Response to Reviewer 1

Point by Point response

Major comments

1) *For single peak frazil growth intervals, the large difference in measured and modeled value of frazil ice volume was shown. As the author suggested, in-situ riverbed anchor ice growth can be a factor of the difference because the river model ignored it. Did the volume of anchor ice growth on the riverbed reach to the level to explain the gap between measured and modeled frazil ice volume with 1 order of the magnitude quantitatively? Does the model overestimate suspended frazil ice volume in the case of lack of in-situ riverbed anchor ice growth? I would like to see more discussion.*

   In the absence of measurements of porosity, neither of our two methods for estimating increases in anchor ice thickness directly quantifies anchor ice volume production rates. Such estimates were only derived from the differences between the sensible heat loss rate immediately prior to frazil onset (derived from the cooling rate) and the latent heat produced by measured increases in frazil fractional volumes. Given that the cooling rates were generally compatible with the measured air temperatures which were also a key input for model simulations of suspended frazil fractional volume growth, our anchor ice volume production rate essentially guarantees that it accounts for the differences between modelled and measured frazil contents. If anchor ice growth were not present and buoyant frazil capture were to be the primary source of surface ice production, an effective model would have had to simulate frazil concentrations 1 to 2 orders of magnitude above measured values. It was precisely this discrepancy that necessitated our inference of dominant *in situ* anchor ice growth.

2) *The author showed that the river ice model overestimate suspended frazil ice volume. The results and discussions were based on the field data during single peak frazil growth intervals. On the other hand, these cases are not suitable to calibrate the model because of the presence of riverbed anchor ice. Are there some frazil ice growth events without the presence of riverbed anchor ice? If the model is able to estimate suspended frazil ice volume in such cases, anchor ice growth becomes to be a great factor for the model simulation*

   As far as we can tell, the rate of frazil growth is only a small fraction of that expected from the heat losses inferred from pre-event cooling rate throughout the onset of frazil growth: i.e. we immediately see from the table that initial frazil production is much less than expected from the energy balance requirement. This tells us that riverd and water column ice growth is initiated simultaneously, with the anchor ice growth lagging only long enough to allow for the attachment of a small number of seed frazil crystals.

3) *The author presumed riverbed and underwater situations for single- and multi-peak frazil growth intervals. These situations are consistent with measured variations of frazil ice volume during these intervals. The author suggested that the air temperature is the key factor to induce those two situations. The multi-peak frazil events were induced during the periods of cooler air temperatures. According to the discussion of section 3.2.2, accumulated anchor ice layer became thicker during higher temperature periods. However, the heat loss from the river to the atmosphere becomes larger at lower air temperatures under same wind conditions, enhancing frazil ice and anchor ice growth. I would like to see more discussion on this point.*

   The reviewer's point is well taken and we believe the consistent association of multiple peak events with lower air temps and greater cooling supports our argument for this view. Nevertheless, it is based upon observations of a total of two events. However, we also were able to include references to crystal size and the strength of ice dams under soft rather than hard cooling conditions. This interpretation is also compatible with our experience in the growth of high quality metal and semiconductor crystals in which rapid growth increases defect density and structural weakness.

#Specific comments

P. 1, L. 15 – 17: "A simple physical model . . .. river ice volume and mass." I agree with your opinion. In addition to it, I would like to see quantitative discussion in the main text.

The model is summarized for single peaks at the end of 3.2.1. Important features deduced from multipeak data are added in 3.2.2. It is extended in 3.2.2 and an integrated summary is provided in Section 4.1 (p18).

P. 4, L. 5 – 6: "Detailed analysis were confined to four of five major supercooling events," Why did the author focus on supercooling instead of suspended frazil ice detected from echogram plots? Cooler river water is lighter than warmer water at the temperature below 4 ◦C. Hence, frazil ice possibly appeared in the water column when supercooling was not detected on the riverbed.

We make no distinction in the text between "frazil" and "supercooling" events: viewing them as interchangeable since frazil formation required supercooling. When frazil appears, it is shows up in comparable amounts throughout the water column.

P. 5, L. 30 – 32: "These runs utilized . . .. a hydrostatic site approximately 370 km upstream of the SWIPS instrument." Was the hydrostatic site located ~370 km upstream of the SWIPS instruments site? It seems to be too far to apply the data to input the model calculation. Does the author have some comments about it?

Yes, 370 km was very far from the SWIPS site and, yes, that distance complicated model/measurement comparisons. In fact, as we make clear in our discussions of the individual events, it usually necessitated time shifts and other adjustments in the assumed environmental forcing to allow correlations with the timing of the observed frazil events. The measurements were part of a BC Hydro's annual river monitoring program which was just one of similar programs carried out both prior to and after the 2011-2012 work. The levels of effort were judged to be sufficient to support development and calibration of ice models for hydroelectric and flood management purposes. Clearly the detail and precision of the model comparisons could have been improved with additional, closer, upriver measurements but we do not believe this would have qualitatively altered the obtained levels of model/measurement agreement or our key conclusions regarding the roles of frazil and anchor ice. We elaborate briefly on the underlying robustness of such comparisons in the revised manuscript.

P. 6, L. 3: "Five separate intervals of supercooling" How large was the level of supercooling? I recommend that the author add the level of supercooling at each interval in Table 1.

As noted in the text, heating of the instrument package was necessary to prevent immediate blockage of the acoustic beams and package destabilization. This precluded local measurements at accuracies sufficient for tracking levels of supercooling. The water temperature data were primarily used to detect initial supercooling and estimate the rate of cooling .

P. 6. L. 34 – 35: *"The timings and intensities of the blockages, ... , are summarized in Fig. 3" I recommend that the author should show the situation of acoustic blockages and the air temperature at the same Figures of time series* *of F(t) in Figs. 2 and 5 or echogram plots in Fig. 4. Direct comparison of the timings of acoustic blockages with time evolution of F(t) or echogram plots helps us understand what the author described.*

The only significant blockages that occur during the portions of the F(t) records plotted in Figs 2 and 5 occurred at the ends of the records in Fig. 2 and are noted in the captions. These blockages imtroduce the step falloff in F(t) at the end of the displayed record. It is important to note that F(t) in periods of partial blockage are of little use for assessing water column frazil content. Meaningful content measurements require absence of anchor ice above the transducers.

P. 7. L. 20 – 33: 1) The author pointed out critical timings such as 08:00 Jan 26, but these are difficult to be found in Fig. 4 accurately. It might be better to show such timings in Fig. 4 using some objects like as triangles.

We have added a 08:00 marker arrow just below the time axis. There is at least a '/+/- 0.5 hour uncertainty in establishing the first faint trace of the blockage onset. Actually, judgement on this timing also should take into account information provided by the the terminating drop in F(t) in those intervals which incur blockage.

The author mentioned the time evolution of "close-in" returns at the lowest end of the range scale, but it is too small to understand its vertical variation. In particular, the author explained that suspended frazil ice disappeared from the echogram plots due to the acoustic blockage by anchor ice at ~10:00 - ~16:00. However, the vertical evolution of the layer close to the transducer was unclear in Fig. 4 at that timing. Additional panels to enlarge the range near the transducer and to show the timings of the acoustic blockages (as shown in Fig. 3) help us understand the situations of frazil and anchor ice growth.

We don't think this is practical and would complicate an already complex Figure. The changes on the display scale are very slight but discernable by the trends even if the precise points where close-in thickening begins are hard to establish from the figure and, as indicated above, should also reflect the F(t) curve.. We do introduce an expansion of the close-in region in a later figure to illustrate important changes which occur in this layer when physically significant accumulations of blockage ice are present.

In section 3.2.2, the author suggested that anchor ice which detached from the riverbed and moved to the river surface was detected with the acoustic instruments. Why was such detached anchor ice not detected in the case shown in Fig. 4? Did the accumulated anchor ice melt and lose the thickness?

Our brief explanations may have led the reviewer to misunderstand the limitations of our measurement technique. We are measuring, at any given time after emission of an acoustic pulse, the sum of all returns from a roughly 4 cm horizontal slice of the water column. (The thickness of this slice is essentially determined by the duration in time of the emitted pulse) The returns used in our F(t) extractions are associated with a range cell 2.3m above the transceiver and thus, arise from circular disk-shaped volumes about 0.5m in diameter representative of returns from the mid-water regions of interest for our analyses. The ProfileView plots give measures of the intensities of the returns of a single acoustic pulse from imaginary adjacent disks stacked at ranges between the transducer faces (which give the close- in signals) all the way up to the river surface which gives the river surface returns. When a piece of anchor ice becomes detached and moves upward to the surface it moves through this imaginary pile of stacked disks as the targets move down river at typical 1.25 m/s speeds. For fragments with horizontal dimensions on the order of 1m, the strong returns would be expected to be confined to just one 4cm thick disk…in one pulse… Subsequently the ice would be completely out of the beam and, probably, in a different range when, 1 s later, the SWIPS instrument emits and detects another pulse. The only way of acoustically seeing such fragments would be in the Profile view plot. If you were looking at the F(t) data analyzed in the paper you would have to have chosen the height in the water column to coincide with the position of the fragment and the two-minute averaging would bury the strong return on a single spike into noise level of the F(t) curve.

As I indicated in the text you can see fragment returns as a few high intensity (usually yellow to red) returns in a single image pixel corresponding, as indicated above, to a single range cell in the returns from a single transmitted pulse. We only convinced ourselves that our model was correct when we found that we could see such pixels after a detachment event but we had to look, very carefully, for them. We included an example in our earlier 2017 ASL Report listed in the references which is available on ResearchGate, Unfortunately, a few isolated yellow-red pixels just get lost in the larger display and there is much better evidence in the literature for anchor ice detachment. We could zoom in on such pixels but all you would see is an orangey spot embedded in a sea of mostly blue and green spots on a black background ……not very enlightening.

In Line 27 – 28, the author described "Smaller concurrent reduction were apparent in the strengths of the longest range components o the saturated surface returns." But, I was not able to find this situation.

If you look at the surfaces around 10:00, Jan. 26 the thickness of red band thins slightly and the faint blue lines above it (which correspond to later-arriving portions of the surface signal) fade.

P. 7, L. 36 – 38: "This pattern . . .. to completely block detection of acoustic returns from water column and surface targets." Does anchor ice covering the transducer prevent return pulse only? Emitted pulse might be prevented by anchor ice?? This is just a comment.

The attenuation of acoustic waves by ice on the transducer is independent of wave propagation directionality…i.e. similar reductions during transmissions and receptions.

P. 8, L. 19 – 20: "Pre-transition sensible heat fluxes, ... change in water temperatures measured on the ADCP instrument." Was the heat loss from the river surface to the atmosphere calculated using atmospheric conditions such as the air temperature, humidity and wind speed? When the water temperature is at the freezing point, the change in the water temperature due to the heat loss becomes to be small. In addition, the water temperature can be changed by advection.

We used the described simple formulation employed by most operational river ice models which do not apply corrections for wind speed and humidity. Given the miniscule amounts of supercooling attained , heat loss calculations were, as described in the text, assumed proportional to a product of the ice-free river surface fraction and the depression of surface air temperatures below the freezing point.

P. 8, L. 25 – 27: What was the heat to transform from the heat loss to the atmosphere if it was not used to form ice?

It cooled the river, I presume.

P. 10, L. 25 – 42: The discussion in this paragraph is interesting.

Yes, and I think it is at the core of the physical model we develop. I must admit, that when we started the analysis portion of our work, having relatively little background in river ice studies, we were surprised by the fact that the *in situ* process wasn't the default candidate for anchor ice growth. We don't understand why Pietrovich's observations appear to have been largely ignored.

In Line 33 – 38, particularly, the author proposed good discussion of enhancing anchor ice growth under hydrographic conditions in the Peace River. I would like to see more quantitative discussion, if it is possible. Does the total volume of suspended frazil ice and anchor ice become to be consistent with modeled value of F(t)? According to section 2, the instruments were heated. Is this a factor to suppress anchor ice growth or accumulation on the riverbed?

We try to elaborate a bit in the edited version but we have a limited amount of information to work with. We were pretty sure that, by now, a follow-up study which included underwater video recorded at and adjacent to the monitoring site would have been available. As far as we know it hasn't been done. Since we monitor anchor ice growth indirectly, all we can say and, we believe, demonstrate in the manuscript, is that the inferred thickening of the ice layer and its associated mass, are consistent with measured frazil fractional volumes and estimated rates of heat loss to the atmosphere. The applied electric heat could not have been sufficient to seriously impact upon ice growth on the riverbed. It obviously was sufficient, as intended, to seriously suppress growth of the critical transducer faces (except possibly in one channel that we did not use in the analyses). We believe the close-in blockages we did see were largely a spillover from the adjacent layer in which the ice bridged across the transducer head, possibly avoiding direct contact with the warm surface.

P. 11, L. 10 – P. 12, L. 2: How large the spatial (horizontal) scale of anchor ice on the riverbed? Was anchor ice distributed around the riverbed with uniform thickness? Is it possible that the instruments promotes/suppresses anchor ice formation and accumulation? Is the discussion described in these paragraphs able to be applied only for the case when instruments are deployed on a riverbed?

The consistency of the observed variations in F(t) and the correlation with the synoptic energy input variations suggest the horizontal scales were on the order of kms. Good evidence of this is that the measured water levels vary smoothly and coherently as can be seen in the ranges of surface returns as well as in the hydrostatic data. Subaerial video collected by BC Hydro show detachment and fragmentation of meter-sized sheets of such ice.. Sidescan sonar measurements confirmed the large extent of this ice form but, as far as we know, have not documented their degrees of uniformity or thickness other than to order of magnitude. It is hard to say how much of our methodology would be easily transferable to, for example, ice formation on vertical surface. My suspicion is that equivalent info would be accessible with moderate amounts of input from underwater photography.

P. 14, Eq (7): Why does the heat flux depend on the air temperature only?

Data on other factors were apparently not sufficiently available on the modelled scales.

P. 14, L. 6 – 23: The author described the impact of river currents on the heat loss of anchor ice in P. 10, L. 28 – 38. Can the author consider this effect to evaluate the cumulative heat flux? The cumulative heat flux of 5.6 MJ/m2 calculated from Eq. (7) may not be suitable to be used as the critical value.

We don't have enough information on the anchor ice to go into the thermodynamic balance near the riverbed. Obviously our estimate has uncertainties and probably may vary with river speed, bottom composition etc. Only studies of additional events can address this problem. As indicated, our choice was based upon being sure that our estimates of cumulative flux began at a time when the riverbed could be assumed to be ice free.

P. 16, L. 4 – 5: "a tendency for water level . . .." This behavior was only found during Interval 3 in Fig. 5b. Did the author mention about Interval 3 only?

We’re not sure what this question means. We’ll be sure that the text indicates that the observed behaviour was unique to that one study interval.

P. 16, L. 5 – 6: “Mean air temperature . . . associated with Intervals 4 and 5.” The author described the air temperature for each Interval for the first time here. I recommend to add the panels of time series of the air temperature in Figs. 2 and 5.

This will be done.

P. 16, L. 19 – P. 17, L. 4: 1) The author suggested than the air temperature was a key factor of distinctions between single and multi-peak frazil events. Are there other possible factors such as wind speed and current speed? I think that turbulence is needed to bring lighter and cooler water down to the riverbed. If it is right, much water with lower temperature is brought from the river surface to riverbed. In the fact, single and multi-peak frazil events occurred during higher and lower temperatures, respectively. However, the relationship between such two situation and the air temperature was not explained.

As noted above in responding to an earlier question, we believe the physical stability of the anchor ice layer is adversely affected by increasing the rate of growth. This is not an unusual feature of crystal growth and two references are given in the manuscript which have noted evidence for similar effects in subsurface river ice.

This manuscript indicated that multi-peaked F(t) was attribute to detached anchor ice. I propose that anchor ice can detach at least one time for “hard” freezing conditions at Ta ≥ -15 ◦C. Then, the instruments can detect the resuspended anchor ice during the end of single peak event. Did the instrument show such an event in echogram plots or F(t)? multi-peak frazil events occurred during higher and lower temperatures, respectively. *If anchor ice was formed around the riverbed and detached, the instruments detect resuspended anchor ice at several times. This scenario can explain the multi-peak F(t) when ice advection was taken into account. How do you think about it? According to Fig. 9, the height of the instrument is a factor to separate between single- and multi-peak frazil events. Does the author have some idea to express the relationship between the instrument height and the air temperature to distinguish the two situations?*

We believe this suggestion arises, as noted above in a question about Section 3.2.2, from a misunderstanding of the measurement technique. All the peaks and features in F(t) interpreted by us as measures of frazil content corresponded to periods free from beam blockage. The height of the instrument only determines the length of the time interval between frazil onset and the onset of blockage. Blockage effects only appear after we recorded the data associated with both types of frazil intervals. At worst, a resuspended fragment of anchor ice from upstream areas would only contaminate one single pulse return if it just happened be suspended at the mid-water range selected for compiling our F(t) time series. The 2-minute averaging utilized in the generating these time series would have precluded any effects on frazil volume estimates.