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Interactive comment on "Analyses of Peace River SWIPS data and its implications for the roles played by frazil ice and in situ anchor ice growth in a freezing river" *by* John R. Marko and David R. Topham

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Review of TC-2020-212: ÂńAnalyses of Peace River SWIPS data and its implications for the roles played by frazil ice and in situ anchor ice growth in a freezing riverÂż

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

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Discussion paper

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.

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.

When 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?

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

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

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

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

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

Some minor things:

- Figure 9. Provide a time axis, I think that would enhance the readability of this figure.
- Provide a proper reference to Topham and Marko (2020). It is a discussion paper in

the Cryosphere and could be referenced as such. - Provide a complete reference for Ghobrial et al. 2020 The Cryosphere.

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