Response to the review of "Brief Communication: Mapping river ice using drones and structure from motion".

The authors wish to thank the reviewers for their constructive comments and corrections to the discussion paper. In the following we have responded to each of the comments from each reviewer. The comment from the reviewer is in *italic font* while the response is in blue normal font.

First a general comment to all reviewers. From a comment from Mark Loewen on the description of the number of images used in the assessment in the Haga bru case, we noticed that we had used a dataset with a lower image quality treshold for the construction of the DEM. The results in the discussion papers are correct given the data used, but the Haga bru case is not done with the same level of image selection as the Sokna case and it does not follow the correct procedure according to the description in the paper. We therefore re-ran the Haga bru analysis to make the cases comparable. This lead to minor changes in Table 1, to Figure 1 and 2 and to the computed size of the ice jam on page 5, lines 5 – 10. We thank the reviewer for pointing us in the direction of this and we are sorry for overlooking this mistake in preparing the discussion paper.

RC1: Responses to Karl-Erich Lindenschmidt

This communique highlights the application of drones in river ice work. Such applications are becoming more and more essential as advances in river ice research are requiring more detailed descriptions of the characteristics and geomorphological settings of ice, hence this note is timely. It introduces a method to other river ice researchers seeking an inexpensive and safe alternative for ice cover mapping.
Before this communique is accepted for publication, some amendments and revisions are required:
Limitations of the photography are extensively elucidated (particularly on Page 6), however little is said about the limitations of using UAVs themselves. Examples may include: - additional qualifications and registration required of the UAV operator, - operations feasible only on wind-calm days, - only short flights are possible (also through regulations) and - UAVs cover smaller aerial extents compared to other methods. Perhaps these can be listed in the Introduction so that the reader can assess if these limitations would hinder their particular case studies.

We agree with the reviewer that this is important information for those that might be interested in employing a drone for assessment of ice. We have added a sentence in the introduction about limitations, and a more detailed paragraph in the discussion with some more information on regulations, flight time, flight distance and other factors related to the operation of a small drone.

The section of consolidated ice shown in Figure 1 is labelled 'ice jam'. This may not be quite correct since ice jams usually extend across the river width to cause backwater staging. Perhaps labelling this section as 'consolidated ice' or 'ice-jam remnant' is be more accurate.

The ice left on the bank is a part of an ice jam that covered the entire river for a period, but a part of it was removed by high water. We have updated the text to "ice-jam remnant" which we agree is a better description.

Line 27 on Page 5 refers to "free water". I'm not sure what is meant here. "Open water" doesn't quite fit either since how can river ice have open water. Please restructure the sentence to clarify its meaning.

We refer to an ice cover with sections of open water, so "free water" is replaced with "open water sections"

Some minor, editorial revisions include:
Page 1, Line 23: replace "difficult and potentially dangerous" to "difficulties and dangers"

We have removed "wrought with" as suggested by the second reviewer and "difficult and potentially dangerous" should therefore be ok.

Page 1, Line 30: "ice processes dynamics" should read "ice process dynamics" or "the dynamics of ice processes".
Updated
Page 2, Line 33: perhaps replace "built" with "constructed"
Updated
Page 3, Line 30: "were", not "was"
Updated
Page 5, Line 17: "where", not "were"
Updated

RC2: Response to Mark Loewen

This is a timely and interesting paper describing an efficient and economical method for using inexpensive aerial drones to map river ice. I am quite certain that this method will prove to be a valuable new tool for river ice researchers. The method does not appear to be novel but its application to mapping of river ice is new and innovative. Accurate estimates of the aerial extent of various ice types can be made and under the right circumstances estimates of ice volumes are possible. The authors point out that the method allows for repetitive measurements to be made of the same reach because data can be acquired quickly and relatively easily. This will allow researchers to study the evolution of ice covers and the associated processes in much more detail than was previously possible.

The authors could consider discussing the following questions: How does the limited range of these inexpensive drones impact the applicability of this method to large rivers? I have deployed a similar drone on the Peace River, Canada and the width of the river exceeded the range in some places.

We added a paragraph in the discussion on limitations using the small drone (also requested by the 1st reviewer). The practical flight distance of the drone when flying under control is an issue in larger rivers, and fixed wing drones may be a better option here.

In extremely cold weather battery life can drop by 50%. Did the authors encounter any difficulties because of this effect?

This is an issue we have experienced when operating in cold weather, and care must be taken to avoid problems with a sudden drop in voltage. We added some text on this in the introduction and recommends careful attention to battery levels and to have several spare batteries stored as warm as possible when operating in cold climate.

Would a higher resolution camera improve the accuracy of this method?

Well yes there will be improvement. The important thing is the sensor size for the resolution and the capability of handling low light conditions. The resolution impacts the SfM workflow in principally two ways: 1) Feature matching between two cameras depends on multiscale image regions common to two images. Therefore, when the resolution is increased, the quality and quantity of features tends to increase. This improves the camera position and orientation estimates of the modelled regions. 2) Depth maps are needed to create the dense point cloud reconstruction. These maps are derived from ray-tracing based on the original images. If the images have higher spatial resolution, the quality of the depth maps, and therefore the dense clouds also tend to increase. Some text is added to the discussion on camera quality.

The paper is well written and the topic will be of interest to readers of the journal. I recommend publication after the authors have addressed the minor revisions suggested below.
Specific Comments:
Page 1-Line 23: Delete "wrought with".
Done.

Page 2- Line 29: ". . .and show examples of output of the method." Awkward wording.
The last part of the sentence is removed.

Section 2.1: Please provide more complete descriptions of the study reaches e.g. widths, slopes, geomorphology etc.
We have added some more information on the river reaches.

Page 3-Lines 21-23: Unclear - there is only one camera on the drone so increasing the number of cameras is clearly done in the software. Please explain this more fully.
We agree that this was a somewhat confusing statement. We have upgraded this to rather explain how many pictures that was taken in total compared to the pictures we used in the SfM analysis. In doing this, we discovered that the reported data from Gaula was not the dataset prepared using the 0.8 image quality threshold. This is now upgraded and it led to a small adjustment of the results presented in table 1, figure 1 and figure 2 in addition to the computed volumes and ice depths. This slipped through the quality check in the original manuscript and we thank the reviewer for leading us on to this problem. We have added a comment to all reviewers at the start of the document to point to this change in the manuscript.

Page 3-Lines 28-29: Are the 9 points referred to here called Control Points in Table 1? This is unclear.
Yes, they are now referred to as control points both in the text and in the table.

Page 4-Line 1: Should this read ". . . index greater than 0.8. . .". Seems odd that when a quality index is greater than 0.8 that images were excluded. If space permits a brief explanation of how this quality index is computed would be helpful.
We have clarified this in the text. It is not perefectly clear from the Agisoft documentation how the quality index is measured, but experiences we have is that the contrast distribution in the image is the variable measured in the quality index.

Page 4-Lines18-21: The authors write that "...the digital elevation model is considered good. . .". They are referring to the errors listed in Table 1 but a brief discussion of how they arrived at this conclusion would be helpful to readers who are not familiar with DEM's.
The text is upgraded to state that ice features can be found with high precision given the deviations found in the GCP and control point measurements.

Page 4-Line 33: Delete "varied from".
Corrected

Page 5-Line 1: Change to ". . .were 1.12. . .".
Corrected

Page 5-Lines 2-5: The meaning of "outermost" and "outer part" are a bit unclear.
We have tried to make this clearer by stating that it is from 25 meter

Page 5-Line 22-23: Why was access to the Sokna reach difficult or impossible? Is it difficult to get to the stream or is it the ice conditions that make it unsafe?
It was due to the ice conditions. The text is updated to state this.

Page 5-Lines 30-33 & Page 6-Lines 1-3: It is not clear to me how determining the open water elevation helps to assess the thickness of anchor ice dams. Anchor ice dams are anchored either to the bed or large rocks so how is their thickness related to the open water elevation? Perhaps I am missing something here but the middle section of this paragraph confused me.
At the study site, we could see the rocks that held the remnants of the broken anchor ice dams, and from these observations we could see that using the water surface as a basis for the computation of the thickness of the anchor ice dams would be reasonable. We do agree with the reviewer that this is not a generally applicable method and have updated the text to state this more clearly.

Page 7=Lines 1-6: Much of this is repetitive. Perhaps it could be deleted?
We do think that the comparison to other methods are relevant, and we are therefore a bit reluctant to remove this entirely. We have tried to rewrite the first part to make it less repetitive.

Page 7-Lines 14-15: This is unclear, ". . .could benefit process understanding and model development and also the calibration and validation process." Please clarify.

The sentence is reworded and improved.

Figure 1: Unclear, is camera position the location of the drone when a single image was taken? What is the red arrow in the lower left corner? If it is flow direction, please relocate it and label it or refer to it in the caption. Note that flow direction should be indicated in all figures.
The camera positions are the location of the drone for each picture, and the red arrow is the flood direction. The figure and caption is updated

Figure 2: Label the plots A, B and C. Where are the last 10 m in the plots? Or perhaps the last sentence of the caption can be deleted since I think this information should be in the text.
This is explained in the text and the sentence is deleted from the caption.

Figure 3: The last sentence in the caption has some grammatical errors.
The sentence is updated

RC3: Response to Shawn Clark

The authors have presented an interesting paper regarding the use of unmanned aerial vehicles to map river ice extent and ice properties such as ice thickness and volume. It has also been our group's experience that UAVs can be very helpful for this purpose, as well as to quantify surface ice concentration and ice pan velocities. Their methodology should be useful for many other researchers in the field as well. I have a few minor comments, followed by some grammatical suggestions. It would have been helpful to have additional detail regarding the specifics of what constitutes a high quality image vs a low quality image, and how one might be more likely to achieve the former. Any practical tips for a successful flight would be appreciated by the readers.
We have added a paragraph in the discussion (as also requested by the other reviewers) with some information on flight times, battery issues, flight distances and flight regulations. We have also added some more information on picture quality and issues related to light which is important for good quality in images and the following SfM analysis.

It also would have been nice to see whether the chosen number of control points was actually necessary. Is it a coincidence that the number of points only varied between 9 and 11? Would the error have been significantly increased if only 5 control points had been used? Are there any suggestions for the placement of these points? For instance, had the opposite bank been easily accessible, would it have been better to have the control points more evenly spaced throughout the measurement domain?

The number of points are mainly based on experience from previous applications of the SfM method. Goldstein et al. (2015) shows that quality of the georeferencing increases when points are increased up to 10, but adding more points above 10 give little improvement. We will add a comment on this in the manuscript.

It is important to spread the control points in the x and y direction and to avoid to have them in a "straight" line. For Sokna it would have been good to have points on both sides of the river, but the opposite bank was out of reach at the day of measurement.

Goldstein, EB, Oliver, AR, deVries, E, Moore, LJ and Jass, T. (2015) Ground control point requirements for structure-from-motion derived topography in low- slope coastal environments. PeerJ PrePrints | https://dx.doi.org/10.7287/peerj.preprints.1444v1

Specific Comments:
Pg. 1, Line 23 – Difficulty rather than difficult
The sentence is updated (also commented by the two other reviewers)
Pg 2, L8 – "area covered by imaged area" could be reworded.
This is reworded
Pg. 2, L21 – be consistent with either 'freeze-up' or 'freeze up'.
Updated
Pg. 2, L24 – control, rather than controls
Updated
Pg. 2, L32 – unnecessary comma
Updated
Pg. 3, L8 – Clarify the meaning of annual mean annual flow.
This was an error, now updated to "28% of the mean annual flow"
Pg. 3, L21 – increase the number of images, rather than cameras?
This is also pointed out by reviewer2, and the sentence is reworded to be clearer and to remove a possible misunderstanding between images and cameras. See comment to all reviewers at the start of the response document.
Pg. 4, L1 – I'd like to know more about what constitutes a quality index of 0.8.
It is not perefectly clear from the Agisoft documentation how the quality index is measured, but experiences we have is that the contrast distribution in the image is the variable measured in the quality index. We have added some info in the text.
Pg. 4, L5 – 14 – There are four instances where the word 'are' or 'was' should be replaced with 'were'.
Updated
Pg. 4, L27 - . . .mechanisms that form . . .
Updated
Pg. 4, L32 – delete the word 'were'
Sentence is updated
Pg. 5, L1 – the shear wall heights were . . .
Updated
Pg. 5, L5 – Figure rather than figure
Updated
Pg. 6, L12 – snow cover develops
Updated
Pg. 7, L7 – Though rather than thought
Updated
Table 1 caption – control rather than Control.
Updated
Caption Figure 3 – "shows and the two sections" should be reworded.

Updated