**Review of Vatne and Irvine-Fynn 2015: Morphological dynamics of an englacial channel**

## 1. Summary of manuscript (MS)

The MS presents a field mapping study of an englacial channel over a decade, to my knowledge, the longest such study. In the Introduction it gives an overview of the literature and theories of both stream and ice-stream morphology and hydraulics. This is valuable as the audience of this paper may have backgrounds ranging from river morphology to glaciology. In the Discussion it advances sound qualitative explanations for the found morphological evolution.

The material is of high scientific value. However, the presentation needs polishing as outlined in the specific comments below. In particular, even though their results and discussion are sound, they often are presented in a somewhat confusing manner. After these shortcomings are cleared up, I recommend to publish it in HESS.

*Response*

*We thank the Reviewer for such a positive response to our manuscript, and we are pleased our effort to contribute to the currently lacking literature relating to englacial channel dynamics has been recognized. Moreover, we are very pleased to learn the Reviewer finds no flaw with our core thesis regarding the evolution of the channel surveyed. Crucially, we do recognize that the Reviewer's viewpoint regarding the clarity and presentation of material strongly echoes the views of Reviewers 1 and 2. To this end, we refer to the responses to Reviewers 1 and 2, in which we outline a clear plan to revise the discussion section of the manuscript, focusing on clarity and brevity rather than substantial changes to inferences, interpretations and concepts. Specifically, we will reorder material to focus on the key questions regarding i)* Do englacial conduits exhibit time-invariant morphological characteristics? *and ii)* Which factors control knickpoint face gradient and upstream recession rate*, and realign any subjective or speculative material into a brief "conceptual model" section, which aims to separate concrete results and findings from our hypothetical interpretation of channel dynamics and processes.*

*We thank the Reviewer for highlighting a number of typographical and terminological elements to correct or revise. Rather than address every point in full below, we have assessed all these suggestions, and are willing to accept and address these technical points by looking to correct phrasing as suggested, or to improve clarity. Where a longer comment is required, we have provided our more detailed response.*

## 2. Comments

P 16 L 16 In the Abstract better use “step" instead of “knickpoint" as that is less jargon

*Response:*
*We agree for the Abstract that this is sensible; however, we will also look to clarify our use of terms in the introductory sections. Specifically, as this point regarding “jargon” and terminology is picked up by the Reviewer later in this list of areas for consideration and improvement. Specifically, we will look to check, edit and revise our usage of terms such as “knickpoint”, “knickzone” and “step-pool”. It was not our intention to cause confusion by a somewhat interchangeable usage of terms, but we thank the Reviewer for alerting us to this issue and the need for us to ensure an improved consistency in the text.*

P19 L21 I like Section 2 which gives an overview over the subject. However, it could be streamlined.

*Response:*
*We had endeavored to provide a strong and supported context for the study presented here, and we are pleased to see such positive note by the Reviewer. Nonetheless, we can look to elements here which could be revised or condensed in an effort to 'streamline' the manuscript, in this section and (as noted) in the discussion section(s).*

P19 L23 The first sentence of this paragraph needs rewording, the second maybe also. I think this should read “knickzones” and not “knickpoints”, as the points will not provide much flow resistance.

*Response*
*We thank the reviewer for noting a typographical error here, and will revise accordingly.*

P22 L26 I think even with clastics, the meandering of supraglacial streams would be induced by helical flow. Thus this should be reworded.

*Response*
*We propose a simple rewording here to clarify this valid point.*

P23 L8,11 I am very used to q being the water discharge in this context. Maybe use a different variable name? Or just combine the two into one equation and state that the numerator is the heat flux.

*Response*
*We had endeavored to present the equations for clarity, but to reduce manuscript length, the*

*suggestion to combine the equations is sensible, and this can be readily addressed.*

P24 L1-4 This is an awfully convoluted way to say that either of surface melt rate and stream incision rate can be larger.

*Response*
*We will rewrite this sentence*

P24 L26 Somewhere in this section the difference between pressurised and open channel flow should be made clear.

*Response*
*This comment is in line with Reviewer 1's observations, and we will clarify this by a clear introduction to the filed site and channel description*.

P24 L28 This sentence needs a better transition from the previous statements. Long sentence. That pool depth is inversely proportional to step height, probably needs a citation, as it seems quite counter intuitive to me.

*Response*
*Deleted "inversely" as this was a typographical error, and one that was picked up on by Reviewers 1 and 2.*

P27 L7 This paragraph is confusing, it mixes up two things: cuspate forms and channel height. I didn't understand what was meant with cuspate forms until looking at Fig. 5. Maybe move this to the observation section?

*Response*
*The observation mirrors that of other Reviewers, and in consideration of these constructive perspectives on the manuscript, as described for Reviewer 1, we will be revising our use of the cuspate forms and making the discussion presented less reliant on this material. Such a revision will require this section of text to be revised and as suggested, elements will be relocated to other appropriate sections within the manuscript. Essentially, we will refocus our emphasis here on the use of potential use of cuspate forms to derive useful measures of channel dynamics, rather than any quantification here based on a limited observation set*.

P28 L29 I struggle with knickpoints being described as having an "extent", as a point has no extent. Also Fig.1 could suggest that the knickpoint is indeed a point. But I could well be wrong. Maybe use "step riser" in conn

Response

We will rewrite this section according to the reviewers suggestion.

P29 L1-5 Fig 4 talks about the “Type 1” and “Type 2” but they only get defined in the next section. This is a bit confusing.

*Response*

*We had introduced the “Types” in Figure 1; however we can see how confusion is introduced, and we will rectify the text to ensure the categories are clearly explained at the earliest appropriate point in the text*.

P29 L14 Entrance point B is never defined.

*Response*

*Reference to B deleted as this was a field-based ID, but as the Reviewer notes, is redundant here.*

P29 L27- Again struggling with the usage of “knickpoints” here. According to Fig.1 the whole assembly (pool + step-riser + knickpoint) is probably called a “step pool sequence”.

*Response*

*We will use step instead of knickpoint*

Entrance point B is never defined.

*Response*

*As above, reference to B deleted*.

P31 L22 It is confusing to end the last section with a statement that the discussion focuses on the longitudinal profile and not the plan form profile and then start the Discussion with the plan form one!

*Response*

*The rationale for this was based on the material and length of discussion and signposting to the conceptual model we preset in the later portion of the Discussion. However, we recognise there may be a perceived lack of parity between the Results and Discussion text. Moreover, given the overarching suggestions from Reviewers 1 & 2 to reconsider the structure of the Discussion section, we are happy to look to revising the text and to ensure material adheres to a consistent order throughout the manuscript sections. We thank the Reviewer for indicating that this section may be*

*best placed elsewhere.*

P31 L25 Why is this study of Myreng (2015) not just integrated into this paper and maybe Myreng made a co-author. The MS presents data from it in Fig.3 and discusses it several times. Considering this is a master thesis, presumably supervised by one of the authors, why not include it?

*Response*

*Myreng utilised the data sets presented here, and conducted a further speleological survey of the englacial cave that is the focus of this paper in 2014. However, the inclusion of these data from that survey do not significantly change the assertions we seek to make in the paper. Nor are they essential to the core message presented. We can and will integrate this material more clearly in the methods section, indicating that a further data set was retrieved in the same manner, and this ensures data can be included directly without further reference to the MSc thesis. We have discussed this with Myreng, who has indicated as long as her thesis is cited and she is given appropriate credit, this will suffice. Given other students were involved in earlier surveys, it seems only fair to treat all equally.*

P32 L6 Now suddenly the discussion jumps to the vertical profile still within of what I thought was the plan-form discussion.

*Response*

*This resembles the earlier comment regarding overarching structure in the Discussion section. We will remedy this through a revision of the order and clarity of material presented here. This is also aligned with Reviewers 1 and 2 requests for a more 'streamlined' and focused debate of the data presented, and separation of any "conceptual model" drawn from our findings.*

P32 L14 Is there any evidence that the crystallography of the ice impacts drainage morphology?

*Response*

*There were no systematic study of crystallography during the surveys, but the survey team was continuously looking for structures in the ice that was impacting on drainage morphology, but none were detected.*

P34 L14-16 This is not about LG reaches and should be moved to a more appropriate place.

*Response*

*Revision of the Discussion section will ensure material is relocated more logically. We thank the Reviewer for highlighting this here*.

P34 L27 It is not clear what “wave-trains” are, either define them or reformulate.

*Response*

*The term wave train is picked up from Loget and Driessche (2009) who used this term for a series of knickpoint incising upstream in bedrock. We will rephrase this to ensure our meaning of “wave train” is evident and fully explained in the paper, with specific introduction to the ideas and concepts in the section in which we present an overview of the terrestrial knowledge-base regarding step-pool streams. Moreover, this reference and concept may also aid in alleviating concerns Reviewer 2 noted for the ideas of knickzone scale changes vs. individual knickpoint changes, and so through use of this reference we hope to resolve these uncertainties. We thank the Reviewer for highlighting how expansion of this term may aid clarity here.*

35 L13-26 Mention again that meanders are a prevalent feature in MG.

*Response*

*We will add a sentence emphasizing the importance of meanders in MG reaches*

P37 L1-19 Would it make sense to discuss MG reaches after LG and before KZ? Define what is meant with “locally”, probably a few channel diameters. As it stands now it seems to conflict with the next sentence which mentions “rate of heat loss” as something independent of local dissipation. If it is very local that rate of heat loss should be equal to the dissipation rate. But this is not the case here.

*Response*

*We have already indicated that, in response to the request from all three reviewers, we will represent the ideas in the Discussion section with clearer adherence to our core research questions and the conceptual model we look to provide. We note that the Reviewer repeatedly comments that our views and interpretations are sound, suggesting there are no fundamental flaws with our logic, therefore we trust that a careful revision of the language, structure and layout in the Discussion will allow for a more accessible manuscript.*

P39 L4 This sentence suggest that this makes the step riser shallower again. How shallow? What are the limiting factors?

*Response*

*This is a good point. Our view is that this may well be the case, and we suspect limiting factors may include parameters such as discharge, ice structure and crystallography as well as the reach lengths and geometries both up and down-stream. However, in the absence of the necessary quantitative*

*data pertaining to this directly, we are unable to provide an answer to "how shallow?" or address the limiting factors in a meaningful way. More crucially, with Reviewers 1 and 2 suggesting our work tended to "speculative" in places, addressing this point would be adding further conjecture to the ideas presented here. Consequently, we do not feel it is appropriate to expand on this point, rather to revise the text to indicate that this is an area for further research. We will include this notion more clearly in the revised text here.*

P39 L25 A thought about Type 2 knickpoints: as they are vertical, recession rates should be quite small as contact of the jet is minimal. Maybe these evolve mostly by downward erosion of the pool/pool-overflow? The evolution could be like so: as a knickpoint migrates upstream it hits on the next upstream knickpoint. Its recession will stop, its face steepen to vertical. Finally, it can only erode downward until its upper pool reaches the level of its lower pool, i.e. it disappears.

*Response*

*We are slightly unclear on the Reviewer's perspective here, and somewhat disagree with this point. Referring to our Figure 1, the Type 2 step-pool sequence illustrates a vertical riser, where the water jet remains in contact with the ice face due to the upstream geometry leading to the rapid headward erosion of the vertical face. We do feel that clarifying this by a slight adjustment to Figure 1, to illustrate a low gradient reach immediately upstream of the step-riser would be helpful. Crucially, the pool represents a zone of rapid energy dissipation, and it seems less feasible for the pool to 'erode' vertically through the continued impact of descending water entering the pool. However, the notion of associated step and pool changes is interesting. We are willing to examine this carefully, and include material that explains the potential mechanisms envisaged, however, again we are cautious to not include more speculative material here in view of the perspectives of Reviewers 1 and 2 particularly.*

P40 l10 Not clear. Is "bed surface surface water flow" correct?

*Respons*

*Bed surface surface deleted*

P40 Eq.3 The treatment of this equation is wrong. The closure rate for a certain radius cannot just be used for any radius! The equation gives a closure rate dependent on the radius … These type of exponential decay laws are generally characterised by their half-life t1/2, i.e. by the time it takes for the conduit radius to shrink to half its former size. … I get the following half-lifes for h1 = 80m and A values from the below referenced table: T = 0C → t1/2 = 0.7 a, T = −5C → t1/2 = 1.8 a and T = −10C → t1/2 = 5 a. Therefore, for temperate ice a channel will close up over a winter to about 1/2 of its

radius but at -5C it will be about still around 75%. The conclusions in this section have to be updated.

*Response*

*We thank the reviewer for this excellent observation, and acknowledge fault resulted from considering a unit-radius scenario, and using an iterative approximation, which propagated into our calculations and figures here. This error was entirely ours, and demands correction; we thank the Reviewer for alerting us to this. Consequently, we will revise our calculations here to ensure the results and values we achieve are entirely correct. Crucially, this does not fundamentally change our primary argument: closure rates do suggesting significant contraction of the observed englacial conduit between hydrologically active summers and yet this does not appear to be the case in our experience of the channel. Specifically, when considering channel dimensions reported for autumn and spring surveys, where generally channel widths are broadly similar, as presented in our data, and vertical incision would seem to be more critical to channel evolution. We will, nonetheless, adjust our inferences and conclusions drawn here accordingly, and emphasise these points.*

Fig. 9 Also, Figure 9 needs to present the data differently: maybe using the second of above equations to make a plot of h versus the % of closure over the typical length of a winter for the observed ice temperatures.

*Response*

*We appreciate the Reviewer's suggestion here regarding the Figure. Our intention was to highlight the relative conduit closure rates for the depth range of the channel surveyed and discussed here. In reconsidering this, coupled with the recognition of our calculation error, we are of the opinion that perhaps a revised Figure 9 would contain two panels, with (1) a contour plot allowing expression of variations in radius, ice parameters, and time, and (2) a specific plot for the "percentage closure" during the hydrologically inactive period for the conduit in question, specifically relating to ice thickness. We believe these revisions to Figure 9 will be appropriate and more informative for the reader. We agree with the Reviewer that a "% closure" value is more meaningful, given the exponential closure law would suggest infinite time is needed for complete closure.*

P40 Eq. 4 I would just use tabulated values of A, for instance from Cuffey and Paterson (2010) Table 3.4, page 75. This avoids having to deal with another equation and a lot of constants which does not add anything.

*Response*

*While we appreciate this suggestion, the tabulated values for A listed in the Table are limited to 0, -2 and -5 at temperatures approaching zero. The temperature data available for Austre Broggerbreen does indeed lie in this range, but our intent was to adhere to the published temperature gradients for*

*Austre Broggerbreen to present a more robust illustration of the anticipated channel closure rates for the depths and temperatures reflective of the englacial conditions. We will revise our calculations and represent the data, but would prefer to adhere to good scientific practice and use the values appropriate for the 'known' conditions for the site in question.*

P40 L21-23 "time invariant equilibrium morphological features" contradicts the statement on p.73, 113 for MG reaches.

*Response*

*We will revise our wording here to remove contradiction*.

Fig 3. Correct two spelling mistakes of x-axis label, move to bottom. Make y-axis from -90m to 0m. State that 2x vertically exaggerated. Then, a confusing feature, which should be explained in the text, is that the depth of the 2008 channel is deeper than the 2014 beyond 150m. Presumably the 2014 channel got more sinuous?

*Response*

*We acknowledge the confusing feature that the depth of the 2008 channel is deeper than the 2014 channel beyond 150m. As pointed out by the reviewer this is likely to be an artefact from the fact that the x-axis shows length along thalweg an not horizontal distance from the entrance. It can also be explained by the uncertainty of the location of the 2014 entrance due to the time between the surveys and as this survey was not conducted by the first author. This will be mentioned in the text*.

Fig 9. The meaning of the legend is not explained. Maybe no need to cite Hagen et al. in the caption, this makes it seem like all of this is from them. (See also above for more corrections)

*Response*

*In light of the suggested revision to Figure 9, the caption would be redrafted, and information revised in the appropriate section of text. We will ensure any legend is made explicit clear, and where data drawn from Hagen et al's paper is used, appropriate credit is given.*

*References*

Loget, N and Van den Driessche, J. 2009. Wave train model for knickpoint migration. *Geomorphology*, 106 doi:10.1016/j.geomorph.2008.10.017. 376-382