Journal: Natural Hazards and Earth System Sciences of EGU
Response to Reviewer 2 Comments
Manuscript: egusphere-2025-6452
Ice jam formation at river confluences: comprehensive field investigation and comparison to laboratory-derived predictive equations

The authors thank Reviewer 2 for the thorough and constructive review. The comments have led to improvements to the manuscript. Below, each comment is addressed in turn.

General Comments – Manuscript structure and model validation

Comment: This very interesting manuscript reports field observations of ice jam formation at 4 confluences over 4 years in Quebec, Canada. Field monitoring including water level, water temperature, and ice thickness measurements were conducted and combined with existing weather datasets. The study goal to compare to laboratory studies of Ettema et al. and validate equations describing formation of ice jams.

Response: Thank you for this positive assessment. We would like to clarify that the study encompasses three equally weighted objectives: 1) to characterize the interaction between tributary and main river ice during breakup; 2) to quantify hydrometeorological factors controlling breakup sequences and jam formation at confluences; and 3) to evaluate the applicability of laboratory-derived equations of ice jam formation to field cases. The evaluation of the Ettema & Muste (2001) equations is therefore the third objective of a broader field investigation, rather than the sole goal of the manuscript.

Comment: The description of jam formation at the study sites is clear, but in places the manuscript reads more like a case study. To move beyond this, it would be helpful if the structure of the manuscript were clarified and in places the analysis were further developed to clearly identify insights from the data for model validation and put forward specific directions for future work.

Response: Thank you for this comment. To address the concern regarding manuscript structure, a sentence has been added at the end of the Introduction explicitly describing the organization of the manuscript relative to the three objectives. The Results section now presents the breakup dynamics observed at each confluence across the four study years, addressing objectives 1 and 2. A dedicated new section (Section 7) following the Results brings together the comparisons to the Ettema & Muste (2001) laboratory framework for all four confluences, addressing objective 3. This restructuring separates the field documentation from the model validation, making it easier for the reader to follow the progression from field observations to equation testing. Regarding the development of specific directions for future work, the Discussion already identifies several targeted directions, including the validation of the flow impact equation at STA-BDN as a basis for further application of the laboratory framework at similar sites, the development of data-driven models trained on the dataset collected in this study, and the development of river ice hydraulic models capable of simulating ice dynamics at confluences. A global sensitivity analysis to assess the relative effect of each parameter on the model outputs has been added as an additional specific direction to complement these existing priorities.

Comment: Because the core goal appears to be to validate the Ettema lab studies, I suggest to make comparison to Ettema an organising factor of the manuscript. For example, were the confluences chosen to find specific processes? Did the authors observe the processes they expected to, or not observed some processes they anticipated?

Response: Thank you for this suggestion. As clarified above, the validation of the Ettema & Muste (2001) equations is the third of three equally weighted objectives. Regarding site selection, a sentence explicitly state in Section 2 that the four confluences were selected based on two criteria: their documented history of frequent ice jam flooding causing damage to nearby urban communities, and their diverse morphological configurations. Their geographic proximity to Université Laval enabling sustained four-year multi-site monitoring was also a criterion not stated in the paper, because it was considered less relevant than the other two criteria. The sites were not selected with the intent of pre-assigning specific laboratory processes to specific sites, so the mechanism identified at each confluence emerged from the field observations themselves.

Comment: The site description in the Results is clear, but it is difficult to follow the model validation between sites. It may make sense to order the site descriptions according to complexity, allowing clear development of model validation, or to report the site-specific results in the Results and develop a targeted Discussion focused on model validation.

Response: Thank you for this structural suggestion. We have restructured the manuscript accordingly. All site-specific breakup dynamics are now presented in the Results (Section 6), and a new dedicated Section 7 brings together all comparisons to the Ettema & Muste (2001) laboratory framework across the four confluences. The site ordering follows a geographic rationale: STA-BDN is presented first as it is the only confluence studied on the Sainte-Anne River and also the site for which the most complete comparison with the laboratory-derived equations was achievable, including full quantitative validation of Equation 5. The three Chaudière River confluences are then presented from upstream to downstream along the river: CH-DL (km 103.4), CH-FA (km 97.5), and CH-BSV (km 75.3). This geographic ordering is consistent with the hydrological context of the Chaudière watershed and reflects the natural downstream progression of breakup processes along the river.

Comment: Finally, it would be helpful if the uncertainty in the measurements and calibrated parameters was developed in more detail to clearly assess the model validation. The Discussion were focused on a clear assessment of points to take forward based on this work, as at present this is a very broad range of possibilities.

Response: Thank you for these comments. Regarding uncertainty, the sources of uncertainty are already discussed in detail in the Discussion section. Measurement uncertainty is addressed in the first paragraph, covering the temporal resolution of monitoring equipment, the spatial distribution of instruments, and the subjective interpretation of ice states from time-lapse cameras and drone flights. Hydraulic modeling uncertainty is addressed in the second paragraph, covering the calibration of roughness coefficients and ice thickness values, discharge routing effects between measurement points, and the data limitations that prevented hydraulic model development at CH-BSV. The third paragraph explicitly acknowledges the assumptions required to extrapolate the laboratory-derived equations to field conditions, and the wide range of calibrated parameters in Table 4 is discussed as likely compensating for unmodelled physical processes rather than reflecting true physical variability (Beven, 2006). To further strengthen this section, the following sentence has been added at the end of the second paragraph: "Future work will focus on a global sensitivity analysis to assess the relative effect of each parameter on the model outputs, which would allow a more rigorous quantification of the contribution of each uncertainty source to the overall model validation uncertainty."

Regarding specific points to take forward, the Discussion already identifies several targeted directions. The third paragraph highlights that the validation of the flow impact equation at STA-BDN demonstrates the transferability of laboratory-derived force balance approaches to natural confluences when appropriate morphological and hydraulic analogues exist, pointing to further application of this framework at similar sites as a priority direction. The fourth and fifth paragraphs present the preliminary empirical relationship between the normalized discharge ratio and CDDT, with site-specific trends described for each confluence, and identify the development of data-driven models trained on the dataset collected in this study as a concrete direction. The final paragraph focuses future work on two specific priorities: the development of river ice hydraulic models capable of simulating ice dynamics at confluences, and the development of data-driven prediction models. The global sensitivity analysis mentioned above constitutes a third specific and actionable direction, complementing these existing priorities.

Comment 2 – Line 1
Comment: this is a key reason to study ice jams, it would be helpful if additional details were included in Introduction. For example, are the major historical incidents of flooding due to ice jams or any annual figures of damages in Quebec?

Response: Thank you for this suggestion. The first sentence of the Abstract has been revised to incorporate quantitative context on the economic impact of ice jam floods in Quebec, stating that annual government expenditures related to ice jam floods average 70 million $CAD in Quebec alone (Ouranos, 2023).

Comment 3 – Line 16
Comment: This portion of the Abstract seems to generalise observations across sites, but the Results are very site-specific with different mechanisms for ice jam formation occurring at different sites. It could be helpful to set out in the Abstract that the mechanisms observed occurred at different confluences.

Response: Thank you for this observation. The Abstract has been revised to explicitly attribute each observed mechanism to its respective confluence. The revised text now states that at STA-BDN, a flow impact mechanism consistently produced ice jams when the ice-free tributary flow restricted ice passage in the main river; at CH-DL, the presence of a downstream ice control structure maintained a stationary ice cover at the confluence, forcing a jam to form on the tributary; and at CH-FA and CH-BSV, morphological controls including depositional bars, islands and bridge piers at the tributary mouth prevented ice evacuation at the confluence.

Comment 4 – Lines 19-20
Comment: This summary seems overly general as for much of the Results, comparison is not possible due to complexity of the field setting or the spatial scale of the jam formation mechanism.

Response: Thank you for this comment. The concluding sentence of the Abstract has been revised to more accurately reflect the variable success of the model comparison across sites. The revised text now states that laboratory-derived equations could be used to predict jam formation at STA-BDN, where the governing mechanism was well-defined and hydraulic parameters were adequately constrained, with bank shear stress values ranging from 1.15 to 16.32 N m^{-2} consistent with values reported in the literature. At CH-FA, the equation was also applied and correctly predicted jam formation, though the large discrepancy between calculated and equilibrium ice thicknesses suggests the need for improved integration of discharge parameters into the equation. At other sites, the complexity of the field setting or data limitations limited the comparison with the laboratory framework to a qualitative assessment.

Comment 5 – Line 25
Comment: As above, it would be helpful if a bit more context could be provided motivating the study of ice jams due to the impact of them on society and the environment.

Response: Thank you for this suggestion. Some sentences have been added to the first paragraph of the Introduction to provide quantitative context on the economic impact of ice jam floods.

Comment 6 – Line 45
Comment: This paragraph provides key information describing the theoretical basis of the study, the lab-derived observations and models by Ettema et al. At present the information is somewhat difficult to follow. It would be helpful if the different jam formation mechanisms were organised using a table or list so the reader can easily follow along.

Response: Thank you for this suggestion. The seven ice jam formation mechanisms identified by Ettema & Muste (2001) have been reorganized as a numbered list grouped by the three controlling factors: ice conditions (processes 1–3), hydraulic conditions (processes 4–5), and morphological conditions (processes 6–7).

Comment 7 – Lines70/Lines80
Comment: It is not clear to me why these specific sites were chosen. Were the authors expecting to see a range of jam formation processes among them?

Response: Thank you for raising this point. As described in the response to the general comment above, a sentence explicitly state in Section 2 that the four confluences were selected based on two criteria: documented history of frequent ice jam flooding causing damage to nearby urban communities, and diverse morphological and hydraulic conditions. Their geographic proximity to Université Laval enabling sustained four-year multi-site monitoring was also taken in consideration but is not stated in the paper because it is considered less relevant than the other two criteria.

Comment 8 – Table 2
Comment: ‘Ice’ is a vague descriptor—I assume this refers to ice thickness measurements. Could this be described in more detail, perhaps by defining a variable to use in the table header.

Response: Thank you for this remark. The column header 'Ice' in Table 2 has been replaced with 'Ice obs.' to better reflect that these observations refer to ice state and surface concentration, rather than ice thickness measurements. The sentence introducing Table 2 in the text has also been revised to clarify that ice observations refer to ice state and surface concentration, as defined in Section 3.2.

Comment 9 – Section 4

Comment: The structure of this section would be easier to interpret if a table or list were used in the Introduction to clearly set out the previously observed processes and perhaps references for existing model equations for them. I realise this section comes before the Results, but it is a bit jarring to only see equations for some processes. Perhaps it would make sense to state that only processes 3,5,6 were observed and so Section 4 describes only these.

Response: Thank you for this comment. Section 4 now opens with a sentence explicitly stating that Ettema & Muste (2001) only developed predictive force balance equations for three of the seven mechanisms: process 3 (merging ice runs), process 5 (flow impact), and process 6 (confluence bars). Section 4 therefore focuses exclusively on these three processes. For the remaining mechanisms, no predictive equations have been published in the literature and the comparison with the laboratory framework presented in the new Section 7 is qualitative.

Comment 10 – Results

Comment: the rationale for the structure of the results is unclear. I can see that the Results is organised by site, but why were the sites ordered in the current manner? To clarify the overall comparison, I would suggest either ordering the respective sites by increasing complexity, so that the clearest comparisons to existing models appear first, or to report the site results first, and then develop a comparison to existing models in the Discussion.

Response: Thank you for this comment. As described in the response to the general comment above, the site ordering follows a geographic rationale: STA-BDN is presented first as it is the only confluence studied on the Sainte-Anne River and the site for which the most complete comparison with the laboratory-derived equations was achievable. The three Chaudière River confluences are then presented from upstream to downstream along the river: CH-DL (km 103.4), CH-FA (km 97.5), and CH-BSV (km 75.3). A brief introductory sentence has been added at the beginning of the Results section explicitly stating this ordering and its rationale. Furthermore, to directly address the reviewer's suggestion of separating the field results from the model comparison, a new dedicated section (Section 7) has been created following the Results. This section brings together all comparisons to the Ettema & Muste (2001) laboratory framework across the four confluences, separating the field documentation from the model validation and making it easier for the reader to follow the progression from field observations to equation testing. The content previously presented as subsections "Comparison to laboratory-derived formulations" within each site description in the Results has been moved to this new section.

Comment: In each site description, I appreciate the narrative of breakup procedures but I am not sure why a given year was chosen for the narrative. I realise that the details of the other years is provided in the supplemental material to conserve space, but a bit more developed analysis in the description would be helpful. Could a bit more detail be provided so that the reader can understand if this is a typical year, with a short summary what variations were observed on the other 3 years.

Response: Thank you for this comment. The year chosen for each site narrative was selected as the most representative example of the dominant ice jam formation process observed at that confluence: 2021 for STA-BDN, 2022 for CH-DL, 2023 for CH-FA, and 2024 for CH-BSV. This selection also reflects the chronological order of site presentation. Regarding the variations across years, the first paragraph of each site's description of breakup dynamics already provides a summary of the typical breakup sequence and highlights the key variations observed across the other three years, and jam formation outcomes. Where a year deviated significantly from the typical pattern, such as the thermally dominated 2023 breakup at CH-

DL which did not produce a significant ice jam, this anomaly is explicitly noted and explained. The detailed results for the other years are provided in the Supplementary Material.

Comment: As in the Summary above, inclusion of error in the Results (ex. estimated uncertainty for parameters in Table 4, Table 5, Table 6) would be helpful in understanding the model validation.

Response: Thank you for this comment. As described in the response to the general comment on uncertainty above, the sources of uncertainty are discussed in detail in the Discussion section, covering measurement uncertainty, hydraulic modeling uncertainty, and parameter uncertainty. The wide range of calibrated values in Table 4 is explicitly discussed as an indicator of this combined uncertainty, and a sentence has been added before the Table 4 directing the reader to the uncertainty discussion for the interpretation of the calibrated parameter ranges. Regarding Tables 5 and 6, the parameters in these tables are either directly measured in the field, calculated from field measurements, or deduced from the force balance equations. Their uncertainty is therefore inherently linked to the measurement and modeling uncertainties described in the Discussion. Sentences have been added following Tables 5 and 6 respectively directing the reader to the uncertainty discussion for the interpretation of these values.

Comment 11 – Table 4

Comment: Here, it would be helpful if the authors could comment using their expertise from a field perspective as to why these parameters are difficult to obtain in the field, requiring calibration. As a 3-parameter calibration necessarily limits the usefulness of the model, perhaps a discussion of future experiments or field measurements could be helpful in terms of looking to future research directions in order to understand these terms better.

Response: Thank you for this suggestion. A paragraph has been added before Table 4 in the Results section explaining the field measurement challenges associated with the three calibrated parameter types. The under-ice Manning roughness coefficient depends on the highly irregular and spatially variable texture of the ice underside, which forms under dynamic conditions and cannot be measured non-intrusively. The bed roughness can in principle be estimated from grain size distributions under open water conditions, but its simultaneous calibration with the under-ice roughness means that both parameters are adjusted to match a single observed water level time series. Because both parameters influence total hydraulic resistance in similar ways, they cannot be fully decoupled, a well-known equifinality problem in hydraulic modelling (Beven, 2006). The discussion of future work to address these limitations has been kept in the Discussion section, where a global sensitivity analysis to assess the relative effect of each parameter on model outputs was identified as priority directions for future research.

The authors hope that these revisions adequately address all reviewer comments. We remain available to provide any further clarification if needed.