We appreciate the Referees' valuable remarks and recommendations and carefully addressed them in the new version of the manuscript. Our answers can be found in the Supplement materials.

On behalf of all authors,
Elena Zakharova

Our answers below are marked ">>"

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
Received and published: 4 February 2021
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.

>> The manuscript was significantly edited according to recommendations of Referees. The section of Methods was extended. The sections Results and Discussion were reshaped. All figures were revised. Several figures were removed or combined after revision of corresponding paragraphs as recommended by Referee 2.

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)
>> Figure 4 is redone.

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)?
>> Yes, the relationship Hice vs CumSum(dsig0/dt) is shown in the Figure 5b.

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?
>> Yes, the cumsum is negative. The X-axis title should be abs(CumSum(dSig0/dt)). The Fig.5b was removed from the text as it was considered to be redundant by the Referee 2.

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.
>> We added a phrase describing our calibration approach and moved part of text from the Results Section to the Methods to facilitate the reading.

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 5 is redone.

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.
>>Figure 6 is redone.

Line 400: shouldn't phenology estimations be compared to gauge records closest to the VSs?
>> Yes, we did exactly this validation. Necessary details are provided in the new version of manuscript.

Line 413: please clarify how "close to zero" was defined.
>> The phrase was corrected for "In 56% of the cases this difference is equal to zero".

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.
>> The algorithm was developed for detection of the melt start. The manual implementation of the algorithm is more accurate than automated implementation (what we can naturally expect during an algorithm development, as not all Sig0 behaviour cases can be coded correctly). As the manual implementation tuned for the melt start detection, logically, it should be not good for the melt end detection. Probably, the dedicated paragraph was not clear. This paragraph was re-written. "Comparing the dates of altimetry-derived melt onset with the ice state records provided by gauging stations, we conclude that manual implementation of our algorithm detects well the start of ice thermal degradation. In 88% of the cases, the difference between manually-retrieved melt dates and in situ observations of first water appearance is less than ±10 days (Figure 7, b). The automated melt date retrievals demonstrate worth accuracy for detection of the melt start, comparing to the manual ones. Only in 54 % of the cases the difference with in situ melt start observations is less than ±10 days. The automated approach is more efficient for the detection of the melt end as ±10 days accuracy was reached in 67% of cases (Figure 7 b,c)."

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.
>> We were not specific enough – the mean did not mean "arithmetic mean". Changed to "median".

Line 444: “significant variability”âˇAˇ Tdoes 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.
>> Thank you for catching this imprecision. We meant the difference between satellite and in situ observations. The text is corrected for " Significant difference between gauging and virtual stations (order of 20 days) is observed only for melt start dates in 2014."

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.
>> We agree that the uncertainties in estimation of the ice phenology dates from altimetric measurements can also be a reason of lacking of the North-south gradient for certain years. As it was recommended by the Reviewer 2, we removed these results from the manuscript as this question would take a separate subsection to address all remarks and will result in manuscript extension.

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

Figure 11: highly recommend the authors using color to represent data from different years or use a better way to differentiate the data.
>> The figure was deleted as in the new version of the manuscript the phrases referred to the figure were removed.

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.
>> On the secondary branches, there is only one gauging station. We could not assess the big/small channel ice phenology difference from the in situ observations. By discussing this difference observed in our altimetry retrievals we wanted to demonstrate the value of the remote sensing methods. As the topic stirred a lot of remarks and questions and their addressing would extend the manuscript, we decided to delete it from the new version of the manuscript. The theme of spatial variability of ice phenology dates will be a subject of a new more detailed study based on multi-sensor approach with more solid proof base.

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.
>> The figure is no more present in the new version of the manuscript as there is no more reference in the text.

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.
>> We added necessary details in the text in the Method Section. When designing the Cal/Val exercise, we initially separated our data on the Cal/Val periods (1:1 split) and found that the

results of validation depend on selection of the Cal/Val years. We consider that the use of the leave-one-year-out method allows to avoid the effect of subjectivity, when separating the short time series (10 winters in our case) into Cal/Val sets. Calibrated by this method parameters, calculated as average from parameters received from 9 leave-one-year-out fitting runs, better account for interannual variability. Using this method, the uncertainties can be estimated for all period and not for one left-out year.

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.
>> The table 2 was edited. The title was changed, the pairs of VS-gauging stations were grouped for better visual representation, the additional pair of stations used for intermediate tests was removed resulting in 10 pairs (one gauging station surrounded by 2 virtual stations).

Line 592: please explain the term "ridging flags"
>> we referred to quantitative records describing the quality of the ice near the gauging station. The "flags" was replaced by "events".

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

Line 613: could optical remote sensing provide information on the ice onset on rivers?
>> Yes. This was demonstrated by several works cited in the Introduction. The problem of the optical images is the cloudiness.

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

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
>> Figures were redrawn.

Minor comments

Line 33: "erosion of channels and banks": need citation here.
>> The reference on Ettema (2002) was added
Line 38: "catastrophic flooding": need citation here.
>> The reference on Beltaos et al., (2013) was added
Line 75: "state and regime" is there a difference between the two?
>> We mean solid or liquid state (is the water frozen or not), which is different from the regime (temporal variability). The phrase was changed "... used for observation of the water state (solid/liquid) and regime.."
The paper needs some language editing.
>> We carefully edited the language in the new version of the manuscript.
Line 126: proposes should be "proposed"
>>Done
Line 198: delete "in" before later freezing, and "in the" thinning of the ice cover.

>> Done

Many cases of unnecessary “an” and “the”

(e.g. Line 187: “The Matplotlib Basemap Toolkit”

>>No longer relevant. Other source was used.

; Line 573: delete the “the” before “both approaches”;

>> Done

Line 18: for “an” estimation (should be “the”);

>>Done