Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2016-308-RC2, 2017

Natural Hazards and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "River predisposition to ice jams: a simplified geospatial model" *by* Stéphane De Munck et al.

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Received and published: 23 January 2017

Paper title: River predisposition to ice jams: a simplified geospatial model

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General comments:

Note that another review has been submitted before mine, but I have not read it before performing my review. BT

This paper presents a very interesting and original model that uses qualitative, geospatial information to quantitatively identify channel locations where ice jams could form. Its development and calibration is supported by an ice jam database. Overall, it seems that there is a great potential for the development of a model that could be used to

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Discussion paper

identify potential ice jamming sites and this could be combined with a river ice breakup forecasting model.

However, the model overlooks or simplify a number of key ice jamming parameters, factors, processes, and information that may limit its reliability and the paper really presents what appears to be the early development stage of an acceptable model. It seems that the authors globally lack in experience and confidence (a lot of sentences seem defensive) and the following points (below) should be considered to improve the next versions of the model (the actual knowledge about river ice processes has only be partially considered). I consider that the actual version of the model is almost dangerous to use by public security services or for flood insurance purposes.

At this point, I am not sure if I recommend (1) that the authors should make multiple technical changes to the paper and include a discussion that mentions the many limitations of the model in its actual form or (2) that the authors should present a new paper with a more advanced version of the model that would address and include most of the following points. I would tend to vote 1 because I consider that the model is original and represent a step forward in the field of river ice and flood forecasting. In this case, I would encourage the authors to present an improved version of the paper with a serious discussion and to present, in the years to come, an improved version of the model that would potentially include a completely new model structure.

1. The authors do not mention that they have observed ice jams and do not refer to any experience in the field (e.g., to verify the sites presented at Figures 8 – 10 or to look under bridges if pillars can be pointed out for ice jamming) . Therefore, this research is only based on theory and the authors cannot really confirm that the model is reliable. Beyond the government ice jam data base, the authors should have conducted a complete historical research and confirmed that the mentioned ice jam dates corresponded to specific hydro-meteorological events. This type of data base often confuses ice jams with other ice processes that generate winter or spring flooding (e.g., anchor ice and hanging dams). Moreover, at locations where observation is not

easy, where there is no societal vulnerability, or where the jamming and release occur at night year after year, ice jams may have gone unnoticed (as somewhat mentioned in the paper).

2. The authors refer to particular factors influencing ice jamming, but do not seem to understand all the physics that link these factors with ice jam processes. The authors never refer to the distinction between the toe and head of an ice jam and they barely mention something about their potential length (that can be much greater than 250 m and therefore extend in sections that have nothing to do with the initiation of the jam). It is crucial to point out that the parameters influencing ice jamming sites refer to the toe (initiation site), which can be hundreds of meters or even kilometers away from the ice jam observation site. This can influence the results of the research positively or negatively.

3. A number of parameters such as channel widening (dissipation of the energy and ice run stalling), the presence of hydraulic structures (weirs, dams, dam reservoirs, etc.), and the presence of a tight, single bend (not a meander) have not been mentioned in the study and could help reducing false-negative errors. On the other end, it seems that channel narrowing is assumed to generate ice jamming but in some cases, the concentration of energy actually favors the transit of an ice run. From my point of view, trying to fit many parameters in a "narrowing equivalent" will limit the potential development of the model.

4. Obstacles and gradient variations could explain a significant ratio of ice jams. This may require a more sophisticated spatial analysis that may become tedious to automatize.

5. One important parameter affecting ice jamming is the potential quantity of ice, i.e., the contributing reach. If there is not enough ice to produce an ice jam that can affect the floodplain, the jam may remain unnoticed. The most critical jamming sites are located downstream of long sections where an ice run would simply not stop. This has

to be mentioned here and potentially included in a future version of the model.

6. The model could consider factors that prevent the formation of an ice jam (e.g., immediately downstream of a reservoir) and the model could gain in accuracy and reliability.

7. In the end, at this development stage of the model, for Quebec, the data base itself could represent a more reliable tool to identify potential ice jamming sites than the model calibrated with the data base.

Specific comments:

Abstract:

There is no introducing context in the abstract.

Line 7: “any” should be moderated. The model has been tested on three rivers only and a number of parameters and factors are not considered.

Line 8: Remove “up”

Lines 11 and 13: Should be “was” and “were”

Lines 16-17: I am not sure that talking about “false positives” is pertinent here. These are not really errors.

Introduction:

Line 19: “emerge” may not be appropriate

Line 19: why using “specific”

Line 21: “precipitation” should be “rain” or “runoff” could simply be used

Line 21: Why “partial”? Mid-winter events can be quite “complete” from my point of view, depending on the rain and breakup intensity

Line 22: I would say “can be” instead of “are” [. . .socio-economically costly]. Not all ice

jams have economic consequences. Additional references could be mentioned.

Line 25: More references could be added. That of Bergeron et al. is a possible one, among others.

Line 26: Rephrase

Line 31: I would say “relatively frequent flooding”

Figure 1: Second part of the Figure may not be necessary. There is a lot of empty space in the map north of Quebec City that could be used to increase the size of the legend or to rearrange the ratio of each sub-Figure. Please confirm that the L’Assomption River watershed is the right one. It seems that it is in contact with the St. Lawrence along 80 km. . . The southern part of the St-Francois River could be indicated approximately.

Background:

This section should be reorganized: The authors should mention that an ice jam can form because of congestion or because of an obstacle. The use of a transport capacity in the literature only represents one simplified interpretation that has been overused here. Most of this section refers to congestion processes as if an ice jam could only be the result of an unimpeded ice run (Jasek 2003) that slows down and stop. Indeed, an important portion of ice runs encounter a physical obstacle (such as an intact ice cover mentioned at the end) and suddenly stop. This has nothing to do with congestion or a “reduction in the ice transport capacity”.

Line 39: I am not sure if Beltaos would refer to “volume of ice”. I believe that it would be the “ice discharge”.

Line 41: “were” should be “are”

Line 41: Authors should seek additional references. The books “River ice Jams” of “River ice Breakup” are potential sources of complementary information.

Lines 43-44: The authors should mention that there are different types of islands. They can be naked bars, vegetated bars or emerging rock outcrops (vegetated or not) and not all of them are associated with a break in the channel gradient. From my point of view, the presence of an island is associated with ice jam in part because the flow can bypass the congested channel and release the pressure on the impeded ice run, therefore leaving an ice jam on one side of the island. The authors only mention “narrowing” as the basic process to identify potential ice jamming sites.

Lines 45-47: “close” to each other. Not all bridges present pillars and pillars are often profiled to minimize to effect on flow conditions and ice transport capacity. Also, bridges are often build at natural (or artificial) narrows that already represent a limitation for ice mobilisation. This may affect the result of the study. Also, the flow often accelerates under a bridge because of the smaller river width and ice runs could easily transit that these locations.

Lines 48-50: Not well explained. The basic process may be that the flow along the concave bank drowns while the ice floats. Also, the authors mentioned that sinuosity may “initiate” a jam. This is correct, but it would mean that the first bend of a meandering reach is more likely to cause jamming that the last one. The authors could include this (or mention that this has been or should be considered) in their study.

Line 51-56: I would replace “narrow” and “wide” by “small” and “large”. Remove “from precipitation”. I would say something like “A quick hydrological response in tributaries may trigger an early breakup and send an ice run into the main channel”. You refer to the milder gradient of the larger channel, but it is not very important if the ice cover is intact in the main channel. Also, note that the gradient is not part of this study. Two ice runs that meet at a confluence at the same time is quite unlikely. Is this what the authors refer with “merging ice runs”?

Line 58: “impetuous” could be “power” or “energy”.

Lines 57-59: This is one of the most important parameters explaining ice jamming.

Lines 60-62: There are many types or gravel bars in gravel bed rivers. The authors only mention two types here (point bars or side bars). I am not sure that I agree with the reasoning presented: gravel bars are usually mobilized when the flow increases and stabilize when the flow decreases. How could they form and migrate if there was never a potential for transport? The first part of these lines does not need a reference as most people know about bars. It is really the second part of the sentence that needs the support of a reference.

Lines 63-64: This refers to the slope and should be presented with lines 57-59, together with reservoirs and lakes.

Lines 65-66: The authors should refer to the concept of an impeded ice run here (Jasek ,2003, or Jasek and Beltaos, 2008). This is the only parameter that does not directly refer to the morphology, but it is very important. This is why I would reorganize this entire section.

Line 68: Please mention what river and what is the dominant morphology?

Line 69-70: This is important because the authors use this single idea of the combination of two ice jamming factors for their model. I understand the need to simplify reality, but I am not sure that one publication can justify this choice.

Lines 70-74: I would refer to Bergeron et al. here. Note that this is already partially mentioned earlier.

Line 74: “However, in the present study”. . .

Line 74: I would remove “(mostly stable over time)” because you mention this two line below

Line 74: “will be” should be “are”

Line 77-78: I would express this differently. The word “dynamic” in the river ice literature usually refers to processes such as ice runs, ice jams and ice dams. Note that the depth

can be linked to the morphology and the cover characteristics as well (e.g., Turcotte and Morse, 2013). If the authors believe that the presence of bars is important, well their emergence is completely linked to the depth and discharge!

Methods:

Line 87: How does the 250 m in length compares with the channel width? Why not using a variable length that depends on the width of the channel or the homogeneity of the morphology and alignment? I guess that this would be complex for automatic interpretation to be performed and it would not fit with the title of the paper.

Line 88: What can be said here about the size of ice jams if the data base does not include such information?

Line 89-91: Is this precise enough to document parameters such as narrowing?

Line 92: Islands: Does the model differentiates bars and stable islands?

Line 92: rapids: This is very important and has not been mentioned before. Ice jams almost never initiate in rapids but often at the end of rapids. Does this includes riffles or just rapids?

Line 93: This is not very reassuring: The authors should mention that a width less than X m could not be included in the model for spatial information accuracy limitations. Then, it means that the model is actually not adaptable to small rivers.

Lines 100-102: I understand that the model is simplified for practical reasons. However, as noted in the general comments and as the authors mention at the end, these four factors are linked to ice jamming processes for distinct physical reasons.

Line 104: “will focus” should be “focuses”

Line 104-105: About the secondary channel presenting a more competent ice cover: I do not agree and the authors do not refer to any study to support this. From my point of view, there could be less (or no) ice in the secondary channel and at some location,

the secondary channel plays a determinant role in the ice jamming initiation process.

Line 105: “The model also assumes”

Line 105-107: Every island site is different and I am not sure that the simplification proposed by the authors is the most adequate one. Food for thought.

Line 108: Do you have any information about the pillars? What is the assumption here? If the bridge is located downstream of rapids, ice blocs may be small and easily pass under. If large is slabs come in contact with wide, rectangular pillars, yes, they might be stopped right there. This would be a serious engineering error that as no real link with a channel narrowing.

Line 109: What do you mean by “initially”? I believe that this factor should have been calibrated more accurately or considered differently (not a narrowing).

Line 112-113: I believe that this is a major mistake made by the authors: Presenting an assumption in the methodology, mentioning that it could be improved before the results are presented and not doing anything later despite this could have been better calibrated.

Line 116: Specify that this gives more importance to large tributaries. Again, this has almost nothing to do with channel narrowing.

Line 117: “In the end,”

Line 118: “the index has” not, “the index will have”

Line 120: Replace “by the most recent maximum width of the upstream sections” by “by the closer upstream maximum width”

Figure 3: Does not explain well how the tributary is considered

Figure 4: The flow direction should be the same than in Figure 3. A narrowing index (equation) should be presented for each presented section.

Line 131: “ranging from”

Line 131-132: This definition refers to SV, not the Sinuosity4... The authors should mention that SV is always larger than 1.

Line 133: I am not sure that “several” will satisfy the reader. Can you present a range? Can this take into account single bends and not only meanders?

Line 141: “did not integrate”

Line 139-142: Did the authors try to use the 5 m resolution? I am sure that some governmental agencies have data concerning river profiles and hydraulic models. This would probably not be precise enough to determine changes between 250 m sections, but it could very well identify slope breaks that are so important in jamming processes. As a reader, I am disappointed about this ending and this introduces a difficulty acceptable omission in the model. “Luckily” for the authors, a change in slope is normally characterized by a change in morphology and pattern and therefore slope breaks are somewhat indirectly covered by the model. Including gradient data would probably improve the model’s result.

Line 145: Not pertinent to mention the 950 jams since it covers all the rivers and since it increases every year.

Line 146: I would say “approximate” since most jams are longer than a single coordinate and because this geolocation does not refer to the toe where the jamming process is initiated. In some instances, the toe could be kilometers away from the observation point. Also, as mentioned in the general comments, some reported ice jams could be intense anchor ice or frazil jam events and these processes could take place at locations where ice jams are not likely to form. A validation with a corresponding rising Q should be performed.

Line 157: This is the Chaudière River section but this sentence refers to the three rivers. Table 1: Put NI and SI in the title as well

Lines 163-166: Note here that you use “reported” ice jams to calibrate a model. Taking into account previous comments may improve the calibration result and their reliability.

Line 170: Remove “would”?

Line 172: Remove “would”?

Table 3: This is a fair analysis tool but note that this version of the model cannot gain a high precision potential in part because it is limited by a 2D IJPI. Also, note that highly sinuous reaches often present a relatively constant width and therefore, the two parameters considered here are not independent. Therefore, a value of 1 may be difficult to obtain in reality.

Results:

Line 201: About false negative errors: Please consider the potential length of the jam and the difference between the initiation site (predisposition) and the observation site (anywhere along the jam).

Line 202: About false positive errors: Not really an error because this is not an ice jam temporal prevision model. Please consider at everywhere in the paper that important ice jams can happen where there is no observation point nor vulnerability.

Line 208: Please replace the terminology “risk” by an appropriate word. This is not a synonym of predisposition.

Line 209: I understand that the model can underestimate some factors, but this is your calibration river. This could have been better considered if you were not limited to two indicators and if observations had been made in the field.

Lines 210-211: Exactly, and this should be stated clearly in the previous sections. Not only a source of incoming ice, but also a source of incoming runoff and javes.

Lines 212-222: Please eventually consider: The contributing area for ice blocks, the gradient, an increasing width, the absence of observation points along the river, the

absence of vulnerability along the river, and finally, ice scars on trees. You should present the potential reasons for false-positive errors in the form of bullets.

Line 217: “is also overestimating [the predisposition] in some areas”.

Line 219: Same comment as line 209. Bridges are often located at natural narrows, but there design does not necessarily impede the transit of ice runs.

Line 224: How short? Please specify a range.

Table 4: You could have further investigated the 6 “no specific feature”. This could mean factors that have not been considered.

Line 245: Note about bridges: Their presence can mean less snow ice and more thermal ice. Their presence can also be associated with de-icing salt falling on the ice surface. Then, what would be the final relative ice resistance at the time of breakup?

Figure 7: Please adjust the errors (“source de renvoi introuvable”)

Table 5: Same comment as Table 5

Lines 259-263: The model may be missing something because there is no info about the gradient. A widening is also a site for ice jams, especially downstream of rapids or riffles. Note also that the analysis assumes that the toe of the jam was correctly located, which may not be true.

Line 260: Please had a reference that supports that sand bars are associated with ice jams. I do not know any.

Line 265-266: Yes. But then, this could be associated with other morphological or areal patterns.

Line 267: Please reconsider the use of “dynamic” parameter everywhere in the paper.

Line 274: The L’Assomption River is only sinuous in its lower portion.

Lines 278-280: A lot of sections may be associated with ice jamming, but the con-

tributing area is just too small. The ice jam will most often form in the first (upstream) predisposition area located downstream of a long stretch of relatively fragile ice.

Figure 8: Potential interpretation (I do not know this site): This is enough narrowing, widening and changing direction to generate an ice jam. The low floodplain on the left bank and the possible secondary channel all support ice jamming. The marker may point a pool between two riffles where ice jams often form (against a small hanging dam. . .).

Figure 9: Potential interpretation: B: The ice run loses energy in the bend and loses further energy in the widening. C: Water evacuation channels and changes in direction, enough to initiate an ice jam.

Figure 10: Potential interpretation: This is an energy concentration area followed by a dissipation area. If there is no info about the jamming scenario (jamming of an impeded or unimpeded ice run), the reason for the ice jam event is hard to certify.

Figures 8 to 10: Note how all these reported jam are located where roads or houses are close to the river, ideal observation points that may not correspond to the ice jam toe.

Table 6: You could lower the False-Positives by considering the length of the potential contributing area.

Conclusions:

Line 299: “static” could be “morphologic”

Line 300 and Lines 302-303: Yes, but the slope could not be considered and this should be mentioned as a potential development, not as a limitation of the actual model. This should be part of a discussion section. Overall, some sentences could be written more positively and confidently.

Line 306-307: This sentence should be removed as it is already mentioned two lines

above.

Line 312: Define “very close” as opposed to “short distance”. . .

Line 314: Here, the bathymetry is considered as a dynamic parameter. Why? Why not just considering morphological (pattern, width, gradient, topography) and ice (type, potential thickness, possible processes) parameters?

Line 320: “interesting” should be “promising”.

Lines 321-322: Do you refer to freezeup jams or to hanging dams?

Line 325: “more different” should be “additional”

Line 325: Please had a reference (Carr et al., 2015?) for the US database

Lines 327-328: Consider this change: “In addition to forecasting potential ice jam flooding sites, an improved version of the model could bring information for. . .”

Line 330: Consider this change: “combining spatial predisposition and temporal forecasting”.

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