

General comment

The authors have done substantial revisions and improved the quality of presentation (both text and figures). However, I still have a lot of concerns and suggestions. As for the majority of my questions the authors just removed a lot of text and figures from the manuscript, I feel that we should have another detailed look into what is left.

I provide here the structure of the paper for an overview:

1. Introduction
2. Regional setup and data
 - 2.1. Study Region
 - 2.2. Data
 - 2.2.1. In situ data
 - 2.2.2. Altimetry data
3. Temporal variability of radar altimetry signal over frozen rivers
 - 3.1. Backscatter variability
 - 3.2. Waveform changes
4. Methods
 - 4.1. Ice onset and break up algorithm
 - 4.2. Ice thickness algorithm
5. Results
 - 5.1. Ice phenology algorithm verification
 - 5.2. Ice thickness retrievals
 - 5.3. Ice thickness estimation for the entire studied river reach
 - 5.4. Winter ice bridge roads operation forecast
6. Discussion
 - 6.1. Factors affecting ice thickness retrievals from altimetry
 - 6.2. Potential improvement of algorithms
7. Conclusion

In the **Introduction** I still miss a thorough description of the knowledge gap which authors fill with your study. It is clear that river ice needs monitoring, that the remote sensing is the great tool for that, and that authors saw and implemented a good potential of the altimetry backscatter. I would like to see more details on the altimetry principles as well as some review on the existing studies using altimetry for the fresh ice monitoring. From this authors can draw the knowledge gap and the objectives. Also, for example, authors mention some SAR studies on the ice thickness (there are also many on the ice phenology which I think you should mention too) but do not provide any drawbacks of them, i.e. why do we need to use the altimetry at all, if we have SAR?

(See also my comments for the first review).

Regional setup and data: I would split the section into two sections, to avoid numbering of the third order (2.2.1 etc).

Figure 2b: I actually see some trend for the maximum ice thickness.

Consider adding similar graphs for four other stations in a supplementary figure.

The whole section **Temporal variability of radar altimetry signal over frozen rivers** is a mix of own results from this study and some discussion of the previous studies. I understand that authors first investigate your data and then build an algorithm based on own findings, previous studies, and

known facts. But I think it should be possible to find a way for rigorous presentation of own results and their discussion with respect to other studies. In general, there are a lot of speculations in this section which are given without any references or proofs.

Here I also would repeat my suggestion to use a typical (or atypical) backscatter cycle over one year as an example, and to illustrate all ice events and corresponding backscatter changes within one year. Authors added color in Figure 3 for the ice cover period, this is already helpful but still it is difficult to follow references to this figure in the text when authors describe the seasonal changes. This also includes Methods. Also, please add the cycle of TB on such graph.

Results

I could follow the results until the attempt to validate the 2D product. Do I understand correctly that:

- authors retrieve the ice thickness at all VS using the relationship between backscatter and in situ ice thickness
- then interpolate the ice thickness from the VS to the entire river
- then extract again the ice thickness at some VS (why those?) from the interpolated product
- and then compare it with the in situ ice thickness?

Sounds like a lot of data juggling here, especially considering interpolating and smoothing. I understand the intention to validate the 2D product but I am not sure that it is possible to achieve here. Why not to extract the ice thickness exactly at the location of the gauging stations?

Also, there are some discussion and speculations in this part of the results which should be moved to the Discussion section. In the Discussion section I still miss some discussions on the place of this study in the context of the other river ice studies, be that in situ observations or other remote sensing techniques, other Arctic rivers, or maybe even some connection to the lake ice studies. The Discussion in the form as it is now would fit to a purely methodological paper but this one is a combination of methodology and scientific results. And that is of course then hard to fit into a common paper structure too. I think that after my questions and suggestions, authors would need to make some amendments to the Discussion and Conclusions parts, as well as to the Abstract. Therefore, I leave it for the next round.

Regarding the style, I noticed the following issues:

- choice and mixing of tenses. Past or present, be consistent.
- missing words
- order of the words in sentences
- wrong or missing prepositions
- typos
- spaces
- lower case instead of indices
- dates format is inconsistent
- writing out versus spelling out numbers
- in situ or in-situ, italic or not?

I can see that the professional proofreading was not accomplished and would like to see that done for the next round of the revisions.

Specific comments:

41: icy conditions – do you mean “ice conditions”?

42: “...for people who are required...” – seems to me redundant in the sentence

51: please add that clouds are limiting factors for the optical sensors, not for the river monitoring in general

58: please check the order of the words in the sentence

81: extends approximately... a preposition is missing?

84: reference to the Figure 1 is odd here – move it to a more general description of the study area.

Figure 1: when I proposed to add color to the Figure 1, I mainly meant the overview part from the first version. I think the black and white zoom-in figure looks better. Just add colors to the gauging station symbols, the main cities, and the new overview map to the previous version. Sorry for the confusion.

101: something is missing between “stations” and “water”

113: order of words

114: “installation of ice cover” sounds odd to me

123: ice onset and melt **date**

145-147: you mention studies for the ice thickness retrievals but you use AMR for the ice phenology. Please clarify.

150: I think you can remove “Jason-2 and -3”, as you do not introduce Jason-3 at this point.

154: difference (bias) – why do you need the word “bias” in parenthesis here? Again, is it the difference or the bias?

157: can you provide a more recent access date?

159: what is ICE1 algorithm? Any references? Please provide a short explanation how is the backscatter coefficient defined and retrieved?

163: it is not the Python code which overlaps Jason measurements?

166: “...the stations names were extended” – “the names of the stations located on the secondary branch were extended...”

178: “installation of ice cover” – ice does not install, please use another word.

180: what do you mean with the word “intercepts”?

193: please introduce Sig0, see also my comment to the line 159

193: Δt is the time period between two consecutive observations? Please mention.

Figure 3: the vertical lines indicating the new year are not visible. Open water line in the legend is also barely visible, especially when printed.

You decided to show the backscatter time series for these 2 stations based on their location (north and south), correct? Please mention it in the caption.

How did you decide where to start and finish the red line, i.e. the ice cover period? Is it based on the in situ observations? Or these are the results of your algorithm implementation? Please mention.

226: I do not see what do you call an intermediate peak in Figure 4. Please indicate it on the figure.

248: "Freezing on the floodplain and banks" – do you mean freezing of the land surface, i.e. soil or sand? Please reformulate.

252: I think it is the other way around – the backscatter increase marks the ice decay.

268: "we used a relative backscatter decrease..." – add for what exactly

278-280: and then what dates did you use for the ice thickness estimation period? The best of automated vs manual?

284: power function, not equation. In the equation, you have H_{ice_alti} in the left part – should it be H in situ? Please use subscript instead of lowercase.

You decided to exclude the scatterplot but I think it is important to show it. Please show all gauging stations (and corresponding virtual stations) with different symbols or colors as well as the power fits. Alternatively, one set of all stations together, with one fit. We can decide later whether to include it or not.

287: "gauging VSs": "-is missing?"

289: why do you use "mean" and "average" interchangeably?

291: "thickness" is missing

321: please explain in the text of the paper why automated algorithm was better for the melt end date retrieval compared to the manual approach.

Figure 6: if you show the histogram, should the y-axis be called pdf? I am not sure, please check and correct if needed. "Freeze up" lost "e" in the title, and also the legend. You can explain M and A in the legend instead of the caption. In general, the quality of this figure is not good enough, please work on it (font size, visibility, etc).

329: If I understood correctly, for the ice phenology you do not train anything - the 10 VS are just for the validation of the retrieved dates.

336-337 and Figure 7: as you show and describe the results of the manual retrieval, why do you refer to the algorithm here? Please also add why you do not provide the graph for the melt end (poor results?).

Do I understand correctly that you show that the manual approach works, in general, better, and for the ice thickness retrievals you use the manually retrieved dates? Please make it clear in the text.

339-344: I think you do not need to explain the legend in the caption, it should be clear enough. The information on what stations are in (4 gauging, 20 virtual) is important. The max-min red lines on b) are not visible.

345-350: I think, we, in general, see the decrease of the accuracy from the north to the south? Interesting! But it may also be a result of the proximity of the VS to the gauging station: Pitlar station is the closest to its VS. Include the distance to the gauging station for each of the VS in the table.

Please include that point into the discussion.

350-352: “for many years and many locations...” – I do not really see that by looking at the figure, except for the station VS12. Please provide then some kind of a quantification of your statement.

361: I do not understand why do you need to refer to the south or north here. You simply do it for all 5 gauging stations, correct?

Table 2. please explain better the content of the table in the caption. I understand now what do you show there but it took me 2 rounds of revisions and very careful repetitive reading. For example, it is really confusing that you provide coefficients for the power fit and R and RMSE for the validation regression – both next to each other.

Figure 8: please be consistent with the used terminology: backscatter measurements or altimetric measurements.

I think it would be nice to arrange the figure vertically from north to south.

386-391:

please include a short explanation how did you choose the window size

>> The details were added: "The size of applied window allowed for preserving the magnitudes and spatial heterogeneity of ice thickness in spatial domain, as well as for reducing the residual noise in temporal domain, which is left after smoothing of backscatter time series with Loess filter". We did not enter into technical details how we adapt our window as it can be found in different corresponding manuals and explanation could result in one more paragraph of important size. Instead, we provided the criteria, which were important for selection of the window.

Thank you for the details. I think that some more details would not hurt here. What are the corresponding manuals, can you cite them? For example, why 40 km window size preserve the spatial heterogeneity of ice thickness? What is changing on the scale of 40 km? Why is it important at all to smooth temporally? Is there a possibility of oversmoothing and affecting results too much by it?

394: starting from this point I am again lost. Which in situ observations are you referring to when speaking about interstation areas?

399-400: how do you derive this information from the Figure 9? What VS are there?

442: please give some reference to the data

445: what means 4 days ahead – simply the difference between predicted and observed dates? Or that one can predict the date 4 days in advance? What would that mean?

447: how do you come up with 4 days of accuracy?

455: mention that the dates are for Salekhard ice road and also add more details in the caption.

457-466: I also have troubles following this. You use the relationship between altimetric melt onset and real ice road closure dates to correct the altimetric date. But then you cannot compare it with the real closure date again, because the two datasets are not independent anymore, can you?

