Reviewers comments
Our response

General comment
The manuscript addresses the important problem of deriving the degree of sea ice ridging from the ICESat-2 laser altimeter measurements. Data and methodology are well presented, however the accuracy of the results and applicability the method to other ICESat-2 measurements is questionable. As suggested below a major revision is needed to assure feasibility of the presented method.

We thank the reviewer for a thorough and valuable review of our paper! The concern of applicability of the method to other ICESat-2 measurements is not without basis and we hope to address it sufficiently both in this response and in the future revised manuscript.

My first concern is that the same data is used both to train the method (by analyzing the elevation anomalies histograms and setting the intervals in Section 2.3) and to validate the method (by comparison of along-track IS2 DIR with maps of FIS DIR in Section 3.1). Generally speaking, usage of the same data both for training and validation precludes conclusion of extrapolating the algorithm applicability. What if it worked well only on these data points?

The reviewer makes a valid point here, and this is also something that we considered when we prepared the original manuscript. This is also the reason why we kept the comparison of our ICESat-2 estimates and the ice charts qualitative. Were we to have independent ridging estimates, we could do a more complete validation of the product. Initially we planned to use winter 2019-2020 data purely for validation but alas 2019-2020 was a record mild ice season and as such, there is not enough data to perform a robust validation.

There is the possibility of only presenting elevation anomalies across the different DIR regions, however we think the most relevant and important result in our paper is that the elevation anomaly distributions differ for different DIR zones (based on FIS charts) on four non-consecutive days and separate locations (when combining the observations, Figure 2 in preprint) to such an extent that non-overlapping ridging thresholds can be determined for each DIR area, and later applied to the entire track where a general agreement with FIS charts is seen (Figure 3-4 in preprint).

My second concern is related to the validation approach and results. Only a qualitative comparison of along-track observations with maps is performed and no quantitative estimation (for example, in a form of a confusion matrix) is provided. Understandingly, the DIR product from FIS is quite coarse and cannot capture all spatial variations of ice ridging. That makes quantitative comparison with IS DIR less useful. But what if it makes useless also the algorithm training (i.e., finding the elevation anomalies intervals)?

The decision to complete a qualitative comparison between IS2 and the ice charts was made because we recognise the weakness of using the same data for building confidence in our method that was used to train it. As the reviewer correctly points out, this does not constitute a true validation and we do not imply this in the manuscript. Our goal is to show how the IS2 data performs over different ice regimes, in addition to the regions used to train the algorithm, and illustrate how IS2 data might augment the information in the ice charts.

We agree that ice charts are not the perfect comparison data set, but as we point out in the paper (Li. 78-81), they are one of the most reliable sources of information for those operating vessels in the Baltic Sea. The resolution difference between the information in the ice charts and IS2 is several orders of magnitude, and the size of features apparent in the IS2 data is surprisingly detailed. While the users of ice charts may not be interested in individual ridges, even those are captured in the IS2 elevations and this finding is one of the main messages of our paper (Li. 276-279).

Based on the reviewer's comment we see the need to add a clarification to the manuscript regarding our goals and the impact of our findings, and will add this in the revised manuscript.

These two questions are open and I, therefore, suggest changing the emphasis of the paper: instead of presenting an algorithm for sea ice ridging estimation from ICESat-2 (with a questionable training/validation approach) the paper should rather present a comparison of ICESat-2 elevation anomalies with ground truth data. Such a change in the accent implies several major modifications:

• Change of the title, goals and, correspondingly Abstract, Introduction and Conclusions.

We appreciate the reviewers comment on this.

However, we believe that our DIR algorithm is valuable - especially the fact that we present an algorithm in a form that can be easily implemented by anyone with basic control of a programming language. This despite the fact (which we will make crystal clear to the readers in the revised manuscript) that validation of the algorithm using the same data that we used to train the algorithm, tells us little about how well the algorithm compares to other data. This said, it is an easy algorithm to implement and we believe it would be of great benefit to ice services, as a supplement of independent information to their current sources, even if they use it as an unvalidated source of auxiliary information. The ice navigation community would greatly benefit from investigations using IS2 high-resolution surface measurements, and this is one of the studies that suggest a possible way of utilising such data. Simply changing the goals/title/sections to a comparison with ground-truth data, where the conclusions are the same as previously shown, perhaps does not provide any additional or new information, nor the insight we wish to provide for end user services. Nonetheless, we shall do our best to clarify our message in the revised manuscript.

• Plotting of elevation anomalies on Fig. 3 instead of classification into IS2-DIR(1,2,3).

Initially, this was a figure that we did make (however, only for 27 March 2019 when comparing with SAR) and which later led to the DIR estimation. We will add this in the revised manuscript.

• More detailed analysis of profiles of photon height (and plots of the profiles) in critical cases when FIS DIR doesn't correspond to elevation anomalies.

While we believe that we commented a lot on both positive and negative cases (A-K in Fig. 3 and 4), we understand the reviewers wish to see the profiles. Thus, for a revised manuscript, we will include photon profiles of critical cases and discuss them in more detail.

• Comparison of elevation anomalies, or photon height profiles with other independent data, e.g. Sentinel-1, Sentinel-2 in the aforementioned critical cases.

In our initial studies, we did compare SAR images from Sentinel-1 to investigate if the elevation anomalies detected by IS2 were consistent with strong backscatter, which seemed to be the case alas with some exceptions. SAR backscatter is not only a measure of large scale surface roughness, but e.g. volume scattering plays a role as well. Furthermore, SAR images also differ from IS2 in spatial resolution, and also the temporal shift between acquisitions (the SAR frames we collected in the initial studies had more than 0.5 day temporal shift) and sea ice drift will have an effect on the comparison. Finally, the ice charts used in the study are in essence an expert analysis of the SAR frames. At the time of writing, we did not think that such comparison brings much added value to the manuscript.

We can include the comparison of ICESat-2 and Sentinel-1 data. Other information on sea-ice roughness/profiles is limited in the Baltic Sea (and in the IS2 period), thus no direct comparison is made (e.g., comparing with campaign data). However, it should be noted that we have made a comparison in the manuscript already (L. 200-205, on page 11), placing our results in the context of ridge densities found by Geguic et al. (2018) using HEM observations collected in previous years.

Specific comments
Title: Consider "Comparison of degree of sea ice ridging in the Bay of Bothnia with elevation anomalies in geolocated photons from ICESat-2"

We shall revise the paper to satisfy the reviewer's main concerns. However, we do not foresee that this will require a change of the title.

Line 15: "information ON ice conditions"

Line 26. "Divergent motion forms cracks ..., and convergent motion results in …"

Line 91: "from 1 TO 3."

Line 91: "is AN imaginary"

We will include the above mentioned edits, thanks.

Figure 2. The black line (DIR2) is almost indistinguishable from the dark blue one (DIR3). Consider green, or any other brighter color.

Noted, thank you.

Equation 1. The equation says that you sum up 150 values of h_max and then subtract h_mean. That does not correspond to the text. Equation needs to be corrected.

Thank you - will be corrected.

Lines 150 – 155. In the first sentence I would recommend to replace “we classify IS2 geolocated photon heights into different DIR categories” with “we compare IS2 geolocated photon height with different DIR categories” and correspondingly rewrite the rest of the paragraph.

Thank you for this comment. However, this would be necessary in case the goal of the paper is changed - for now, since we do not aim to change the goal of the paper, we will keep the sentence.

Figure 3 and 4: Color of fast ice and FIS DIR0 is very close and hard to distinguish. The same applies to FIS DIR 1 and FIS DIR4. Figure caption describes DIR3 as blue and DIR4 as red, but to me it appears as violet and pink.

Yes, that is true. This is simply because there is a transparency in the filled contours due to the fact that we wanted the underlying small islands to be visible in the product, since some areas with elevation anomalies/DIR coincide with or are close to small islands. If the filled contours are not transparent, the islands will not be visible. We will investigate whether the figure can allow the contours to be plotted before the outline of the small islands or if it can be highlighted in some other way. If that is not the case, the transparency will be kept. We will look into alternative colours for DIR0 to see if it becomes more distinguishable.

And yes, the DIR3 and DIR4 does look more violet and pink - again, this is caused by the transparency. However, if we put the exact same colors as contours below, then one cannot see when the IS2 DIR points are on top. We will revise the figure caption, since there is a colour scale available in the Figure that shows the exact colors.

Figure 3 and 4: 27 March appears before 23 March. Is it incorrect title or incorrect map? The same with 01 and 17 February.

Yes, it is in reverse order. We will change that. Thank you.

Figure 3 caption: “where several photon heights could be extracted from AND compared to …”

This will be changed, thank you.

Figure 4: Color of DIR and counts of sails is almost indistinguishable or has very bad contrast (e.g. light blue points on light purple DIR3 polygons).

Yes, we agree. However, we did have issues with choosing a color scale that did not have this problem in one way or another. Thus, we decided to use this one in the end. We will investigate alternative color scales to see if it becomes easier to distinguish.

Line 156. I disagree that behavior of IS2 DIR follows the FIS DIR zones even generally. I would say that they correspond only in 50% of cases. That’s why it is better to compare not DIR to DIR but h_a to DIR. This sentence and the paragraph below need to be rewritten accordingly.

We thank the reviewer for this comment. We want to draw the attention to L. 150-160 in the manuscript, where we state:

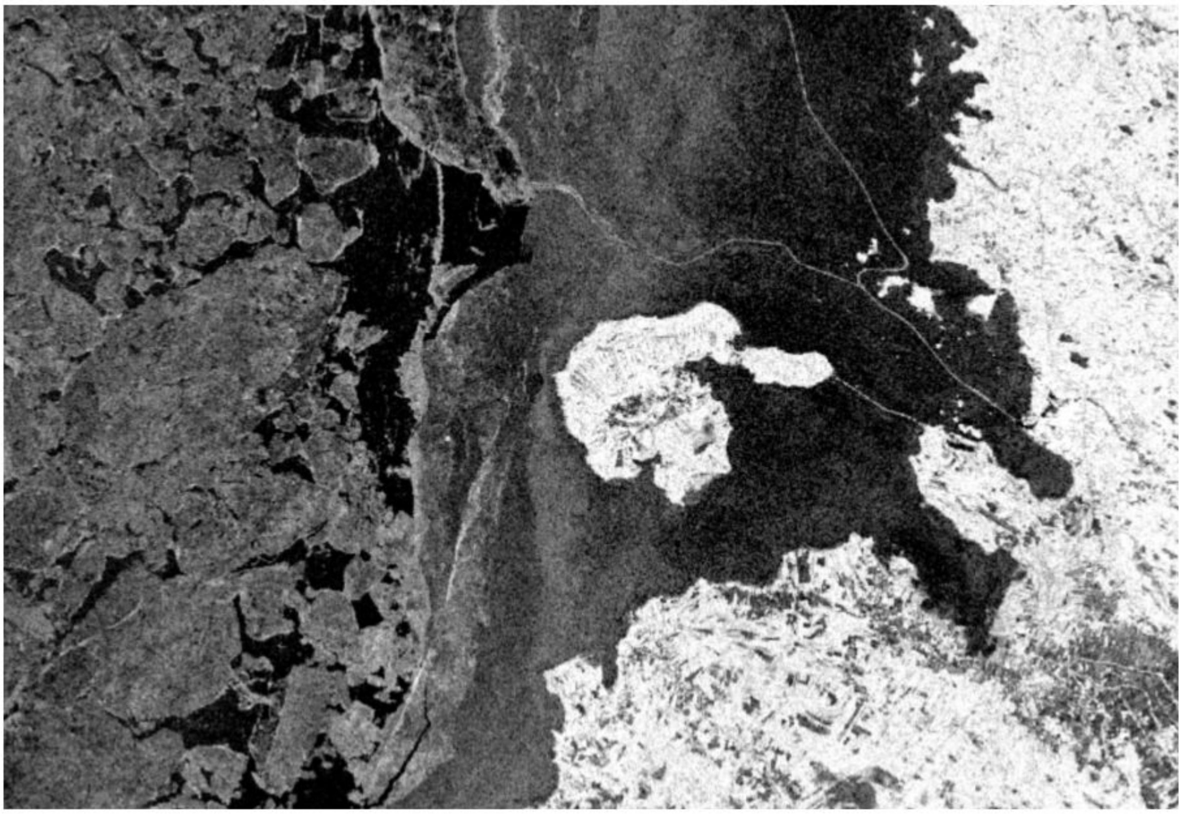
“Using our method, we classify IS2’s geolocated photon heights into the different DIR categories and present the results in Fig. 3, together with the DIR zones provided by the FIS ice charts. As expected, IS2 photons classified as DIR2 (slightly ridged ice) occur in all FIS DIR zones simply because there are areas with smoother surfaces, i.e. level ice floes between ridges, in all zones. Similarly, IS2 derived values of DIR3 and DIR4 will also be seen in a DIR2 zone since even if the area has comparably little deformation, there may be individual ridges present. In other words, IS2 is able to distinguish features at much smaller scales than the resolution of an ice chart or indeed what is practical for tactical navigation. The general behaviour of the distributions of IS2 DIR estimates follows the DIR zones from the ice charts. However, IS2 data carries much more information than just the overall DIR for the zone. As mentioned before, the ice chart DIR is a simplification, and in reality large areas that have been assigned to one single DIR are a mixture of several ice types. If ridge features are sparsely distributed and the area has a relatively large amount of open water, the zone will be assigned DIR2 by the FIS”.

So, it is not reasonable to assume that all points will be labelled the same DIR of IS2 as labelled by FIS. However, we agree that a comparison with SAR images may provide additional information in this area, especially to understand how some areas of FIS DIR3 show as DIR2 in IS2. We plan to include these in the revised manuscript.

Line 167. If “more deformation is expected to occur due to the ice drift pushing ice floes towards the coast” why is that not reflected in FIS DIR? Area west of Oulu is heavily trafficked and presumably the ice charts are the most accurate here as a lot of reports from icebreakers should come. But IS2 reports a lot of DIR4 measurements unlike DIR3 reported by FIS ice chart. Inspection of a SAR image on 23 March 2019 (see below) shows a lot of ice flows separated by leads. Maybe covered with thin ice. Could the IS2 DIR4 be modulated by edges of the floes, rather than the ridges? In my opinion that is a very good example to illustrate better by collocation of IS2 and SAR and showing profiles of geolocated photons and the detected ridge sails.

We thank the reviewer for this expert insight. In fact, this does raise the question of whether the difference between the ice floes and the open water is large enough to create a DIR4 area. If this is the case, the surface anomaly would require a difference between the lead and the top of the ice floe edge to be 0.6-0.75m. The question is then whether the sea ice is this thick in this area. Nevertheless, it definitely requires more discussion, by e.g. studies of ice floe topography using airborne laser scanning could be encouraged (albeit would be in the Arctic, which is a different sea ice regime), or by investigating the topography of ice floes using ICESat-2 - however, that is beyond the scope of this manuscript. A subject that we must discuss here is also the snow on top of the ice floes, as IS2 measures the top of the snow layer. Especially in the shallow area close to Oulu it is not uncommon that pieces of ice are stable for a considerable time and then break up as floes which start to drift. These floes may have a significantly thicker snow layer than floes that have not been stationary, resulting in higher freeboards. In revision, we shall look at the development of ice in this area from a time series of SAR images to gain additional insight on this.

However, as a comment to the first sentence here, the deformation increasing near the coast is reflected in the ice charts, both in the one from 23 March, however located further south, and for the 27 March where DIR4 is observed along the entire fast-ice region and near Oulu - the area of where the SAR image is taken. Again, here it must be considered that the winter season of 2019 was mild - and on the 17 February, the sea ice cover did not experience a lot of deformation/roughness beside right above the island of Oulu (DIR3 area). So, it is likely due to the mild winter and sea-ice drift, that you see this opening near the island of Oulu (which is also reflected in the FIS ice charts). However, what you also see is a large DIR3 area near this coast - and DIR4 forming. In fact 4 days later, DIR4 formed along the entire eastern coast of the Bothnian Bay.



Line 199: On fig 4.a most of green dots (very low number of ridges) occur over DIR3 (ridged ice) which is not discussed in the text. That’s a major disagreement which seems to be ignored. For the sake of completes of the study, not only the positive cases but also the negative cases should be highlighted and explained.

Thank you for this observation - it has most definitely not been the intention to ignore this. In fact, we found high variability across cases, that has been discussed in the text (A-G, J-K in

Figure 3-4). But, we will comment on this in the revised manuscript, perhaps even include SAR imagery and/or photon profiles to compliment.

Line 224: Cloud cover is another major factor limiting availability of IS2 measurements. It cannot be ignored as it also limits the applicability of IS2 for operational ice charting (see also the comment below).

We appreciate the reviewers comment on this. Cloud cover is a major factor impacting the IS2 observation, and has not been ignored, but commented several times (L. 180, 186, 216, 225, 238), but as has also been highlighted by Reviewer#1, it should be discussed in further detail. We aim to include more on this in the revised paper, including a thorough description of when data was available/disturbed by clouds, and we will provide quantification of the percentage of photons removed in the pre-processing step.

Line 228: I don't agree that "this study shows the potential". The study only compares IS2 and manual ice charts on a couple of cases with ~50% accuracy. How can actually IS2 measurements be used in ice charting? Which weight should an ice analyst give them compared to icebreaker observations? How accessible IS2 data would be due to cloud cover and latency? These and other questions need to be answered to show the potential.

We appreciate the reviewer's comment on this, and while we hoped we had answered some of these questions already (latency, cloud cover etc.), we see the need for even more discussion on these aspects. We aim to include this in the revised paper. The goal of our study is to demonstrate the utility of IS2 data for the community of end users operating in ice-covered water, and we hope that our paper will provide motivation for new IS2 elevation products with lower latencies in the future.

Line 233: "to estimate the ice conditions in certain areas for planning purpose"

Thank you.

Line 255: Can you specify how IS2 could be used?

An example for ice charting in general has already been provided in L. 234-235: "An example of such planning would be compiling the statistical information on ice conditions required by the International Maritime Organisation (IMO) polar code to create a regional climatology (IMO, 2020)."

We shall give more concrete examples in the revised manuscript.

Line 262: Given low correlation between IS2 and DIR I would recommend to rephrase: "… we have showed that there is some level of correspondence between FIS DIR and height anomalies using geolocated …"

Noted, thank you.

Lines 270 – 273: Please rephrase "…we find that in some cases along-track densities of relative …" and split into simpler sentences.

Noted, thank you.