

We thoroughly replied for all comments and introduced all modifications.

On behalf of all authors,
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General comment

The authors have done revisions and improved the quality of presentation (both text and figures). However, several questions and suggestions were answered only partly. Below I provide a new round of comments.

I asked the authors for the professional proofreading already two times but it seems that authors ignore this suggestion. I see that another Referee made the same suggestion and authors responded: We paid the Elsevier publisher Service for English grammar correction for the 2nd manuscript version....It always worked well for our previous publications. .. Not this time... In new version we corrected many typos, missing words, rearranged the sentences. I hope we detected all catchy errors; Few occasional errors (articles and prepositions) could hopefully be corrected by specialists from journal technical team (what was the case with our last article published by EGU publisher).

I find this response unsatisfactory. I find that the manuscript requires both scientific and grammatic proofreading. I suggest the authors find professional proofreading service for the scientific texts – not only for English grammar but also for consistency and scientific rigor.

Reply. The text was sent for professional proofreading. We hope that in this version of the manuscript will satisfy both scientific and grammatic requirements. The corresponding certificate can be provided if necessary.

Some of the questions are answered only partly or not even thoroughly read and comprehended as it seems:

Reply. We thoroughly replied for all comments and introduced all modifications. The problem could arise from misinterpretation of certain remarks (because of their vague formulation) or from difficulty to trace the corrections introduced, as the text was significantly re-structured to match the recommendations of both Referees.

Fig. 11: For a comparison of ice thickness from 2D product and gauging station, why not to extract the ice thickness exactly at the location of the gauging stations?

Reply: The figure was re-plotted with the values extracted for location of the ground stations.

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. Old Reply: Number of stations used is provided in the figure caption. The line width was increased. The legend was removed.

Why did you remove the legend? The suggestion was to use legend instead of the caption. Please also refer to the journal guideline and rework all of your legends and captions correspondingly:

A legend should clarify all symbols used and should appear in the figure itself, rather than verbal explanations in the captions (e.g. "dashed line" or "open green circles").

Please distinguish clearly throughout the manuscript when you refer to the visual picking of the phenology dates ("manual") or to the automated algorithm. I am not sure that the manual selection can be called an algorithm (for example, line 436).

Reply: The legend was added and the caption was edited correspondingly. The "manual" or "automated" algorithm is now changed on "manual routine" or "automated routine". On the line 436 the word "algorithm" was changed on the word "approach". However, following the definition of the term of "algorithm" ("a finite sequence of well-defined instructions" that we described in the equation 1, which was deleted in the current version after the Referee's request), we consider that the use of the word "algorithm" in relation to the manual retrievals (issuing from manual implementation of the developed sequence of instructions) is possible. Nevertheless, we changed, where it was not critical, the word "algorithm" to the word "approach". The word "algorithm" was kept only for general cases.

On the lines 429-429 we indicated that "As the manual routine demonstrates better accuracy than the automated one, it was therefore selected for further analysis of results and for use with the ice thickness retrieval algorithm." Starting from here, we meant only the retrievals from the manual routine.

Sections 5.4 and 5.5 clearly belong to the Results. The suggestion from the other referee:

2) The method sections should be expanded with a section describing how the results are validated and all the additional analysis performed, which currently is described in the result section.

It means you should describe HOW the results are validated but not the outcome of the validation.

Reply. We interpreted this remark differently. Now, we added the phrase on how the results were validated and moved back the subsections to the Results.

132: do you mean "on average"?

Reply. No. The phrase was changed " Snow depth records represent values calculated as average from three snow-depth measurements located around the hole."

135: how is the complete freeze-over defined?

Reply. The complete freeze-over is defined from the records (flag corresponding to freeze-over state) of the gauging stations. Details are added : "according to records provided by gauging stations".

140: when?

Reply: the phrase " of the Ob River" was added.

142: this statement contradicts with the paragraph below.

Reply. the phrase " but has not yet resulted in a significant change in the ice regime of the entire lower Ob River" was deleted.

143-146: please check the grammar and syntax. Add numbers – how much later and how much thinner.

Reply. Corrections were introduced. Trends were evaluated.

Figure 2: a) and b) are missing. I like the figure more now. I disagree that adding other stations does not provide any additional information. Please add Kazym Mys as well in b) as you did in your response to the review. I think you do not need labels "Date" and "Years" as it is clear. Please use more regular time intervals on a) – first date of the month, for example?

Reply. a) and b) were added. Axis labels are usually mandatory. We checked how other articles published in The Cryosphere present the time series and found that the Years, Time, Dates labels are given in all plots. We would prefer to keep the axis labels. The 4th station was added on the subplot b.

The time interval was changed (for monthly, 10-days and 20-days intervals). Unfortunately, the use of first date of the month degrades the figure readiness.

3.2 Altimetry data – you describe here brightness temperature data as well, so reconsider the title please. For consistency, I suggest to describe altimetric measurements from both Jason-2 and -3 satellites, and then add the AMR instrument description afterwards. Thus, move 175-178 upwards.

Reply. The Title was modified and the lines were moved upward.

163: “used” instead of “considered”?

Reply. Modified.

165: can you provide a number or range?

Reply. We did not find any publication which provides the size of the Jason-2 radar C-band footprint. The band-C is rarely used. Our information is based on our own unpublished observations of behaviour of C-band waveforms over the lake ice in areas close to lake banks. However, as many other authors we have not been interested in estimation of the Jason-2 C-band radar footprint and do not have the range values that we can publish here. Nevertheless, our statement can be supported by a study of Jiang Ch. et al., A Study of the Technology Used to Distinguish Sea Ice and Seawater on the Haiyang-2A/B (HY-2A/B) Altimeter Data. *Remote Sens.* 2019, 11, 1490; doi:10.3390/rs11121490. The HY-2A/2B missions are equipped with the Poseidon radar instrument those footprint (according to this publication) over the flat surface was 1.9 km and 10 km respectively for Ku-band and C-band. To avoid any mistakes we deleted the phrase under the question from our manuscript.

171-173: should the reference go to the end of the sentence?

Reply. Two publications (Kouraev et al., 2007; and Du et al., 2017) are dedicated only to ice phenology. If we place them all together it will give an impression that the use of AMR for ice thickness retrievals is widely-applied method.

182: would it make more sense to move the sentence about ICE1 to the line 185 before the sentence “The ICE1 retracking algorithm...”

Reply. The phrase was moved.

210: please add something like “based on our own interpretation of the altimetric data from this study”

Reply. The phrase was added.

237-...: I think the opposition here is wrong – you should oppose calm - rough water surface, not calm - river, as river surface can well be calm as well.

Reply. The phrase was modified.

264: avoid saying “Our studies...”, just cite them as any other studies.

Reply. The phrase was modified.

268: better refer here to the Fig 3c, as, again, in Fig 3a one cannot see anything in detail.

Reply. The phrase was modified. We slightly improved the resolution of the figure 3a and added the crosses on the top of the intermediate peaks.

Figure 3a: Why for the TB the labels are 37 and 18? Shouldn't they be 34 and 18.7?

Reply. The Figure was modified.

Figure 3c: very nice figure! A question - you mark melt onset at the same point as you mark ice free period. Should it be corrected?

Reply. The Figure was modified.

289: retrieved using manual / visual approach?

Reply. The phrase was added.

296-298: you could refer to the Fig 3c here

Reply. The phrase was added.

299: please explain what is spring-summer peak (maybe in Figure 3c) and why its height would be suitable for the freezing detection?

Reply. "Summer peaks" was added on the Figure 3c. The following phrase was added " This helps to distinguish the peaks related to appearance of first ice (higher peak) from the peaks related to appearance of water on ice (smaller peaks)".

304: please explain why do use the difference between 34 and 18.7 GHz and the value of 2K (any references?)

Reply. References were added and explanation for 2K threshold was provided. " As during the winter the ΔTB values vary around zero and do not exceed 2K (Figure 3c), we select the backscatter peak at time t , if in a time frame of $(t-1, t+2)$ of satellite cycles at least three of the four ΔTB values are $< 2 K$. "

The formulae 1 and 2 are not mentioned in the text and look useless to me. You describe this all in the text and you provide Fig.4. What is the operator "length"? By max you probably mean local maximum but then it should be reflected in the formula. dTB and ΔTB are the same? I know that you included the formulae after suggestion of the other referee but it does not look mathematically rigorous to me.

Reply. We deleted formulas 1 and 2.

By the way, why did not you use the relative backscatter decrease/increase for the phenology dates retrieval as well? Is there any reason behind?

Reply. For sea ice and lakes ice, the relative backscatter decrease/increase approach is quite robust. In more complex case of the river ice, its performance degrades. For ice thickness, the use of relative changes allows for reducing the effect of initial conditions on Sig0 values related to configuration of VS: the higher the portion of land in the footprint the lower the backscatter is in the beginning of freezing. Moreover, the correlations of in situ Hice with relative Sig0 are stronger than the correlations with absolute Sig0.

317: please give more details – how, why, etc

Reply. Details are added.

Fig.4: $\Delta TB < 2$ in $[t-1: t+2]$ – does your formula imply consecutive dates within this interval?

Reply. No, any combination.

For the break-up ΔTB I am also not sure that the formula is correct.

Reply. we deleted formulas 1 and 2.

344-345: including all years of observations?

Reply. No, for each year. The clarifications were introduced.

Figure 5: include in situ station names in the plots title. VS135 is given twice, please check.

Reply. Station names were included and the number was corrected.

377-381: in my opinion this paragraph belongs to the previous section.

Reply. The paragraph was moved up.

406-409: move this part to the Discussion please as this is your interpretation.

Reply. The section was moved to the Discussion

Figure 7: please rework the Figure that it fits the style of other figures in the manuscript. Increase the font and line width, give titles to the subplots, remove the unnecessarily fine grid, etc.

Reply. The figure was re-worked according to recommendations.

448, 458: instead of 249 you mean 240?

Reply. Yes, thank you.

535-570: In my view, this part lacks consistency.

1) Why don't you provide figure for the ferry operation stop (535-538) as you do for the ice road dates?

2) Why do you need to correct the prediction for the closing date but not for the opening?

3) Do you conclude that the corrected prediction of the road closing is sufficiently accurate for the forecast as opposed to the road opening date?

Reply. 1) The plot for the ferry closing dates was added to the fig.12.

2) We modified the text and deleted the phrase about "correction".

3) Yes, we can conclude that the road closing prediction is sufficiently accurate. We provided a table containing main statistics allowing the evaluation of the accuracy of the forecasts.

4) If you correct the opening date prediction, would you be able to state that the forecast is reliable?

In any case, I am personally quite skeptical about such correction. For me it would be enough if you showed not corrected dates for the closing prediction as well.

Reply. We modified the text and deleted the phrase containing the word "correction". We also deleted the Figure 12b, showing this "correction".

We do not state that we elaborated a reliable forecasting system for the ice road operation. This part of the manuscript was a "case study" aimed just a demonstration of capacity of satellite observations for the particular socioeconomic application. A development of reliable forecasting approach basing only on ~10 years of available data on the Salekhard ice road operation (i.e. observations) is out of question. This can be done in the future basing on suggested here (or different) predictors derived from the current or extended altimetric product. Several corresponding sentences were added in to the new subsection of the Discussions.

Describe how do you calculate the leading time of the forecast. Provide the information on the difference between predicted and observed dates (RMSE, max-min) and on the leading time in a consistent and systematic manner for all three cases.

Reply. We provided a table containing main statistics useful for evaluation of the forecast. We deleted phrase "leading time" to avoid any disagreements.

541: do you mean Fig. 12a instead of 11a?

Reply. Yes, the number was modified.

541-547: you can also mention that the predicted dates are consistently earlier than the actual dates of the road opening.

Reply. We added the suggested phrase. Note, that the main phrase was moved to Discussions.

543-547: you should move this part into the Discussion

Reply. The part was moved.

546-547: maybe not so much of interannual variability but an overall trend for an earlier opening of the road demonstrated by both time series?

Reply. The phrase was modified according the suggestion.

541, 563: please be consistent – is it also RMSE for the opening date?

Reply. The table with statistics was added. The text was modified correspondingly.

Discussion

7.1 I suggest to name this subchapter “Factors affecting altimetric backscatter signal” or something like that, as it reflects better what you discuss here.

Reply. The title was changed

7.2 Here in the beginning you actually discuss retrievals of the phenology dates and ice thickness. Please consider restructuring/renaming.

You could start with ice phenology dates retrievals (manual and automatic), the factors influencing the accuracy of these retrievals, and potential improvements. Would inclusion of SAR data be beneficial for the phenology dates retrieval? Are there any SAR-based river ice phenology studies you could compare your results with? Then you could move to the ice thickness retrieval and do the same. You mention two studies in the section 4 (Unterschulz et al and Mermoz et al) which use SAR data for the river ice thickness retrieval – could you compare you results with those?

Reply. We extended and restructured this subsection starting from comparison of manual and automated routines. Then, we compared our ice thickness retrievals with similar statistics (RMSE) found in other studies (unfortunately only few provided RMSE).

The SAR data will certainly helpful for the river ice phenology refining and we will take a contact with the SAR specialists when the dedicated funding will be available.

I also suggest that you give a separate and clearly distinguished paragraph where you discuss your forecast prototypes and their viability.

Reply. The subsection was added

616, 619: wrong Figure numbers provided

Reply. The text was restructured and this sentence does not exist anymore.

617-...: again, what about the road closing date prediction?

Reply. The subsection discussing the ice road dates prediction was added

627-630: Do you mean that the detection of the first consolidated ice would require another sensor / data? If it is possible with the same dataset, why did not you try it? Please explain in the text.

Reply. We think that this will take an additional efforts and time and likely will call for multi-satellite data, at least for validation of algorithm. Our current phenology algorithm is the result of our experience in elaboration of river water level retrievals in the Arctic that debuted more than 10 years ago. The backscatter has been used in our water level retrieval algorithm. Probably, we could use for detection of the consolidated ice the same dataset. We got several ideas recently when applying the Sentinel-3 altimeters data for the lake ice. We are going to explore these ideas in relation to the river ice when the funding will be available.

Nevertheless, we mentioned utility of the multi-sensor approach in the text.

Could you provide for this subchapter some information on the availability of the in situ observational stations on the other Arctic rivers? That would be a great outlook on the potential future studies in a large geographical context.

Reply. We are not able to provide this information in the framework of the current manuscript. This would be possible to make such an assessment if the unified database with the station list and their coordinates were available for public access. This is not the case for Russian stations. Two publically available databases allowing to make similar assessment rapidly (Arctic-RIMS, GRDC) are dedicated to discharge stations. Their part in total number of stations is probably 1/5- 1/7. Unfortunately, the requested assessment takes extensive river-by-river search in large geographical domain: the big Siberian rivers with the seasonal ice cover are of >3500 km length.

Conclusions – I think the title should be in plural

Reply. Modified

665-666: I would say that a generally low number of in situ observations and their general infeasibility to cover vast areas are the main drivers.

Reply. We added this phrase to the sentence.

668: freeze-up, breakup dates?

Reply. Modified

669: again, do you refer here to the automated one? Briefly reiterate please the basic principle of the algorithm.

Reply. Manual one. Details were added.

674: please mention that there are 5 stations and 12(?) years of the simultaneous measurements

Reply. Details were added.

675: what is it in the percentage of the max/average ice thickness?

Reply. 7-18%. The phrase was added.

681: mean accuracy?

Reply . The phrase was re-written, providing the RMSE values as the measure of uncertainty.

681: please reconsider what you report here based on my earlier suggestions in the Results

Reply . The phrase was re-written, providing the RMSE values as the measure of uncertainty.

683-700: very good!