Response to Mark Loewen :

First of all, the authors would like to thank you for the constructive reviews, that helped us improve the quality of the manuscript. We have much appreciated your help in guaranteeing the accuracy and precision of assumptions and quantities mentioned in the text, as well as encouraging the clarity of the definitions and detailed explanations. We corrected the manuscript accordingly. Please find below our responses to your remarks.

Best regards,
Fabien Souillé, on behalf of the authors.

Figure: In all figures with multiple plots you should label the plots (a), (b) etc. to avoid confusion.
Absolutely. We added (a), (b) etc. the to plots.

74: Change “supposed to be” to “assumed to be”.
As recommended, this was corrected.

191-193: Explain the reasoning for fixing the number of initial particles at zero for classes exceeding a radius threshold.
There has been various approaches in previous works concerning the choice of the initial distribution for particle sizes. Introducing a threshold r0 and considering it as uncertain allowed us to study the influence of this parameter since different authors used different r0 without really justifying why. To clarify this point, I added another reference in the text, and explained that our goal was to stay close to previous works and check if this parameter actually matters.

In practice we could have tried other methods such as log-normal initial distribution, but we lack data to support this modeling choice. This can for example be the object of future investigations to study the influence of the distribution shape itself on the uncertainty and sensitivity analysis.

262: Define UQ.
As recommended, we added the definition.

Sec. 3.3: I recommend this be moved to an appendix.
Our second referee, Dr David Rees Jones, also commented that this section should be summarized and moved to appendix. We accordingly moved most technical details of section 3.3 in appendix. It indeed simplifies the reading, thank you for this recommendation.

384: I agree the uncertainty in the initial volume fraction is very large.

500: In Fig. 5 time is plotted in seconds so referring to minutes in the text is inconsistent. Also please check the value of 0.1 C I think it is inaccurate.
Absolutely, thank you for this remark. We replaced minutes by seconds in the text, and 0.1 by the precise value of 0.097°C.

511: Are you referring to the median value of the time of maximum supercooling here?
Exactly. This was not clear in the text, I clarified it.

514: I think for experiments this argument is valid but I am not convinced this is true in rivers. There are very few reliable measurements of dissipation in rivers available and the uncertainty could easily be an order of magnitude or larger.
Thank you for raising this issue. We corrected this in the text by underlining that in the absence of data, we could still use approximations to estimate turbulent dissipation in rivers (such as equation (25), or more generally k-epsilon models in hydraulic codes). It might of course not be as reliable as measurements but still provide a rough idea of the order of magnitude for epsilon. In contrast, for initial concentration, we don't have any model to estimate it, which makes it more difficult to set in models.

518: Here you refer to "the initial distribution parameter" and it would be helpful to state also that these are C0 and r0 .
I added C0 and r0.

520: In Table 1 C0 and r0 are categorized as "Initial conditions" and here they are referred to as initial distribution. I find this confusing.
For the MSC model, the initial distribution is part of the initial condition of the system. One has to provide an initial volume fraction value for each class of radius and this is done via C0 and r0 in the presented methodology. But more generally, I wanted to stress that there is no particular reason to keep C0 and r0 and the presented methodology to set the initial distribution. We could use a log-normal distribution for example, which would be characterized by other parameters. I added precision on that.

521: You write "At the recovery" and later refer to a "recovery time" and these are both too vague. You define the "recovery phase" previously but this includes all time after steady state is reached. So clear terminology and clear definitions are required. In the same line you write "the parameters of secondary nucleation and flocculation processes". Please list all of these parameters here.
This was indeed not precise enough. I added a definition for the recovery time, and recovery phase in section 2.5. I also listed the parameters for secondary nucleation and flocculation.

523: How did you observe interactions between parameters?
In the diagrams of first order Sobol indices, the blank space separating the last First order Sobol index and 1 actually corresponds to the sum of all interactions (high order Sobol indices). So by seeing an increasing blank space in these figures, we can see that interactions play a important role. Another way to see the increase in interactions is to observe the evolution of Total Sobol indices in Appendix F, G, H and I. To clarify this in the manuscript, I added explanation and reference to Total Sobol figures. I also added more precision in Section 3 to explain what First order and total Sobol indices stand for.

Figure 8: Supercooling of -0.2 C is quite extreme so some comments on these values are required. In the right plot please also comment on the fact that for P95 the maximum supercooling is not reached even after 3000 s.
I added the following explanation in section 5.3 : this is due to the highest values of the buoyancy velocity combined with low thermal growth rate. High gravitational removal withdraw a large amount of the frazil volume fraction from the water and thus limits the amount of latent heat release to water that would increase its temperature. This combined action impacts both the time to maximum supercooling and the amount of supercooling.

546: List the parameters please.
Done.

552; Replace “coherent” with consistent?
I changed coherent to consistent.

554-555: Excellent point I agree.

559: Spelling changed to predicted.
Corrected.

560-562: This is a very limited discussion of Figure 10. Seems too brief - presumably there is more to discuss.
I added reference to similar observation made by Carstens (1966) and Ye et al. (2004) and discussed the figure more extensively.

571-573: Awkward wording, please rewrite this sentence.
I rephrased and hope this is clearer.

Figure 11; Add info to the caption to explain the symbols i.e., dots, error bars etc.
I added details in the caption.

582: I think you mean quantitative here not qualitative.
Exactly, corrected.

591-599: Excellent discussion here.
Thank you.

599: Define API.
I added the definition.

632: Replace relevant with suitable or promising?
Replaced by suitable.

640: I found it very interesting that you found that the turbulent dissipation rate plays a major role. Laboratory studies have found that the mean particle size varied with dissipation rate but the results are inconsistent. Can you use your model to examine this?
In the present work, we just looked at the total volume fraction, but the methodology could definitely be extended to the analysis of class volume fraction, the evolution of the output distribution or at least statistical moments of the output distribution such as the mean radius.
The MSC model could definitely be used to explore this by running simulations with different dissipation rates and analyse the evolution of the mean particle size.
And more generally, I think using Monte Carlo simulations with the MSC model could be used to help define new models for class interactions, like flocculation.

640-641: I do not agree that the dissipation rate is often appropriately quantified. Reliable measurements of dissipation rates in rivers are virtually non-existent and even in the lab it has not been accurately measured very often.
Thank you for raising this issue, also linked to your comment on line 514. I rephrased to reflect the idea that It could at least be modeled (even if it is not properly quantified).

645-646: You write “The long-term evolution of the system also showed increasing interactions between parameters, which can be explained by the balance in the physical

processes involved in class interactions". This was not clear to me since I do not think you explained this well in the paper.
Yes, this needed clarification.
From a computational standpoint, at steady state, the shape of the distribution (class volume fractions) is stable and the stability is due to the balance of thermal growth with secondary nucleation and flocculation. I think this equilibrium explained the observed interactions. I added an explanation in the text, as well as a reference to appendices for interactions estimation. Also, I rephrased « which can be » to « which could be » as it is more a possible interpretation than a direct result. This could be more precisely quantified by comparison to simulations where one of the terms (either flocculation or secondary nucleation) is set to 0. Using class volume fraction as model outputs could also help to better understand this.

652-653: Your conclusion regarding the rise velocity is a very significant result – well done!
Thank you. I was quite surprised by this result. I was expecting an influence, but not that important compared to other parameters. Now my dream would be to have unlimited data with clear vertical distribution of frazil so we can validate non-well mixed numerical models including buoyancy velocity.