Interactive comment on "Morphological dynamics of an englacial channel" *by* G. Vatne and T. D. L. Irvine-Fynn

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Review Comment- Morphological dynamics of an englacial channel

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GENERAL COMMENTS

This paper reflects an impressive body of survey work and literature review for which the authors should be commended. It generally contains very salient discussion of key englacial channel observations and highlights their implications for the broader field of glacial hydrology.

Response
We are pleased that the Reviewer recognizes the potential impact of this work, and specifically the linkage between terrestrial channels and englacial forms. With an absence of published literature focused on the morphological dynamics of englacial channels, and our findings that suggested both similarity and contrasts in processes that control the evolution of such channels and their terrestrial step-pool counterparts, we felt this was an important area to present to the wider hydrological readership of HESS. Moreover, it is pleasing to see the Reviewer's opinion of the scientific value of this paper is shared by the other two Reviewers 1 and 3.

However, these findings are buried within a verbose and sometimes poorly organized text that often presents unnecessary detail. The discussion section in particular tends towards being speculative. More quantitative treatment of the processes described would strengthen the arguments presented, particularly in section
5.2. The paper would benefit from a re-focusing of ideas and removal of unnecessary detail in order to bolster and clarify conclusions.

Response
We are disappointed to see the Reviewer felt as strongly as they did in terms of our writing style given the phrasing of their commentary. However, we acknowledge that both Reviewers 1 and 3 indicated elements of the text should and could be revised to improve its clarity and focus. As detailed in our response to Reviewer 1, we propose to address this by a substantial reordering and representation of the material contained within the discussion section. By maintaining focus upon the core questions (i) do englacial conduits exhibit time-invariant morphological characteristics? and (ii) which factors control knickpoint face gradient and upstream recession rate, we will eliminate uncertainties related to the current mix of evidence and inference that all Reviewers seem to identify makes the discussion section a challenging portion of the manuscript. We propose such a revision would facilitate the introduction of a brief "conceptual model" section to be utilised to reduce and clearly separate any speculative assertions in the material derived from the observations and results. We do however, wish to draw attention to Reviewer 3's comment that fundamentally, our logic and interpretations are sound. We feel that a prudent revision of the latter portion of the manuscript will relieve any concerns over the boundary between evidence and inference.
In terms of addressing the "quantitative" nature of the manuscript as presented, we would like to highlight the nature of englacial channels and the surveying thereof is not an insubstantial logistical challenge. Here, we have developed and employed the speleological techniques that the primary author has employed for 15 years (e.g. Vatne 2001; Vatne & Refsnes 2003) and ones that have been presented by other researchers in the field (e.g. Pulina 1984, Gulley 2009, Gulley and Benn 2007) techniques that have, to date, been viewed as robust. To our knowledge, and that noted by Reviewer 3, this is the first paper to present a true time-series (for which n > 2) of englacial channel morphology, allowing for a discussion of the likely processes of channel form and change. While we understand that the comparison of measurements taken over a decade, with the uncertainties we carefully declared, may not allow the type of quantitative analysis that could be achieved by repeat laser scanning or photogrammetric techniques, we feel our data does enable a broadly quantitative assessment of channel change. However, we accept the Reviewer's viewpoint as so will look to ensure the degree of "quantitative analysis" presented is made

clear at an earlier stage, and revise text which perhaps may overstate the precision of any magnitudes or rates of changes observed within our data.

That being said, this paper presents an important bridge between the fluvial geomorphology and englacial hydrology research communities, a relationship often overlooked by other englacial hydrology papers. I urge the authors to simplify and clarify their message such that their results will be more widely impactful in both communities.

Response

The Reviewer is kind to highlight the link between fluvial and glaciological hydrology that this manuscript aimed to contribute towards. We recognize that this view presented by the Reviewer mirrors those of the Reviewers 1 and 3, and while we are disappointed that our description of channel evolution tends to a more qualitative than quantitative exploration of the data, we concede that a careful reformulation of our material, and removal of elements which may appear 'tangential' to our primary thesis, would simplify the arguments presented here. Again, we note that Reviewer 3 sees the existing analysis of our morphological data as "sound qualitative explanations for [the channel's] morphological evolution". Therefore, we see no fundamental flaws that suggest a cautious edit of the existing manuscript would not suffice here. As detailed earlier in this response, we propose a reworking of the material in the discussion, but without substantial change to either the content or inferences draw, rather a reordering and shortening of the text.

SPECIFIC COMMENTS

Response

Below, the Reviewer provides a number of "technical" suggestions, interlaced with more substantial comments requiring a slightly more detailed response. 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.

Title:
Suggested change to "Morphological evolution of an englacial channel on Austre Broggerbreen, Svalbard"

Response

We thank the Reviewer for this suggestion, but we do not see any particular rationale to change the title. This view was not one revealed by either Reviewers 1 or 3. The location at which this work is based is clearly indicated in the Abstract, and while site specific, this is the first paper to report temporal changes in englacial channel morphology, and we feel it is of value to highlight this and engage the hydrological community in furthering research in the area of englacial channel dynamics. We would hope the paper may serve as a driver to engage more of the hydrological community in a field which to date has received rather limited attention, with much of the 'science' being presented in lower profile outlets.

Abstract:
Move sentence starting on L10 to first sentence L8: Delete 'albeit… channel counterparts.'
L15-22: Clarify that channel system is in rapid transition towards an equilibrium morphology that is reached within the survey timeframe
L22: Delete 'in light of this'
L24: Delete 'and role'

Introduction:
P7618 L2 Streams 'with respect to' meandering
P7618 L8,11 Repeated use of 'given way to'

P7619 L17 In lieu of either direct measurements of knickpoint face erosion rates or extensive erosion model parameter testing, I do not understand how this question will be quantitatively addressed.

Response

Our analysis does allow for a degree of quantification of the rates of change observed in the channel surveyed here. However, we do detail the limitations to this in our Methods section. To mitigate the Reviewer's concern here, we will look to rewording and reemphasizing the nature of "quantification" we are able to deliver with the data presented.

P7619 L18-20 Since this article does not present a complete conceptual model of channel formation and evolution to equilibrium state, this sentence over-reaches the presented conclusions.

Response
We are disappointed to see the Reviewer feels the material presented does not facilitate the development of a conceptual model for the morphological channel evolution. The channel surveyed here is known to be a cut-and-closure channel, and the formation and evolution of these channels has been described by other authors (e.g. Vatne and Refsnes 2003, Gulley et al 2009) Here, we do provide a conceptual model of how our data suggests such cut-and-closure channels continue to evolve over time, and towards what we described as a stable end point. However, this may not be an equilibrium state. We therefore thank the Reviewer for highlighting this lack of clarity in our meaning, and we will address this by revising the text to describe a channel that is in rapid transition towards a perhaps quasi-equilibrium state. Further, we note that Reviewer 1 also suggested some clarification of the channel form, perhaps in the field site description section, would be beneficial. We anticipate these revisions will address this concern.

Theoretical context:
P7619 L23 Specify that the following discussion will be about terrestrial streams flowing over rock, not englacial streams

Response
We thank the reviewer for noting this requires clarification, and we will revise the signposting in the appropriate sections to ensure readers are clear how the background section(s) are presented.

P7620 L25 Suggested re-wording: define as a critical slope segment preceded and followed by a shallow slope segment
P7621 L8-12 Delete sentence, unnecessary for logic flow
P7624 L28 Delete last sentence

Field Site:
P7625 L10 Unusual use of 'reduced dynamics'
P7626 L4 What was surface air temperature during that time? Is it surprising that liquid water was being stored?

Response
The surveys have been conducted at times when surface air temperatures ranged from -4 to -27 °C. However, surface air temperature is of little relevance as there is no way of transfer sufficient heat from the liquid water to the surface except from convective cooling of the conduit that starts when the snow lid is removed. Unfrozen water exists because the ice is not capable of conducting away the latent heat liberated as water freezes, hence large unfrozen pools exists at depth. There is substantial work highlighting the slow refreezing of water filled ice-walled channels (e.g. Lundarini 1988), water storage in englacial channels (e.g. Schroeder 1998) and interesting conceptual ideas relating to englacial conduit air circulation patters (Schroeder 2007). However, we simply reported the channel conditions in our manuscript, and do not see the direct relevance here; we are unclear if the Reviewer expected additional text to be included in a manuscript that was already assessed as being overlength. Consequently, we do not see a need to revise material here.

P7626 L13 What was error associated with difficulty using a magnetic compass at such a high latitutde, or what measures were taken to accurately measure bearing? This has been a consideration in other englacial mapping projects on Svalbard.

Response
It is, as the Reviewer points out, challenging to use a magnetic compass at high latitudes as it can be erratic. However, it is difficult to assess the errors as the measurements were made in an ice cave where celestial observations are impossible and communication links with alternatives such as GPS arrays are not available. However, the main purpose for the mapping was to look at relative changes in orientation between channel segments, within a limited area. As for other studies, back sighting was used to reduce compass errors. However, based on the problems using a magnetic compass for such studies, we have been careful in interpreting changes in planform between the individual surveys due to the potential errors in compass readings. We are able to include detail of the potential uncertainties here.

P7626 L16 Was inclination not measured from station to station? If not, how were channel slopes measured?

Response
A sentence addressing this was added.

P7626 L16-19 Delete sentence
P7626 L23 i.e. Steps were defined as slopes greater than 45 degrees? Clarify.

Response
Classification and delineation of channel elements is a challenging task, particularly in an englacial conduit where a wide range of channel slopes and forms exist. However, field observations suggest that at slopes lower than 45 degrees, pools where less pronounced, hence we focused on steep channel features where downstream forms where developed.

Results and data analysis:
P7627 L1 How were submerged step heights measured if the pool was frozen? Why should pool depth be greater for smaller steps and smaller for larger steps? Either clarify for delete this sentence.

Response
As indicated P767 L29 pool depth was not measured, but the pool bed could in many places be observed through the ice lid. The word "inversely" is deleted as it obviously is wrong, and a typographical error on our part, as also noted by the other Reviewers.

P7627 L5 Why exclude 'channel rapids' from analysis?
Knickpoints likely grow from small perturbations, rapids may reflect some intermediate stage of knickpoint development. . .

Response
We decided to focus on knickpoints for several reasons. The first is that many studies have shown knickpoints in the form of step risers to dominate channel evolution and incision. Moreover, steps are relatively easily to identify with a simpler morphology than the rapids, that were observed to vary in length and slope and also existing through channel curvatures. Hence to focus also on rapids would have needed more detailed channel surveys. We agree, rapids may represent a transition or initiation stage, but interpretation of this can be challenging. We will edit the text to ensure the rationale for excluding rapids for our assessment is made clear.

P7627 L7-14 Content in this paragraph belongs elsewhere in paper: cuspate forms in 'wall groove' section, conduit height measure- ment difficulty in survey technique section

Response
In response to the other Reviewers, we have reconsidered the inclusion of material on the cuspate forms, and the inferences that can be drawn from the limited observations we are able to present here. Consequently, all material referring to the cuspate forms will be revised, specifically alluding to the potential such morphological forms may have in providing quantitative details of channel development.

P7629 L10 The 'taller' the steps (ie not higher elevation in glacier)
P7629-30 and figures: watch consistency with terminology for wall grooves vs. grooves vs. cuspate forms vs. cuspate morphology throughout the manuscript (in text and figure captions)

Response
As noted above, following the advice of the other Reviewers, we will adapt the material relating to the cuspate forms. However, we thank the Reviewer for noting this is a section where we need to revisit our wording.

P7630 L26 Do you have data that shows that all knickpoints incise and migrate? You are careful to say that you cannot see individual knickpoint migration in Fig3 longitudinal profiles, yet here make interpretations on the individual knickpoint scale.

Response
Indeed, we do not have the data available to assess individual knickpoint adjustments, and we are disappointed the Reviewer would appear to be overlooking the time-frames between individual surveys which reveals this clearly. Moreover, due to the nature of the channel change we describe, our focus is on knickzones, and specifically the overarching change in channel profile, rather than the more localized variations which may also be associated with changes in meandering. We recognize that more frequent surveys of the channel would potentially help illustrate the likely behavior of knickpoints at a more localized scale, but with reference to the nature of knickpoint recession and incision in ice, with larger forms potentially masking or overwriting the smaller knickpoints, we also suggest that it may not be possible to identify individual or track such changes even at yearly

intervals. We will clarify our assertions here, but there is no opportunity to consider finer scale details than those presented.

P7631 L7 Sinuosity units of mm-1?
P7631 L12 No entrance B marked on map in figure 2

Response
The reference to entrance B is removed from the text.

P7631 L8 How do parallel channel walls indicate primarily vertical incision? Clarify.

Response
A sentence has been added to clarify this issue

P7631 L14 Figure 6 shows quite dramatic changes in meander location and size, and seems to suggest that significant lateral channel meandering does occur. The conclusion to not interpret these results seems poorly-founded, particularly given the current scientific interest in meander formation and evolution (a la Karlstrom et al., 2013). The argument that knickpoint migration masks this signal needs to be clarified. A schematic figure may be useful here. If differences between the 2000 and 2008 profiles in Figure 6 are not going to be commented upon, the figure holds little value.

Response
We used Figure 6 to illustrate the broadly similar overarching orientation of the channel throughout the survey period. We will emphasis this point by revising the text for clarity. Critically, we had rehearsed the notion that, due to the nature of the survey data, and uncertainty on absolute xyz positioning, "large upstream horizontal migration in the profile can relate to changes 5 in planform curvature". The Reviewer's observation here suggests we need to revise this, and ensure a reader is clear that apparent meandering in planform does not preclude marked changes in the vertical dimension. We note that the Karlstrom paper referred to by the Reviewer is a numerically driven paper, and while meander wavelength and migration are discussed, specifically relating to slope control, meander amplitude is less clear. Here, thanks to the Reviewer's observations, we will be in a position to clarify our meaning, to highlight the nature of meandering from the plan view perspective, and to demonstrate how, without fully georeferenced survey data, the inferences relating to the changing channel profile and meandering planform can only be subjective. However, we return to the Reviewer's earlier comment regarding precision of the magnetic compass bearing data, again emphasising that the focus here was the channel profile rather than plan view given the limitations of the data as we had detailed in the original manuscript text. Nonetheless, we are able to ensure this is made clearer to the reader.

Discussion:
P7632 L6 "This challenges the prevailing. . ." this is a key finding! Highlight it explicitly in the abstract and introduction.

Response
We will rewrite this part to highlight this finding

P7632 L10-26 This paragraph should be moved to the channel survey results section. P7632 L20 "Therefore the flow regime through the conduit is likely to be moderately stable.." Has this been studied? If so, please cite. In absence of a study, further explanation is needed, that the hydrograph of supraglacial streams is more stable than terrestrial counterparts is somewhat counter-intuitive.

Response
Here, we can see a simple clarification will address this concern. There are existing studies of meltwater flow regimes in the locality, and we will emphasise our meaning is that 'flood' events which may be more common for terrestrial streams in more maritime climates are less likely to be an occurrence here.

P7633 L24 Were channel adjustment rates measured? This should be detailed and results presented.

Response
Rates of adjustment can only be derived from comparison of the survey data presented here, and the Methods section make our surveying techniques clear. The Reviewer's comment appears to suggest there is a need to present high temporal resolution changes in channel or ice-wall position, something which has not been achieved, nor something that is readily assessed with ease during the hydrologically active season. However, we thank the Reviewer for indicating there is some

uncertainty in our language here. We will address this.

P73634 L25 This suggests 'that' LG channel reaches 'are' stable..
P7636 L20 'The direct effect is that step risers migrate. . . at several times the vertical incision" Is this based on measurements of only one knickpoint? More quantitative support of this argument would make it more robust.

Response
As we noted above in response to the concern over the ability to track changes for individual knickpoints between surveys, and in light of our existing caveats given over the nature and limit to our englacial survey data, the Reviewer seems to raise the same concern here. We appreciate that more quantitative data would be applicable here, but in the absence of this, and with respect to the challenge that surveying englacial channels presents, we are unclear of the Reviewers expectations. Our focus was on the overarching profile, and endeavoring to present a conceptual model to describe the changes we were able to measure, albeit relatively crudely. This is the first study to achieve this in a meaningful way. As we noted previously, we suspect that tracking individual knickpoints, even at the annual scale, in the absence of being able to measure channel geometry in real time, may not be readily achievable. Nonetheless, the Reviewer's opinion here is noted, and in revising the discussion, we will clarify the measurements we can make and the inferences we take from these..

P7639 L18 How was the submerged hydraulic jump observed and measured?

Response
This is an inferred element within our conceptual model of channel evolution. We are disappointed the Reviewer seems to suggest here, as elsewhere, additional quantified data should be presented for an environment which is poorly characterized in terms of even fundamental channel morphology, let alone numerical models. Direct observations of flow regimes and parameters in a closed but highly dynamic conduit with discharges of up to 5m^3/s are extremely challenging to make, and we reiterate that this is the first 'time series' of channel morphology. The timing of our surveys, being in the hydraulically inactive portion of the year suggest that assessing 'actual' hydraulic jump dimensions is not possible. Moreover, given we demonstrate the rapidity of changes in channel morphology for a channel characterized by a variable discharge regime, it would be clear that 'measuring' the hydraulic jump from singular observations of channel bed morphology would not necessarily be representative or appropriate. We have, as elsewhere, borrowed ideas from knowledge of supraglacial streams and the analogies held with other terrestrial studies, both in terms of observations and the field of hydrological engineering. However, we thank the Reviewer for raising concern over our language here, and we will address this with edits to the text here and elsewhere to ensure the data and our meaning is clearer, and that this as a conceptual understanding of the process is evident.

Conclusions:
P7641 L16 delete 'Vatne, unpublished data'
P7641 L19-23 Awkward/unclear phrasing, clarify OK
P7641 L25 AB not marked on map AB = Austre Brøggerbreen inserted in the text
P7641 L17 Separate sentences: We provide the basis for a conceptual model 'for the formation and stability' of step-riser geometry

Figures:

Response

The items listed for correction in the Figures are readily achieved, and we agree to the inclusion of scale either directly or through the figure captions. Reviewer 1 also noted the usefulness of flow directions, these we will include.

Figure 2: missing map elements AB, B?
Table 2: Vertical lines between year/LG, LG/MG, and MG/KZ
would ease interpretation. Clarify description of H and V in table caption.
Figure 3 Expand Y axis to the left so initial knickpoint is more easily visible.
Figure 4b Scale? 4d photo from below showing drops may be more insightful, if available
Figure 5 Scale?
Figure 6 What is flow direction? For section that loops under itself, make lower section dotted
Figure 7 Scale?
Figure 8 Scale? Could be combined with Figure 4.

TECHNICAL CORRECTIONS

Response
As noted above, we will correct and address all the following technical points. We thank the Reviewer for a thorough review here.

(typos and grammatical corrections) see Specific Comments for suggested language and clarification changes)
p7626 L27: precluded
p7627 L5: delete 'the' both the step
p7628 L28: add just upstream 'of' the meander
p7629 L5: add arguably 'the' definition
P7629 L29 we suggest 'that knickpoint morphology be divided'
p7631 L1 add comma In all surveys,
p7635 L3 delete next, 'the next,' albeit
p7635 L15 add subsequently receded
p7635 L21 delete s in leads
p7639 L14 delete 'the controls' the degree
p7639 L27 add 'retains' an equilibrium
p7640 L3 delete has 'for' long
p7640 L11 delete fluvial erosion 'erosion'
p7641 L8 add evidence 'suggests' the persistence
Figure 3 'Thalweg

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