Authors responses to comments posted by Referee #2

General comments and suggestions:

The authors present a well written study which investigates the properties and concentrations of frazil flocs in reaches of the North Saskatchewan River (NSR), Peace River (PR), and Kananaskis Rivers (KR) using a submersible camera deployment called FrazilCam. The manuscript presents in-situ observations of floc shape, size, and concentration which is of use to the frazil and river ice communities for improved modelling efforts. The following general suggestions can be considered by the authors for potentially improving the quality of the manuscript. Specific comments follow these general suggestions.

> The authors thank Referee #2 for the constructive comments and helpful suggestions. We are confident that the recommended revisions will improve the quality of the manuscript. Please find our responses below in blue.

1. The methodology used for classification of flocs was challenging to follow at times. Perhaps adding in a flowchart or process-flow diagram would aid in reader comprehension to better follow all the processing steps.

> We agree and will add a flowchart to the revised manuscript.

2. The heat flux analysis requires additional clarification and minor reanalysis. Namely, description of equations used and some minor addendums to methodology. Please see specific comments.

> We will revise Sec. 4.2 Heat flux analysis to include a description of equations used and justifications for why we chose these equations.

Detailed comments and suggestions:

Study Reaches:

L108-111: Is the data obtained from Kellerhals et al. (1972) the most up to date site data available?

> Yes. To our knowledge it is up to date and is representative of the current characteristics of the study reaches.

L114: The site map is well made and clear.

> Thanks!

Instrumentation, Methodology, and Deployments:

L115 and L126: The study modifies the FrazilCam system developed by MacFarlane et al. (2017). Can the specific modifications from the original system be made more clear in this section? Was the FOV the only aspect modified (L126-127).

> The system was modified to enlarge the FOV and increase the gap between the polarizers. These sentences will be revised to read:
>
> "The camera system was modified to enlarge the FOV and increase the gap between the polarizers to enable larger flocs to pass through and fit within the FOV. The modified configuration has an ~6 times bigger FOV and 1.6 times larger gap compared to McFarlane et al. (2017)"

L147: Which frequency Aquadropp ADCP was used? My concern is for blanking distances, as the river sites have quite shallow depths (Table 3).

> We used a 2 MHz AquaDopp High Resolution Profiler with a blanking distance of 0.1 m. As mentioned in L146, we only used the velocities measured at 60% of the water depth to get estimates of depth-averaged water velocity so the blanking distance should not be an issue. We will revise this paragraph to make it clearer.

Image Processing:

L221-223: Were the preliminary experiments conducted by yourself? If not, please provide some more context.

> Yes, the preliminary experiments were conducted by myself.

L234: 'S' is not defined prior to its use here.

> *S* was previously defined at L205.

L237: Perhaps it should be made clearer earlier in the study that only 1 deployment coincided with an entire 'principal' supercooling event.

> We agree and will add a sentence in Sec. 3 right before Table 3 to make this clear earlier.

L231-264: The addition of a process-flow diagram or flowchart would largely help the reader understand the methodology used. Additionally, it would be useful to consider provided quantitative measurements of how many images were taken (in total), followed by how many were removed at each processing step.

The requested diagram will be added and Table 3 will be revised to add number of images processed for each deployment as suggested. We will also add that images removed during the rinsing period constituted 2~14% of the total number of images.

L255: One key missing piece of information was the specific sampling time used for each deployment. On L131, it is noted that 5 images at 1 Hz every 9,15, or 19s were acquired depending on field conditions. It would be more transparent to describe under what case/deployment each sampling time was used. If sampling times were mixed for a given deployment, this should be also stated and justified.

We agree and will revise Section 3 right before Table 3 to provide the following information: NSR-L1 to L6 and KR-E1 used 5 images every 9s; PR-F1 and F2 used 5 images every 18s; and KR-F1 and F2 used 5 images every 15s, sampling times were not mixed for any given single deployment. Note that, we noticed a typo in the manuscript - the third time interval is 5 images at 1 Hz every 18s, not 19s and this error will be corrected in the revised manuscript.

Heat Flux Analysis:

L273: It is uncommon in heat flux analysis to explicitly consider an albedo in the longwave spectrum as nearly all radiation in this spectrum is thought to be absorbed at/near the surface (shown by your use of a very small albedo of 0.03). It is recommended to remove this.

The albedo of 0.03 in the longwave radiation calculation was calculated for water temperatures of normal ranges (Raphael, 1962). It has been used in multiple recent river energy budget studies in the river ice fields (e.g., Richard et al., 2015; McFarlane and Clark, 2021; Yang et al. 2023a). Therefore, we decided to keep the albedo of 0.03 to be consistent with these studies.

L277: Mean water temperatures are used here for conducting a surface energy balance. While vertical turbulence may be well developed and river depths relatively shallow, some degree of caution should be presented on this matter. Surface temperatures may deviate significantly depending on flow and meteorological conditions. The assumption that the river reach is fully vertically mixed should be stated explicitly in this case.

We agree and will state this assumption explicitly as recommended.

L278-280: Satterlund (1979)'s parameterization relies on data from Aise and Idso (1978) from continental Montanna and is extended with Stoll and Hardy (1955) for measurements in Alaska. Perhaps it may be more prudent to select a more well-used scheme for clear sky conditions shown effective in higher latitude regions of North America (e.g. Efimova, 1961). Key et al. 1996 provide a review on the matter using data from Alaska and the Northwest Territories on their

review of parameterization schemes. It is left to the authors' discretions to keep the current scheme or adopt one of the above-mentioned after reviewing the noted references.

Efimova, N. A. (1961). On methods of calculating monthly values of net longwave radiation. Meteorol. Gidrol., 10, 28-33

Key, J.R., Silcox, R.A., Stone, R.S., 1996. Evaluation of surface radiative flux parameterizations for use in sea-ice model. J.Geophys. Res. 101 C2, 3839–3849

> Thank you for this suggestion. In a previous study (Yang et al., 2023b), the co-authors investigated various formulas used to calculate downwelling longwave radiation and the latent and sensible heat fluxes during freeze-up on the North Saskatchewan River. Formulas for the downwelling longwave radiation for clear-sky atmospheric emissivity and cloud effect were assessed using direct measurements. The combination of formulas used in this study were the ones that provided the most accurate results in Yang et al (2023b). Therefore, we decided to keep the current scheme.

L281: Where was Bowen's ratio obtained from? In addition to the above-mentioned, please describe equations used for the flux analysis within this section. It provides the reader with the information readily, rather than having to access several other sources to understand the approach taken.

> The Bowen's ratio is given by:
>
> $$B = \frac{C_a P}{0.622 l_v} \times \frac{T_s - T_a}{e_s - e_a},$$
>
> where C_a is the specific heat of air; l_v is the latent heat of vaporization; P is the atmosphere pressure; T_s is the surface water temperature taken the same as the measured water temperature. T_a is the air temperature; e_s and e_a are the saturated and actual vapour pressure of water, respectively.
>
> We will revise this section as suggested to include all the equations used for the heat flux analysis as well as justifications for why we chose these equations.

Results:

Floc Shape, Size and Concentration:

L303: It was quite interesting to record such a large floc size (99.69mm). As I understood, the FrazilCam in this study had an increased FOV relative to its predecessor. Would recording a max floc size such as this in KR-E1 suggest perhaps the FOV may need to be further increased? Perhaps there may be potential for biasing floc sizes too low, as larger flocs that are unfavourably oriented interact with the polarizers and break.

Yes, this does suggest that further increases in both polarizer size and the gap between the polarizers might be needed to allow even larger flocs to be imaged. We will add a short discussion to the revised manuscript regarding the possibility that the current system may be under-sampling the large flocs.

Floc Size Distribution:

L319: You note that lognormal distributions are reasonable fits for the distributions. Would you be able to provide a quantitative measure of the fit for each of the histograms?

This statement was based only on visual examination that a theoretical lognormal distribution was a reasonable fit to the observed distributions. As suggested we attempted to provide quantitative evidence for this statement. We applied the Chi-Squared test and none of the distributions passed the test with a 5% significance level. This could be due to various reasons. First, as discussed in Sec. 5.2, the use of cut-off size to eliminate sediment particles produces a sharp cut-off in the distributions, the effect is more pronounced in deployments that recorded smaller floc sizes. Second, the small number of samples in some deployments resulted in noisy size distributions making it unlikely that they would be a good quantitative fit for a smooth lognormal distribution. We will revise the discussion section to make clear that our conclusion that the measured distributions are approximately lognormal is based on visual comparisons and that the Chi-Squared test did not statistically confirm the lognormal distribution.

Time Series:

L332: It is understandable that not all sites are presented within the contents of the manuscript. I do however believe that the reader is left curious as what the other sites might have looked like. Perhaps it can be considered to add in the other deployments data (similar or simpler versions to Figure 8,9,10) as supplementary data for further transparency.

This is a good suggestion and we will provide the other deployment data as supplementary data as recommended.

L357-358: The usage of hourly component flux data for correlating floc properties and concentrations is a bit questionable given the timescales of the deployments (~1-3hr). It is described between L155-167 where this data is obtained from. If these sites (minus the NSR reach) have sub-hourly data that can be used for the heat flux analysis, please consider updating.

Unfortunately, we do not have sub-hourly data for the KR and PR sites to update our results. Please note that the hourly heat fluxes were plotted in the time series only to illustrate the trends of the heat flux variations and to identify the dominant component during the deployments. We think this is useful because it facilitates better understanding of supercooling which is directly related to the surface heat flux. We did not try to correlate the hourly heat flux time series data with the floc properties or concentration time series data.

L375, 380, 383: Figures 8,9, and 10 would benefit from the addition of air temperature data.

We agree and will add the air temperature data as recommended.

Discussion:

L483-484: A larger limitation would be the use of hourly data rather than neglecting surface conditions and sediment heat fluxes. This is likely too coarse for the intended goal with correlating net surface heat flux with floc properties. This can be considered going forward for future studies on the matter.

We agree that this is a limitation of the study and we hope in the future to deploy our own weather stations to obtain meteorological data at faster sampling rates at these more remote sites. We did conduct a correlation analysis using only the heat flux data with 10-min resolution from the six NSR deployments and still did not find any significant correlations. Therefore, including the deployments with the 1-hour heat flux data did not change the current result. We will describe this analysis using only the 10-min data and explain that there was still no correlations found in the revised manuscript.

References

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