The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-71-AC1, 2018

The Cryosphere
Discussions

Interactive comment on "Modelled subglacial floods and tunnel valleys control the lifecycle of transitory ice streams" *by* Thomas Lelandais et al.

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Received and published: 12 July 2018

Answer to the referee's comments

We would like to thank Carrie Jennings and the anonymous reviewer for the careful reading of our manuscript and the helpful comments. Replies to referee's comments are addressed below (blue colored). Broadly speaking, both reviewer agree on the novelty of our experimental approach but ask for a more clarification on the limitations of the experiment.

Referee #1 : Carrie Jennings General comments

"I appreciate this modeling attempt. I am not aware of any other work on modeling subglacial hydrology since G. Catania and C. Paola, 2001, Braiding under glass. Geology,

29(3), 259-262. I believe it is relevant and should be cited. Models inform our intuition. They cannot prove anything but they can lead us to a better understanding of physical processes if we understand the limitations of the model setup. I would like to see the model and its limitations more fully described. What about model is not like real world? What are the shortcomings? How could these shortcomings affect model results and deviate from real-world processes?"

The referee is right to point out that analog modelling provides intuitions and ideas on a specific process but does not constitute a proof. Experiments produce morphologies and dynamics that, although imperfect, compare well with natural systems despite differences of spatial scale, time scale, material properties, and number of active processes. Thanks to the numerous comments of the referee, we added (i) some restrictions on the interpretation of the experiments and (ii) the limitations of the model to reproduce its natural counterpart. An entire paragraph (section 2.3) is now entirely dedicated to the limitations of the model.

Specific comments – Abstract

L20 – 21. Do they ever evolve to be efficient drainage systems? Tunnels seem very short-lived and episodic to me and ice-streaming redevelops again and again. Your experiment represents a very coarse-textured bed when scaled up, so this may be an effect related to grain size.

In our experiments, tunnel valleys stay active during the whole experiment. Meltwater routing, dynamics of tunnel formation and evolution of ice stream dynamics are intricately connected during experiments. Figure 5 in the revised version shows that tunnel valleys, once reaching a certain overall volume, reduce the silicon flow velocity until the modelled ice stream switches off. This suggests that the tunnel valley system evolves in an efficient drainage system able to drain all the subglacial water, thus reducing water pressure and enhancing basal friction. Grain size has necessarily an effect on the drainage capacity of experimental tunnel valleys. The use of a substratum with different properties would probably change tunnel valleys amount and development rates.

Specific comments – Introduction

L32-33. drainage pathway for sediment...reword ?

We suggest to reword the sentence as: “approximately 80% of the ice discharge is focused in a finite number of ice streams, which act as preferential drainage pathways for meltwater also (Bamber et al., 2000; Bennett, 2003)”

L34. Ancient ? Is palaeo a word by itself

We suggest to replace every use of palaeo by “ancient” or “former”.

L35. Is m. correct ?

We thank the reviewer for this comment. It is true that the classical way to write any ratio is to use a slash. We suggest modifying all the “m.s-1” by “m/s”.

L37. How about the evolution ?

We suggest to modify the end of the sentence in: “and the controls on their dynamics evolution remain debated”.

L54. Reference for this? I think it relates to Ice Stream C and B in WAIS

We suggest to add Vaughan et al., (2008) and Carter et al., (2013) as references for the subglacial water piracy processes.

L68. ours are much shorter but formed in segments–or at least are interrupted by ice-marginal fans.

We modify the sentence to inform the range of tunnel valleys dimensions: “These valleys are elongated and over-deepened hollows, ranging from a few kilometres to hundreds of kilometres long, from hundreds metres to several kilometres wide and from meters to hundreds of meters deep.”

L72. not by all–this feels a bit like you are setting up a straw-man type argument.

We agree with the reviewer that this sentence was perhaps misleading. We suggest to rewrite as follows: “Indeed, ice streams commonly operate because of high basal water pressure while the development of a tunnel valley system generally leads to enhances drainage efficiency and basal water pressure reduction (Engelhardt et al., 1990; Kyrke-Smith et al., 2014; Marczinek and Piotrowski, 2006).”.

L74-76. I would say that from the field evidence, there is a third process: 1) ice streaming; 2) drainage through tunnel valleys; 3) stagnation of the ice margin.

We suggest adding a sentence to explain that field studies have already suggested a link between outburst flood and a set of events involving ice streaming, tunnel valley formation and ice margin stagnation: “Several field studies have already suggested a connection between catastrophic glacial outburst floods at ice sheets margins and a suite of events involving ice streaming, tunnel valley development and stagnation of the ice margin. (Bell et al., 2007; Hooke and Jennings, 2006; Jørgensen and Piotrowski, 2003; Alley et al., 2006).”

L79. Bering glacier behavior during and after a surge comes close https://www.cambridge.org/core/services/aop-cambridge core/content/view/08A39D0DD9EBE9C32D232B7769B55728/S0022143000202311a.pdf/la

This reference does not connect ice streaming with subglacial erosional processes so we choose to not add this reference because it is not appropriate with the meaning of our sentence.

L86. How would tunnel valleys influence the location of ice streaming if they only happen after streaming is already occurring? This may need to be more precisely worded.

We agree with the reviewer that this sentence requires clarifications. We suggest writing as follows: “We propose that the location and initiation of ice streams might arise from subglacial meltwater pocket migration and drainage pathways and that the evolution of ice stream dynamics is latter controlled by subglacial drainage reorganization and tunnel valleys development."

L86. This work ?

We suggest to writing as follows: "This study reconciles into a single story . . .."

Specific comments – Experimental ice stream model

L94. partially overcome ?

We agree that this phrasing might not be the best one to explain that our model is useful to explore the connection between subglacial meltwater routing and ice dynamics without being a perfect representation of nature. We suggest to rewrite this section as follows:

"Considering all these processes and components simultaneously, together with processes of subglacial erosion, is thus a challenge for numerical computational modelling (Fowler and Johnson, 1995; Marshall, 2005; Bingham et al., 2010). Based on this statement, some attempts in analogue modelling have been made to improve our knowledge on subglacial erosional processes by meltwater (Catania and Paola, 2001) or gravity current instabilities produced by lubrication (Kowal and Worster, 2015). To combine ice flow dynamics and erosional aspects in a single model, we designed an alternative experimental approach that allows simultaneous modelling of ice flow, subglacial hydrology and sedimentary/geomorphic processes. With all the precautions of use inherent of analogue modelling, our experiments reproduce morphologies and dynamics that compare well with subglacial landforms and ice stream dynamics despite some differences of spatial and time scales and a number of active processes (e.g. Paola et al., 2009)."

L97. Paola et al., 2009–the way this is referenced now makes it seem like they simultaneously modeled these things.

We thank the reviewer for this clarification. See the modification made to answer the last comment.

L98. Use as an example? This is not the fundamental reference for the previous statement. Shreve, R.L. 1972. Movement of water in glaciers: Journal of Glaciology, 11(62), 205-214? or even earlier: Glen, J.W. 1952. The stability of ice-dammed lakes and other water-filled holes in glaciers. Journal of Glaciology, 2(15), 316-318.

We agree with the reviewer that it is not the fundamental reference and we added Shreve (1972) and Glen (1952) according to your proposition.

Line 99 : thus in part controlled by....This seems overly simplistic or at least backwards–active margins of ice sheets are an expression of mass balance, bed topography and ice surface slope

We agree with the reviewer that meltwater routing is function of many parameters. We suggest modifying this sentence so that we understand that ice slope is prevailing to control meltwater routing but that subglacial topography, and the mass balance also influence meltwater routes. We suggest to add a section dedicated to the limitations of the model (2.3 Scaling and limitations) and to rewrite this sentence as follows: "Subglacial meltwater routing is indeed controlled by the ice surface, slope, the bed topography and the glacier mass balance (Röthlisberger and Lang, 1987). The ice surface slope controls potentiometric surfaces, generally guiding subglacial water flow parallel to ice sheet surfaces (Glen, 1952; Shreve, 1972; Fountain and Walder, 1998)."

Lines 100-101 : I am not following. It appears I need to refer to the earlier paper. Can this be avoided by providing a bit more here?

We understand the enquiry of the reviewer to understand the scaling of the experiment without referring to the first paper presenting the experiment. We propose to add explanations on the scaling and to move this section after the description of the model (cf. new section 2.3. "Scaling and limitations"): "Considering that meltwater is here simulated by an injection of water, the rules of a classical scaling where the model is a miniaturisation of nature are not practical (Paola et al., 2009). Subglacial water drainage is generally controlled by fluctuations in locations of ice sheet margins. Similarly, in our experiments, the silicon putty margin controls the water pressure gradient. In this perspective, we base the scaling on the displacement of the natural ice and experimental silicon margins through time. We use a unit-free speed ratio between the silicon/ice margin velocity and the incision rate of experimental/natural tunnel valleys. The scaling is designed to ensure that the value of the ratio between margin velocity and incision rate of tunnel valleys in the experiment equals its value in natural. The projection of the minimal and maximal experimental speed ratios on the field of possible natural speed ratios highlights the field of validity of the experiments and defines the range of natural settings we can reproduce experimentally (full details in Lelandais et al., 2016). The main scaling limit regards the viscosity ratios between glacier ice, silicon putty and water. The size of the experimental ice stream, being partly controlled by the high silicon viscosity, may be underestimated compared to the size of modelled tunnel valleys."

L103-106. Models at SAFL U of M often use hollow glass beads to overcome issues of density when using small models.

Producing DEM of the ice-bed interface was one of the main goal of this study and glass beads properties would have probably been less suitable for photogrammetry and 3D reconstruction (reflection problems, transparency, lack of roughness etc…). However, we think that glass beads would probably lead to the same suite of events, with a similar process of tunnel valley formation as the density of glass beads and the sand we use are similar. However the morphologies of tunnel valleys would probably differ due to changes in substratum permeability and friction coefficient.

L110-113. Comment: Having trouble visualizing where water is injected based on this description. Is water focused in one area?

The water is injected through an injector placed at the centre of the model which corresponds to the center of the silicon layer. The radial boundary of the silicon layer provide a radial flow of water so water is not constrained to flow in only one direction. For the visualization the cross-sectional profile in the Figure 1 show how water is injected in the system.

L118. again, placement makes this feel like the first time someone suggested that rheological softening was function of strain rate, T, etc.

We suggest to modify in e.g. Bingham et al., 2010.

L119. Nor can the potentiometric surface of water within the ice.

We suggest adding another restriction to our model in this sentence. Water flow is not driven by the silicon surface slope in the experiment. We suggest to rewrite as: "This punctual injection does not simulate the mosaic of meltwater production regions existing beneath glaciers or the episodic input from supraglacial/englacial meltwater reservoirs. Experimental meltwater routing is predominantly controlled by the water discharge we inject in our system and therefore differs from parameters controlling hydrology in glacial systems. Subglacial meltwater routing is indeed controlled by the ice surface slope, the bed topography and the glacier mass balance (Röthlisberger and Lang, 1987). The ice surface slope controls potentiometric surfaces, generally guiding subglacial water flow parallel to ice sheet surfaces (Glen, 1952; Shreve, 1972; Fountain and Walder, 1998)."

L120.: Appropriateness of reference: as I recall, he speculated and modeled that it was (based on dilatancy of layer?), but others measured it in W. Ant much more recently? Reword the way the citation is used?

We agree with the reviewer that the till influence on ice stream should be mentioned. We suggest to rewrite as follows:

"This model, designed to decipher the interaction between subglacial hydrology and ice dynamics, hinders the influence of bed topography and geology (especially the influence of subglacial till) (Winsborrow et al., 2010). The deformation of the subglacial till and its complex rheological behavior is known to promote ice streaming (Alley et al., 1987), modify the subglacial hydrology and alter the size of tunnel valleys. The development of an analogue material scaled to reproduce subglacial till characteristics is extremely difficult so we did not try to include the equivalent of a till layer in the experiment."

L120. Till would also change the behavior of water beneath the ice and potentially after the tunnel development

L123. and narrower tunnels

A till layer is extremely difficult to reproduce so we did no try to include one in our model. Doing so, we probably enhance some processes in the development of the ice stream and in the development of tunnel valleys. Hence we suggest to add some restrictions as follows : "The deformation of the subglacial till and its complex rheological behavior is known to promote ice streaming (Alley et al., 1987), modify the subglacial hydrology and alter the size of tunnel valleys. The development of an analogue material scaled to reproduce subglacial till characteristics is extremely difficult so we did not try to include the equivalent of a till layer in the experiment. We thus assume that the velocity contrasts observed in the experiment are thus likely to be amplified in natural ice sheets, by the complex rheological behaviour of ice and till. This may lead to the development of narrower ice streams with higher relative velocities and sharper lateral shear margins in natural ice sheets than in the experiment (Raymond, 1987; Perol et al., 2015)."

L130. 3 levels? I don't understand and diagram doesn't help resolve.

The figure is not helpful to understand how we dispose the UV markers. Hence, we modified the figure 1 in the revised version of the manuscript to help the readers distinguishing the 3 levels of UV markers.

L130. this word helps and could be used in text. However, why this style of water injection is considered to be realistic escapes me. How could water be added to the center of an ice sheet? The central position of the injection is specified in an earlier comment.

A circular shape of the silicon layer was preferred to avoid any preliminary constraints on the water flow route and to avoid lateral boundary effects on silicon flow. This circular layer of silicon simulates only a portion of an ice sheet but not the whole ice sheet. The central injection of water is thus simulating an upstream source of water along an ice sheet portion that does not correspond to the centre of an ice sheet.

L139. Vertical? You are seeing the ice surface sink? I think that this needs to be better explained because I thought it was probably as a result of horizontal advection of "ice" and deformation of ice into a void that is formed as sediment is evacuated. But from the caption I see that it is also (primarily?) because of the water pocket forming and that the surface is elevated. The caption and figures help but the text is not clear and I have to work hard to figure out all the possibilities. Are you facing a word limit? If not, make it easier on your reader to follow experimental design and expectations.

As the injected water is pressurized, we can observe and monitor vertical displacements of the silicon surface due to water flow. We did not see the silicon sink properly but we could monitor a subsidence area when the water pocket was moving from one place to another. We propose to add the following sentence in the next section describing the UV device: "The monitoring of every UV marker positions (in both horizontal vertical plans) through time was used to produce velocity and vertical displacement maps. Vertical displacement maps are interpolated from the subtraction of the DEM at time t with the DEM generated from the photographs taken a few seconds before the injection."

L169-170. I'd like to see photos of the setup also. These may have photo backgrounds but I cannot tell with the color overlay. Seems very idealized. Look at Ginny Catania's description of her model of subglacial drainage. Catania and Paola, 2001.

We agree with the reviewer that, in order to convince the readers who may not be familiar with such models, we should propose a better explanation of our device. Thus, we have add a picture of the device in Supplementary data. We also added a new figure in the revised version of the manuscript (Fig. 2) with six raw photographs of the main experiment stages described in figure 3. L171-172. What is the scale of these tunnel valleys? Approximate volume of the fans? If the box is 70 cm across, how well resolved are they? I would like to see them rather than just trust the drawing. The addition of figure 2 in the revised version with raw photographs of every stages should solve this problem. We also specify the size of every tunnel valleys and fans for every stages in the revised version of the manuscript.

Specific comments – Experimental results

L181-185. How long was the experiment run? How long did it take this change to occur?

L 188-189. Same question about length of experimental run.

This experiment was repeated 12 times with identical input parameters. A six-stage ice stream lifecycle linking outburst flooding transitory ice streaming and tunnel valley. The experiment typically lasts 30 minutes (cf. Figure 3). The silicon flow pattern changes instantly when water injection starts (cf. Figure 3). We added the duration of the experiment and the information that the silicon flow pattern evolves spontaneously in response of water injection within the text as follows: "This experiment was repeated 12 times with identical input parameters (a 30 mm-thick silicon layer of 150 mm radius; constant water input of 1.5 dm3/h during 1800 s). After an initial identical state, a six-stage ice stream lifecycle linking outburst flooding, transitory ice streaming and tunnel valleys development has been observed for all these simulations (Fig. 3a-f, Fig. 6)."

L 197. There are earlier references that first describe and document this phe-

nomenon: https://www.igsoc.org/journal/35/120/igs_journal_vol35_issue120_pg201-208.pdf . or https://www.cambridge.org/core/services/aop-cambridge-core/content/view/B2AD36180AEF8E3E8403A7BF2D627319/S0022143000002689a.pdf/sh

We thank the reviewer for this reference and propose to add it at the end of the sentence.

L 201-202. Reword. This sentence seems to be missing a word. It doesn't make sense as written.

We agree with the reviewer that this sentence is not correctly written. We suggest to modify as follows: "The lack of channels incised in the substratum indicates that water flow occurs as a distributed drainage system without any basal erosion".

L 208. What does this mean, to migrate by distributed drainage? Can you simplify and say the water pockets migrate? Are you saying that the water pockets stay at the ice-bed interface and migrate?

We agree with the reviewer and simplify the sentence as follows: "The experiment suggests that the migration of water pockets at the ice-bed interface can contribute to the emergence of ice streams".

L 211. What is channel scale and how does it evolve? Is water emerging from the ice margin through a narrow channel that grows headward and bifurcates?

At this stage, water is drained as a sheet flow at the silicon margin. This drainage is associated with widespread erosion, wider than the subsequent tunnel valley, producing the first low-angle fan described in supplement Figure 3. Once the water pocket drained, the water flow channelizes and a narrow valley starts forming by regressive erosion. Tunnel valleys are constantly growing along the experiment. Their size (length, width, depth) have been added for every stage.

L 212. What is the width of the head of this fan? Is it a lot broader than the fan that develops in the next stage? Need scales. I do not know of anything like this at a terrestrial margin. Till deltas off the coast of Antarctica might be of a similar scale. I wouldn't over-emphasize this since you just have sand, not till in the subsurface. I don't think that real-world ice margins leak this easily, especially where frozen. So instead you get thrust moraines from water pressure drop and tunnels.

The width of the head of every fan have been added for every stage. The first fan originating from the outburst is similar to the fan developing afterward apart from the angle. Indeed the first fan is a low-angle fan and the subsequent fan developing with tunnel valley is a high-angle fan. Although both fans are similar in size, the first one originating from the outburst flood forms nearly instantly, however the second one forms progressively during tunnel valley development.

L 213-216. I would expect the ice stream to evolve immediately prior to the tunnel valley formation. Why decreasing basal water pressure lead to it? Are you sure ice stream isn't when bubble reaches margin, immediately prior to TV formation? How long is all this taking and can you really resolve it? Are you slowing down the cameras? Is it video? Stop motion photography?

Silicon flow is progressively accelerating during migration of the water pocket. Obviously, the silicon flow is high when the water pocket reaches the margin but we record a peak velocity when the water pocket drains. We use a system of 7 cameras with a 5 second delay, but to be sure of our results we ran some experiments with a 1 second delay to visualize and validate the process described in the text.

L 219. Comments: e.g. Kamb? Bering Glacier surge and outburst is well documented.

As pointed out by the second reviewer this reference is not the best fit to support our result so we decide to choose Anderson et al. (2005) who documented ice flow acceleration triggered by outburst flooding.

L 221. Magnusson et al., 2007

We suggest to add e.g. before the reference

L 226-227. What is their scale in the experiment? How do they scale with the ice streams?

We agree with the reviewer that the figure itself is not sufficient to estimate the dimensions of our tunnel valleys. Consequently, we added tunnel valleys sizes at every stage in the manuscript. Compared to nature, experimental tunnel valleys are disproportionate compared to the ice stream size. The model itself constrains the size of the ice stream. The disproportion could also be the consequence of the analogue material we use in the experimental setup. The silicon putty is way too viscous to be scaled to glacier ice (this is clearly mentioned in the text). Using a less viscous material, the ice stream would have probably been wider and more scaled to tunnel valley size. However, silicon was selected as it shares some essential characteristics with glacier ice and mainly because its perfect transparency allows DEMs of the silicon-bed interface to be reconstructed.

L 227-228. I would like to see a cleaner description of model observations because here it appears that interpretations and discussion are interwoven.

We have choose this organization which alternates description of the model and natural examples, directly after the description of every stage, in order 1- to validate each experimental observations by natural examples, 2- to avoids many repetitions in the manuscript to facilitates the reading. We wanted to base the main discussion on more general and global implications of our modelling results.

L 230 . What is the width at the head of the fan? Is it the width of the tunnel?

We added sizes of tunnel valleys and fans for every stage in the results part. As silicon flows, the silicon layer progressively pushes the fan so the width at the head of the fan is wider than the width of the tunnel valley.

L 249. and that eskers formed in tunnel valleys represent a waning flow stage.

It is true that eskers within tunnel valleys symbolize a decrease in water flow velocity.

Hence, they might represent the ultimate stage of tunnel valley development. However, we cannot simulate this final deposition stage in our experiment.

L 259-260. We also see stagnation of the ice lobe margin. Large glaciotectonic thrust masses at ice margins are located near tunnel valley fans and seem to represent the fast flow stage immediately prior to drainage.

We thank the reviewer for this useful comment. We suggest to use these field evidence in the revised version to support our experimental results: "Large glaciotectonic thrust masses at the ice margin near tunnel valleys fans are generally assumed to be a field evidence a fast ice flow stage prior to drainage through tunnel valleys (Hooke and Jennings, 2006)."

L 265. I do not see field evidence of two, very different scales of floods and two styles of fan formation. I suspect that the first one you observe is more a result of the unusual way you are building water pressure beneath your ice sheet (at a single point).

The first outburst flood resulting from the first water pocket drainage is more obvious and its consequence on silicon flow is more visible (cf. Figure 3). The second outburst flood consists in the drainage of a new water pocket probably originating from the inefficiency of the first tunnel valleys to drain all water. This second water pocket is probably less significant than the first one so the consequence on the silicon is less visible. In nature if tunnel valleys originate from an outburst flood, we might not find any evidence from another catastrophic drainage as they are probably occurring in the same water path.

Specific comments – Proposed lifecycle of transitory ice streams

L 274. Are these the earliest references to this phenomenon? I think not. Use as examples or cite the foundational work.

We agree with the reviewer and add some earlier references: Bentley, 1987; Blankenship et al., 1993; Anandakrishnan et al., 1998

L 284. this is a review paper. As reviewed in Kehew and Piotrowski,

We agree with the reviewer and we suggest modifying the sentence as follows: "As reviewed in Kehew et al. (2012) and suggested in Ravier et al. (2015) this relation was suspected from the occurrence of tunnel valleys on ancient ice streams beds."

L 285-286.You may be setting up a "straw man" because I don't know how widely believed/modeled this is.

Ice streams may arise from various processes (basal decoupling, deformation of the substratum. . .). However most of the models, emphasizes the development of high water pressure in the bed or at the ice-bed interface. For tunnel valleys we agree that we might not be so categorical about water pressure. We suggest to modify this sentence as follows: "However, it raised a contradiction: subglacial meltwater pressures are generally supposed to be high below ice streams (Bennett, 2003) while tunnel valleys are generally assumed to operate at lower water pressures (Marczinek and Piotrowski, 2006)."

L 288-290. I think this has been speculated before from field evidence. You may be the first to physically model it, however.

We agree with the reviewer that we might rewrite this sentence to emphasize that although several observations have already connected ice stream with outburst flood and outburst flood with tunnel valley formation we are the first to model these interactions and to propose a single model connecting outburst flooding, ice stream and tunnel valley development. "Although speculated from field evidences, our results demonstrate that ice streaming, tunnel valley formation, release of marginal outburst floods and subglacial water drainage reorganization may be interdependent parts of a single ice stream lifecycle that involves temporal changes in subglacial meltwater pressures (Fig. 6)."

L 291-293. no comma after Approximately. But more importantly, this pocket migration is highly dependent on the focused way you introduced water into the sublacial environment. I would not make too much of it since it is highly unrealistic.

We agree with the reviewer that the way we introduce water in the model is unrealistic. However, water injection at a given discharge triggers water flow at the silicon/substratum interface. The consequences on silicon flow should be comparable to the influence of subglacial meltwater on ice flow dynamics and therefore not be so unrealistic.

L 297-300. Why would they switch on precisely when water drains? How do ice streams migrate headward in this scenario? I think the ice stream migration timescale is very different than the water drainage timescale. You refer to timescales in a vague way in the beginning of the paper. Time to return to those ideas?

In our experiment, we define the ice stream birth phase when a corridor of high silicon flow appears. Before, the silicon flow velocity only increases above the migrating water pocket. Using a more viscous material than glacier ice has a consequence on timescale involved in ice stream migration. . The silicon putty accelerates the process of ice streaming and we cannot observe the headward migration of the ice stream. The migration of the experimental ice stream in response to re-routing of the water occurs almost instantaneously. Once more, this very quick migration may be a consequence of the high viscosity of the silicon we use during modelling.

L 309-310. in contrast, we see ice stream locations and tunnel systems becoming fixed. Tunnels are reoccupied again and again as an ice sheet retreats.I'm not saying that ice streams dont' migrate, just at a different time scale (or again, this might be an artifact of the way you are introducing water.)

Our study only proposes an alternative solution for ice stream migration and lateral development of tunnel valley, based on meltwater re-routing. If we consider that tunnel valley development occurs progressively, the drainage can be inefficient, possibly leading to meltwater storage. The drainage of stored meltwater could trigger a second

phase of ice flow acceleration. If drainage occurs laterally to the main drainage path represented by the pre-existing tunnel valley system, drainage may indeed trigger a lateral migration of the ice stream path and the formation of a new tunnel valley (as observed in our experiment; Fig. 3). Of course, this drainage could also occur within the pre-existing tunnel valley system, thus not modifying the position of the ice stream path.

L 324-327. This seems to be taking the results of the experiment a bit far and a slightly misrepresenting the conclusions (or at least the dire nature of them) of these papers.

We agree with the reviewer that we might have over-interpreted the references we used. We propose to rewrite this sentence as follows: "This further suggest that tunnel valley development could secure ice sheet stability as hinted by Marczinek and Piotrowski. (2006) by preventing catastrophic ice stream collapses".

L 328. a slash is not proper punctuation (a pet peeve).

We suggest to rewrite as :

"In a global change context, phenomena of ice stream stabilisation would requires that pre-existing and newly forming tunnel valleys systems expand sufficiently fast to accommodate increased meltwater production."

L 329-332 : Comment : You may have needed to make the conclusions seem relevant to a broad audience but to readers of The Cryosphere, this seems a bit extreme and sensational.

We agree with the reviewer that the current conclusion is a bit extreme and focused on current global warming issues. We deleted the "sensational" part of the conclusion in the revised version of the manuscript.

L 335-336. This schematic is fine as long as we understand it is an interpretation. I'd like the original model results to look more "real" like photos of a model, and less like this.

This schematic is actually an interpretation of the six-stages ice stream lifecycle we describe in the manuscript, based on true experimental data. This interpretative sketch is actually drawn using experimental DEM of the bed and flow velocity maps of the silicon. We just additionally drew an ice column and fans to the experimental data to obtain a practical model, easier to compare with nature. We decided to mix interpretational and experimental data in this final diagram of the manuscript to constitute a synthetic model, easy to understand and usable for any glacial geologists or glaciologists. We already added in the revised version a figure (Fig. 2) displaying "real" photo of the experiment that will help the reader to better apprehend the model.

Referee #2 : Anonymous reviewer

General comments

This paper describes an analog experimental model for ice flow over sediments and water, and uses the results of the experiments to describe a transitory lifecycle of an ice stream. The paper is short; it identifies some of the known features of modern and paleo ice streams, discusses the combination of conditions that are thought to play a role in the dynamics of ice streams, describes the experimental setup, the results of an experiment, and the inferred 'lifecycle' behaviour of an ice stream. The experimental approach is quite novel (though not without precedent; notably the paper of Catania & Paola (2001) is absent from the references and deserves comment) and I think it is welcome. You might also reference the laboratory work of Kowal & Worster (2015), which has some similar results. The setup appears to be quite sophisticated, allowing detailed mapping of elevation changes and velocities. There therefore appears to be considerable scope with this approach. However, the current manuscript is somewhat lacking in detail and I think there needs to be more scientific discussion about the extent to which the experiment does and does not represent the real world. There also is relatively little data presented on the detailed measurements that have evidently been taken. At present, it reads like a re-hash of a submission to Nature, and I think it needs a bit of expansion to fill in some details for the more discerning reader. The

paper is nevertheless well written and interesting, and I think with improvements it can be a valuable contribution to the literature.

As pointed out by both reviewers, we agree that the current version is lacking some data especially on tunnel valleys size and a section specifically dedicated to discuss the limitations of this model. This model do not and cannot simulate the whole complexity of a natural system, we therefore added some clarifications throughout the manuscript (cf. replies to referee 1's comments) on how this model works but also on how some of the experimental parameters can potentially alter the model validity.

Specific comments:

The experimental approach is advocated partly on the basis that numerical modelling and field observations are not able to include all the coupled components of the ice stream, sediment, water system. However, there is almost no discussion given to the drawbacks of an experimental approach; in particular, the issues of things that are missing (the analog 'ice' does not change phase for example), and the extent to which the processes can be scaled down. There should be more attention given to this. For example, what is the Reynolds number of the subglacial water flow? Are the dimensions of the 'tunnel valleys' that form comparable to real tunnel valleys (relative to ice thickness, say), and does the grain size of the sand not have some effect.

We agree with the reviewer that the lack of discussion on the drawbacks of an experimental approach is an issue. We suggest to add a section within the methods to mention the process that are not simulated in the model (cf. section, 2.3. "Scaling and limitations").

How was the flow-rate of water to be injected chosen, and are the results sensitive to this? Is it realistic? (In terms of water flux as compared to ice flux, say). How is it decided when to start injecting the water? Does this make a difference?

The flow rate of water is calculated so that water pressure is exceeding the combined weight of the silicon and sand layers. This is calculated beforehand to initiate water flow at the silicon-bed interface. The flow rate of water is not realistic against the silicon flux because it would require a perfect scaling which is impossible from a material point of view. We added these details in the methods part of the revised version of the manuscript:

“Water discharge is calculated beforehand so that water pressure exceeds the combined weight of the sand and silicon layers. The injection of water starts when the silicon layer reaches the dimensions we fixed for every experiment (15 cm radius and 3 cm thickness) and a perfect transparency. Once injected, water flow is divided into a Darcy flow within the substratum and a flow at the silicon/substratum interface. The water flowing at the silicon/substratum interface originates from a pipe forming at the injector once water pressure exceeds the cumulative pressure of the silicon and sand layers. The ratio between the Darcy flow and the flow at the silicon/substratum interface is inferred from computations of the water discharge flowing through the pipe based on the substratum properties and the input discharge. We estimate that 75% of the input discharge is transferred as Darcy flow in the substratum and 25% of the input discharge along the silicon/substratum interface.”

How much of the water flow is through the permeable sediments and how much in a film at the sediment/silicon interface? How thick is the water layer? Are the sediments in suspension or carried as bedload?

We have estimated that 75% of the water is flowing through the permeable sediments and 25% at the interface. This ratio have been inferred from computations of the water flowing through the pipe forming over the injector. We add these details in the methods part (See the modifications to answer the last comment). Water layer thickness (over a 1 mm inside the water pocket) can be inferred from the vertical uplift maps in Figure 3.

Only one particular experiment is described in any detail. It is not clear how repeatable this is except for the comment on l190 that the observed lifecycle is the same for 12

identical runs; but it is hard to imagine that the development of the three ‘tunnel valleys’ is exactly the same each time. Is there really always two stages of streaming? Do they always appear on the same sides of the experiment? How different are the plots in figure 3 between different experiments (in terms of peak velocity for example)? There should be more discussion of the other experiments.

We agree that it might be confusing to state that every experiment leads to the same outcome. It is true that every experiment lead to the development, migration and drainage of a water pocket that subsequently trigger ice streaming. For every experiment, the drainage phase is followed by the development of tunnel valleys that causes ice stream deceleration. However, there is variability in the amount and size of tunnel valleys we form between the different experiments. We also notice that there is not always a lateral migration of the ice stream when the drainage efficiency of the tunnel valley system is sufficiently high to prevent storage/drainage of a second water pocket. A new paragraph (section 3.2 “Experimental reproducibility and variability”) discussing the range of experimental results has been added to the revised version of the manuscript.

Figure 2. It is not completely clear what is shown in the first column, and the color scale chosen is not particularly suited to showing elevation changes (e.g. it is quite unclear where zero is). Given that there are negative values, this is presumably an elevation change from some reference? What is taken as the reference, given that the silicon is anyway spreading (and presumably lowering?) before injection starts?

The surface elevation maps are made from a reference picture taken just before the injection (few seconds before the injection). We suggest to add the 0 on the color scale for the vertical displacement maps and to add information on how silicon flow velocities and elevation map are interpolated within the methods section:

“The monitoring of every UV marker positions (in both horizontal vertical plans) through time was used to produce velocity and vertical displacement maps. Vertical displacement maps are interpolated from the subtraction of the DEM at time t with the DEM generated from the photographs taken a few seconds before the injection. Velocity maps are interpolated from the subtraction of the position of every marker at time t with the position of the same markers at the previous stage."

The surge of the Variegated glacier referenced on line 219 was, as I understand it, accompanied by a decrease in the outlet discharge of subglacial water rather than an increase. A subsequent increase in discharge, with the development of a more efficient drainage system, accompanied the termination of the surge. So I am not sure this is quite the same behaviour as seen in your experiments.

We agree with the reviewer that the study of Kamb (1985) on the Variegated glacier is not the best fit to compare with our results as the surge termination is associated with an outburst flood. We suggest to switch this reference with the study of Anderson et al., 2005 which observe and measure an ice flow acceleration following the outburst flood of the Hidden Creek Lake, Alaska.

The slow-down of the ice stream is attributed to a lowering of subglacial water pressure together with the growth of tunnel valleys, but presumably in the experiments there is also an influence of the changing silicon geometry which is driving the silicon flow. The surface is lowered over the central part of the dome and the driving stress is therefore reduced. What is the evidence that the ageing of the ice stream is not simply due to this effect? (which is also present in the real ice-stream problem too).

Broadly speaking it is true that the change of silicon geometry will inevitably slow down the silicon flow as the amount of silicon which is available to flow progressively decreases. However, we described an experiment where all processes of ice streaming and tunnel valley formation occur in a short period of time (30 min). Hence, the progressive decay of the ice stream related to ice thinning is negligible here. However, if we consider a context where the silicon layer thins significantly, water pressure would decrease similarly to nature. Indeed, the size of the pipe forming when we inject wa-

ter within the substratum is dependent on the thickness of the sand and silicon layers. Hence, if the silicon layer thickness decreases the pipe circumference would significantly increase, which conduct to an increase of the water discharge flowing at the substratum/interface and a decrease of water pressure. Consequently, the stagnation of the ice stream in our 12 experiments is always achieved despite an increase of water flow at the silicon/substratum interface.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-71, 2018.