The Cryosphere

Discussions

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Interactive comment on "River ice phenology and thickness from satellite altimetry. Potential for ice bridge road operation" *by* Elena Zakharova et al.

Anonymous Referee #1

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The paper titled "River ice phenology and thickness from satellite altimetry. Potential for ice bridge road operation" by lead author Elena Zakharova and coauthors explored using radar altimetry data to infer river ice phenology and ice thickness. By conducting the study over the lower Ob river in Russia, the authors reported accurate retrieval of river ice phenology and ice thickness by comparing ice phenology/thickness estimation from altimetry data at virtual stations to those obtained manually and those from the in situ gauge records. The authors have done an excellent job of describing the details and nuances of the ice processes, and how it can complicate the radar backscatter signals. The authors thoroughly described the uncertainties of the studies and provided valuable recommendations for future work and an assessment of the social impact of the conducted research.

My major concerns with the paper is the lack of clarity in the methods section. The authors reported many interesting results however, as I detailed below, not all of their methods were well described. Please see below for my comments. I would recommend the authors make the methods clearer and make the figures more informative and easier to read. Overall, I think the paper is well written and the implication and uncertainty of the study thoroughly discussed.

Major comments

Figure 4: are the dates in the format of dd-mm? I suggest to make the dates more explicit and move the surface types to a more prominent places (e.g. using a.b.c and refer to the surface type in the caption)

On line 373: the authors argue that decrease in backscatter is proportional to gain in ice thickness. If this is the argument, would it make sense to plot the changes in ice thickness (Hice) against cumsum(dsig0/dt)?

On line 375: the authors mention cumsum(dSig0/dt), which should be negative for the freeze-up period. However, in Figure 5b all the values are positive along the x-axis. Am I missing something here?

Lines 387–393: calibration and validation using the eight VSs were mentioned in this paragraph, however, no detailed methods were provided in text nor in figure 6. I would highly recommend providing how the calibration and validation were carried out.

Figure 5: labels for the subfigures should be placed at more prominent locations. The legends should be placed at a consistent location of the figures.

Figure 6: it is nice to have a flowchart to guide the readers through the processing steps. However, I found the one presented here hard to follow: data and procedure are better separated and represented using different boxes.

Line 400: shouldn't phenology estimations be compared to gauge records closest to the VSs?

Line 413: please clarify how "close to zero" was defined.

Figure 7: the author should discuss why for melt end, the results from the manual algorithm have a much bigger bias than that from the automated algorithm.

Figure 8, 9, and 10: the authors need to justify why for the gauge data the mean was used and for the VS data the median.

Line 444: "significant variability"âĂŤdoes this refer to the difference between the manually determined and the gauge mean, or does it refer to the variability amongst the gauge data. Need clarification.

Line 440–451: it is easy to attribute years lacking north-south detected difference to local effect. However, such explanation is not satisfying without any evidence backing up the claim, especially given that so many factors (e.g. uncertainties in the percentage of pixels of different surface features) can affect the detected dates.

Figure 10 and 11: the authors need to clarify or show the location of Tr187 in the x-axis label.

Figure 11: highly recommend the authors using color to represent data from different years or use a better way to differentiate the data.

Lines 494–500: the active melting period (melt end) is highly dynamic and presents a challenge, as the authors noted, to any automated algorithm. I think the patterns presented in Figure 11b–c and the interpretation given in the paper is very interesting. However, it will make a much stronger argument if similar patterns contrasting the similarity at the melt start and variability at the melt end can be found in the in situ gauge data.

Figure 12: it is rather hard to see the rivers when everything is frozen. I suggest the authors add some labels on the images for a few key locations discussed in the paper to orient the readers.

Lines 507–508: the authors need to clarify whether the correlation and RMSE were calculated based on the gauge that was left out of the parameter estimation step.

Lines 573–576: the authors mentioned both approaches of building relations were evaluated at 11 VSs nearest to gauges. However, only two sets of values were presented for VS 109 and 12. I think it will be really helpful for the authors to explain Table 2 in detail since it is pivotal to the understanding of the algorithm performance.

Line 592: please explain the term "ridging flags"

Figure 14: instead of using decimal year in x-axis labels, it will help readability by converting it to month.

Line 613: could optical remote sensing provide information on the ice onset on rivers?

The authors should add scale bar and north arrow for all the maps presented in the paper.

Figures in the paper are of various styles and should be made in a consistent style with consistent places for subfigure labels and the legend

Minor comments

Line 33: "erosion of channels and banks": need citation here.

Line 38: "catastrophic flooding": need citation here.

Line 75: "state and regime" is there a difference between the two?

The paper needs some language editing.

Line 126: proposes should be "proposed"

Line 198: delete "in" before later freezing, and "in the" thinning of the ice cover.

Many cases of unnecessary "an" and "the" (e.g. Line 187: "The Matplotlib Basemap Toolkit"; Line 573: delete the "the" before "both approaches"; Line 18: for "an" estima-

tion (should be "the");

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