We thank the reviewer for their comments which have helped us to improve the manuscript. Their key issues were regarding our discussion of ice streams and with the relative timing and mechanism of landform formation. In response to this we have discussed the possibility of a lack of preservation of geomorphic signatures beneath ice streams in addition to the possibility that the dominant drainage mode and the resultant signature may be different here. In terms of our discussion on ice streams and the potential for a highly channelised bed to moderate ice velocities, previous geomorphic studies (e.g. Storrar et al., 2014) and examples in the contemporary (e.g. Sole et al., 2013; Williams et al. 2020) and modelling literature (e.g. Lelandais et al., 2018) support the concept that a well-drained bed (i.e. due to increased channelisation) has the ability to stabilise an ice sheet by regulating ice sheet velocity.

In response to the reviewer comments we have also improved the link between the proposed model and the geomorphic signature in the updated manuscript and have better grounded this within a growing body of theoretical, experimental and contemporary evidence, which supports; (a) the occurrence of over-pressurisation of subglacial conduits; and (b) the potential ability for geomorphic work as a result. The very nature of large-scale geomorphic studies such as this focus on providing information on where drainage does or does not occur and how differing signatures can be related to background controls. As such, we do not focus on finer resolution signatures and we believe that our updated manuscript now clearly explains the temporal aspect of formation within the model (i.e. corridors forming over 10's - 100's years within the ablation zone and eskers forming near the margin) and the assumptions involved when making comments about changes over time inferred from changes over space. In addition to this, we have reduced our use of acronyms throughout to make the paper easier to follow.

Reviewer comments are in black and our responses are in green.

Kind regards,

Emma Lewington (on behalf of all co-authors).

The focus of the work was on the concentration of a variety of landforms of interpreted subglacial fluvial origin in corridors and then an explanation for this. Areas previously mapped as ice streams were noted to have fewer meltwater landforms. The suggestion that this was a matter of preservation potential of landforms in these locations was not addressed.

Thank you for pointing this out. We have now included a sentence considering the possible lack of preservation of features formed in this area due to enhanced erosion of fast flowing ice (e.g. Boulton, 1996). We also acknowledge that the presence of soft floored sediments may not result in the same geomorphic signature (i.e. water may be stored in till or organised into braided canals), and thus the absence of meltwater routes does not explicitly indicate the absence of efficient flow.

The timing of the formation of the landforms can be assumed by basic geologic principles. However, in this case, the authors suggested that the landforms represented approximately 1,000 years during deglaciation and were not overly concerned with a finer temporal resolution made possible by cross-cutting relationships or ice-marginal or ice-collapse features.

Within the study area we find no evidence of cross cutting of meltwater corridors or eskers. We do find eskers within meltwater corridors and have added in a section to make the temporal aspect of our model clearer - i.e. meltwater corridors were eroded within the ablation zone due to repeated pressure fluctuations over 10's to 100's years while eskers were most likely deposited near the ice margin. This paper presents a large-scale study and we are interested in the overall trend. We agree that more detailed morphology (e.g. esker splays and anastomosing esker sections) could be informative for high resolution ice margin retreat (e.g. Livingstone et al., 2020) but this is beyond the scope of this paper.

Without more geologic evidence, I was left with questions on: the contemporaneous nature and subglacial origin of features such as tunnels and similar-dimension positive- relief features; the subglacial origin and sedimentology of the hummocks; and the origin of splay landform which is similar to ice-marginal fans. I found the justification for their interpretations lacking. However, this is part of the very nature of a paper based on the cataloguing of land- forms from remotely sensed data: questions will remain.

A subglacial meltwater origin has been established for meltwater channels and tracks within the literature (e.g. Rampton, 2000; Utting et al., 2009; Peterson and Johnson, 2018) and we are not the first to suggest a potential continuum between these features (e.g. Sjogren et al., 2002; Peterson and Johnson, 2018). However, we agree that we may have overstated the importance of eskers surrounded by lateral splays as a potential positive relief expression of this. Furthermore, we acknowledge that there are issues grouping eskers with lateral splays with the meltwater channels and tracks as some or all of these features may have formed subaerially or even supraglacially at the ice sheet margin rather than subglacially. Therefore, we no longer include eskers with lateral splays within the term meltwater corridor (Table 2).

Hypothesis for formation will need to be tested with observations of the materials at the very least. Measurements made on modern glaciers or demonstrated in experimental work would also help provide credibility to their interpretations that landforms vary with the pressurization of the subglacial water system.

We agree that our proposed model for formation of meltwater corridors will need to be tested and we hope that publication of our paper might prompt such work. In terms of modifications to the paper, we have updated the proposed model section to demonstrate the increasing evidence from contemporary and modelling studies, which support the occurrence of conduit over-pressurisation and the potential for this to access and erode sediment from the bed. This process has been inferred from simultaneous field observations of ice velocity and uplift, surface melt and proglacial river discharge in contemporary settings and we suggest is likely to have occurred in Keewatin, an area located on resistant shield bedrock that would have experienced high surface meltwater production rates during the time period covered by our study. Therefore, we think there is sufficient evidence to support the plausibility of our model as a simple explanation of a wide range of geomorphic signatures.

This is not a novel hypothesis, nor one that explains all observed features of the glacier bed but it is the primary interpretation of the paper.

We disagree with the first part of this statement and believe we now provide sufficient evidence from the geomorphic and contemporary ice sheet and alpine glacier literature to support the conceptual model proposed within this study. We believe this paper and our proposed model provides support for viewing geomorphic evidence of subglacial meltwater landforms as a holistic signature and provides a more realistic understanding of the distribution of drainage beneath ice sheets.

We agree that our model does not ‘explain all observed features of the glacier bed’; it would be a large task indeed to explain everything and we don't claim to.

The explanation for ice streams lacking meltwater features because of their low surface slopes is not the only plausible hypothesis; fast moving ice over deforming beds will destroy the evidence of channel formation.

We agree and this has now been modified (please see first comment response).

However, the authors emphasize that a well-drained bed results in more stable ice. This is simply reversing the emphasis typically presented in ice-stream papers but using similar reasoning for the bimodal behaviour of ice flow.

There is evidence from the contemporary literature, which suggests that gradual dewatering of the bed is occurring in response to increased surface melt and inferred subglacial channelisation, which results in an observed year-on-year reduction in ice sheet velocity (e.g. Sole et al., 2013; Tedstone et al., 2015) and vice versa (Williams et al. 2020). Although this a temporal relationship (i.e. when surface melt is higher and subglacial drainage more channelised, regional ice velocity is lower) and our argument for Keewatin is spatial (i.e. where there is evidence for more channelised drainage there are fewer ice streams (e.g. Storrar et al., 2014)), we are invoking the same mechanism. This idea is also consistent with modelling of Lelandais et al., (2018), who suggest that efficient drainage (i.e. the development of tunnel valleys) can shut down ice streams and may be crucial for stabilising ice sheets during periods of climate change.

Non-standard and casual punctuation is pervasive throughout the paper: and/or, presence/absence, substrate/geology, splay/glaciofluvial, meltwater track/meltwater channel, erosion/deposition, streaming/surging. Please choose a conjunction or a single word. Hyphens are used in noun strings but not needed with adverbs. I would avoid unnecessary complicated acronyms: MW subscript route(s); MW subscript track(s), MW subscript corridors; VPA–variable pressure axis (a new one for me); GrIS for Greenland Ice Sheet (couldn't see where you defined that one). What is the point of using any of them in an online manuscript with no limits on characters or words? They are also not used consistently, e.g. line 516.

We understand that the use of acronyms may have been frustrating and made the paper more difficult to follow. We have now reduced the number of acronyms used and have made sure to state clearly what they refer to when using them for the first time. We have also amended the non-standard and casual punctuation throughout.

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