstrations via figures or tables of statistics is necessary to prove why waveform-retrieved LIT was used for thick ice, while the backscatter-based LIT retrievals were used for thin ice range.

Line 453. For me, the track length within lakes Baker and Har looks the same (Figure 1), so the reason given in the text is not strong.

Figure 9. For simulated LIT+SnowDepth provide the line as well. The gray shadow-black line difference is not visible.

Line 466. What the "effective water level" is? Hydrostatic water level or water-ice interface?

Line 468. improvement comparing to what?. I prefer to see RMSE for each lake compared to the RMSE of "what this improvement refers to". Please, give the metrics found in Yang. etal 2022.

Figure 10. and everywhere. STD and RMSE statistics should be round to centimetre. The accuracy of the altimetry height retrievals over the inland water objects is still from several centimetres - to several tens of centimetres.

Lines 480-485. For me, the variability of the RMSE and STD between the lakes is low. Moreover, the STD is almost equal to RMSE, so keep only the RMSE. The explication of observed inter-lake uncertainties provided in this paragraph is unrealistic. The text does not correspond to the figure.

For the Discussion Section see my general comments.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2022-132/tc-2022-132-RC1-supplement.pdf