Response to Reviewer 2

Point by Point Response

The paper deals with the application of a SWIPS in the peace river and the interpretation of the results. The paper also compares the findings from the SWIPS with modelled data using the CRISSP1D river ice model. I think this is interesting findings, and the application of SWIPS could provide new insight into the formation and transport of frazil and anchor ice in rivers. So, I think this could be a valuable paper for ice researchers. I do think some clarifications is needed in the paper and it could also benefit from a simplification of the structure and the objectives of the work. I find some of the text quite dense and detailed and sometimes hard to follow. Four events are singled out for the SWIPS analysis, it is single peak and multipeak events, there is the data from the CRISSP1D model and there are other observations from literature mixed into the discussion in chapter 3 and 4 and also in 1 and 2: I miss a clear section of the objectives of the study as a final part of the introduction. What is the main objective? Testing of SWIPS? Determining the relation between in situ anchor ice growth and frazil? Testing the CRISSP1D simulations against SWIPS data? Please guide the reader. - There is a form of reading guide at the end now which could be improved. This promises something on CRISSP1D in section 2, which only amounts to some info on the setup.

A improved version of this would be helpful.

Apologies. Restructuring was badly needed in the Introduction as well as a little more elaboration on the Section to Section structure. It has now been done.

There is a focus in the abstract (line 13-15) and in the introduction (30-33) which have "anomalously" low frazil content compared to the CRISSP1D model which I understand from the text does not simulate the formation of anchor ice formation. If this is the case, I am not sure I understand this comparison and the focus on the differences. If the model does not handle anchor ice properly, I do not see why this comparison is an issue at all unless you want to convey to the model developers that they need to improve their model? Or is there a previously understanding from observations that the formation of anchor ice is not a large part of the ice formation process in the Peace river? Do the discrepancies between modelled frazil and observed frazil + anchor ice development match in some way? It could be I am misunderstanding this but in the summary it seems that in-situ growth is a surprising discovery. I thought this was a well-established principle of anchor ice development, particularly in smaller rivers and streams where large quantities of anchor ice is seen developing while the amount of suspended frazil could be quite low. There are a number of works outlining this mechanism, e.g. Turcotte et al. in several papers. On page 20 you seem to reject the principle of growth of anchor ice by capture of frazil. This might be the case on a large and deep river like Peace, but I do not think this is the case if you look at anchor ice formation in general. In shallow turbulent streams accumulation (capture) of frazil should be considered, see e.g. Stickler and Alfredsen (2009, Hydrological Processes). But it could be difficult to distinguish these processes at times, and I agree with the need to address this as outlined at the end of section 4.2.

There is a complicated history here. Our initial involvement in frazil studies primarily involved helping with the deployment of instruments which we manufacture and carrying out the processing of the acquired data. We had, in earlier processing of data from previous BC Hydro deployments, noted serious problems with physical instrument instabilities…completely losing one instrument and incurring beam blockages in all annual programs. This necessitated adding electrical heating to the instrument package. Nevertheless, after processing the results from the 2011-2012 deployment we were told that we had missed something …the deduced fractional volume values were very far below those required to make the CRISSP1D model simulations compatible with observed surface ice volume production rates. The latter rates required frazil contents to be about two orders of magnitude larger than indicated by our data. Our description of these results in the abstract as "anomalous" relative to a model which excluded anchor ice growth reflected the fact that such a model was considered to be fully credible at the time. This view was based upon prior simulations which appeared to justify neglecting anchor ice growth since its inclusion significantly worsened agreement with surface ice growth data and introduced interpretative inconsistencies. Unfortunately, the anchor ice growth calculations were based upon the frazil capture mechanism which was, at the time, viewed as the principal or only mechanism for anchor ice growth in rivers as large as the Peace. This view was explicit in Steve Daly's remarks in the Beltaos river ice compendium. The consensus opinion , at that time, was that in situ growth was confined to small creeks and streams where it was usually morphologically detected by numerous people (Kempema, Ettema, Alfredsen, Stickler and Turcotte, among others) in the midst of other ice forms, Consequently, in its purported absence, the CRISSP1D simulations had to produce large quantities of frazil in order to be consistent with eventual production of observed amounts of surface ice. The fact that we did not see anything close to the required frazil concentrations was, indeed, anomalous. However, after much scrambling and rechecking, we concluded we had not made a major error and suggested that the fault was in the model. Not being well-steeped in the literature, it seemed obvious to us that only an *in situ* mechanism could account for what the Peace River was doing to both our instruments and to our clients' data expectations. Nevertheless, we could not convince at least two very esteemed river specialists, that we were in touch with, that this was the case…the frazil capture mechanism was apparently too well entrenched to be displaced by measurements with a new and unfamiliar technology. Most importantly, we could not get an early version of the present manuscript published, primarily because of rather hostile receptions by 2 or 3 opposed reviewers who were obviously much more influential than an almost equal number of positive reviewers. Frustratingly, the only offered objections were very vague and centred on the SWIPS calibrations which included perfectly valid measurements on polystyrene frazil surrogates. Those objections now have been, we believe, adequately addressed and supplemented with on-ice field data in a companion manuscript also recently submitted to TC.

There are two distinct camps in contention. One of which believes that frazil capture can somehow produce the large amounts of ice needed to cover a river surface. This situation persists in spite of the fact that, largely due to unofficial circulation of our results, several studies have now been carried out which have verified the predicted presence of extensive anchor ice fields in the Peace River which are hard to explain in terms of frazil capture. Also, as you note, we reference and critique, in Section 4.2, a recent example of the persistence of this alternative point of view which does not address the critical energy balance issue. We did not intend to ignore earlier in situ studies in smaller and probably more complex bodies of water. However, the physical situation appears to be simpler in larger rivers which are, as well, more accessible to both measurements and successful modelling. Just in case, we've gone over the text to be sure it's clear we are not claiming to have "invented" *in situ* anchor ice growth, *anchor ice is released from the bottom, is drifting anchor ice captured by the SWIPS? Can this be distinguished from frazil particles? It is indicated in the text, but do we see it on the echograms?*

Such fragments should give exceptionally strong returns. The reason you don't see such returns on the Echogram is that they would only be visible if we zoomed in on a single (or maybe two) orange (high digital count) pixels surrounded by a few green or blue (low or medium digital count) pixels on a black background. This is because the signal returns from a 4 cm deep range cell at a mid-water height in our profile correspond to the average returns received over the duration of a single pulse by backscattering from targets in an imaginary 0.5 m diameter, 4 cm deep, fluid disk. The returns thus get diluted considerably and their sources are moving at about 1.25m/s with the river flow and so would be unlikely likely to be detected on more than one pulse. Moreover, given pulse durations and repetition rates, sampling at any given water level occupies only 0.01% of a given monitoring interval. That said, we **were** able see individual "anomalously" strong returns in the form of orangey single image pixels. One example of this, on the same scale as the Echogram included in the manuscript, was provided in an ASL Report listed in the references. It was not an overly impressive display. We thought about doing a zoom to highlight an strong return pixel but decided it was not worth the effort since someone would still want us to analyze the pixel statistics to prove we weren't just looking at noise. Instead, we include a reference to above-water video data showing active surfacing of anchor ice fragments.

What level of super cooling was observed at each event in table 1? Was this measured locally, if so how?

The fact that we were applying heat to the instrument package precluded credible measurements of supercooling and we were limited to detecting reaching the zero degree isotherm.

Clarify how these periods were identified (start of section 3.1). You have water temperature and discharge measured at a site 370 km upstream of the SWIPS. How representative is these regarding the location of the SWIPS, e.g., how well did the model simulate the changes in water temperature over this considerable reach?

The choices were the easy part since only 6 of the 7 intervals corresponded to significant periods of subzero water temperatures and only 4 of these preceded consolidation at the SWIPS monitoring site. The large separation between the site and location associated with the discharge and upstream water temperature data was a problem that we had to work around. BC Hydro has been using CRISSP1D together with a similar network of data inputs to guide its flood control and power production strategies with apparent success. That level of success can judged from a Fig. in the 2017 Marko et al. ASL Report (available on Research Gate) which gives the water temperatures as measured at the SWIPS site and as modelled on the basis of atmospheric parameters and 370 km distant water temperature data. We think the results are reasonably impressive but because of the complexity of a 3 dimensional river encountering tributaries along the way, getting a model that could precisely anticipate the timing of supercooling was a bridge too far. In general, the model does pretty well for above-zero temperatures but, as far as we can tell from our acoustic data it cannot get the timing of supercooling exactly right. Consequently, comparisons with model predictions for F(t) required the shifts and artificial tricks employed as described in our manuscript. These adjustments facilitated model/measured comparisons which were of interest to show: the common form of a modelled frazil event and its incompatibility with the observed form as well as the large differences in the expected and observed F(t) magnitudes. We will add a sentence or two just above Table 2 to account for the effectiveness of such adjustments.

Table 2, please clarify the methods used to compute the heat fluxes.

These fluxes were equated to the product of the heat capacity of the water column under a 1 square meter of the river surface with the measured rate of water temperature decrease immediately prior to frazil onset.

Is figure 8 necessary? Could this just have been left for the textual description?

No it wasn't. It can be replaced by a line of text.

Page 10: Last paragraph is interesting – could be expanded with quantification.

I don't think we can add much here. Except for the two intervals for which we can use brief adjustments of the upstream temperature to make the environmental inputs associated with the modelled and observed frazil contents identical, the credibility of the adjusted results arises largely from the fact the shifted and unshifted model intervals are associated with similar average air temperatures and similar time trends. Given that we now know that the model's fatal flaw is ,neglecting in situ anchor ice growth, the important aspects of the model results are their estimation of magnitudes and the incompatibility of their step function time dependences with observations.

Page 16: Is the flow the same in the single and multipeak events?

To within 2%.

Some minor things: - Figure 9. Provide a time axis, I think that would enhance the readability of this figure.

Can be done.

- Provide a proper reference to Topham and Marko (2020). It is a discussion paper in C3 TCD Interactive comment Printer-friendly version Discussion paper the Cryosphere and could be referenced as such. - Provide a complete reference for Ghobrial et al. 2020 The Cryosphere. Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-212, 2020

Can be done.