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Peer Review File

Seasonal freezing increases High Arctic erosion and landscape response to climate extremes

Corresponding Author: Mr Jonas Eschenfelder

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Mr Eschenfelder,

Your manuscript titled "Seasonal freezing increases High Arctic erosion and landscape response to climate extremes" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Nicola Colombo, PhD
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

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DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This impressive work represents a significant advance beyond Chartrand et al., 2023, Nature Communications. By employing technically impressive flume experiments, this study demonstrates that frozen riverbeds in cold regions can incise more rapidly than their unfrozen counterparts, driven by localized riverbed melting and intensified convective overturning in the early thaw season. The experiments are carefully designed and well executed, and the theoretical framework is coherent and compelling. The manuscript offers a valuable new perspective by showing that coupled thermal and erosional processes in frozen riverbed have strong control on riverine landscape evolution in cold regions, which will be of broad interest to geomorphology and fluvial research communities. However, I have several concerns about how well the experiments represent real-world processes, as some of the experimental setup may not fully align with actual conditions in cold-region rivers. These issues, together with recent literature context, need further clarification and discussion.

Specific comments:

1. Line 517 to 528, the experiments use water at room temperature (18 °C) to thaw the frozen particle bed, which is greatly warmer than typical river water in cold regions (general < 10 °C). Such high temperature would be expected to accelerate thawing of the frozen riverbed. However, I am surprised that the experiment with 8°C water results in greater erosion than the 18°C runs. Additional discussion or sensitivity analysis is needed to clarify the physical mechanism by which colder water leads to a higher bed-erosion rate than warmer water. Furthermore, it would be helpful to justify why the 18 °C experiment is presented in the manuscript, as the 8 °C experiment appears more representative of cold-region rivers.

2. Although the field photos on Tallurutit (Fig.4) show step-like morphology that is consistent with the flume experiments, such features could also be produced by other geomorphic processes such channel narrowing and local slope breaks. Moreover, the thaw-depth measurements (Table 3) do not provide convincing evidence for localized TFUs. Thaw depths around the channel appear shallower than those farther away, which seems to contradict the proposed mechanism of deepening TFUs beneath the channel. More field evidence should be provided to support the interpretation presented in Fig. 4.

3. The 2-cm flume width constrains the system to 2D behavior, which may limit its ability to capture the full lateral transport of water and heat that occurs in natural river channels. In real rivers, beds along the outer and inner banks experience markedly different hydraulic forcing and thermal exposure, might result in different balances between thermal and mechanical erosion.

4. In addition, permafrost riverbeds commonly contain taliks formed by the heating effects from relatively warm river water, leaving little or no frozen substrate in which TFUs could develop. Therefore, the accelerated incision described in the manuscript may be restricted to small, shallow, or seasonally ephemeral streams, rather than typical rivers with well-established taliks. A clearer discussion of the field conditions under which this thermal-controlled incision is expected to occur would enhance the manuscript’s generality.

5. I suggest improving the readability of Fig. 4d. Although the caption provides very extensive detail, the figure itself remains difficult to read because too many elements are presented. Separating key components into subpanels would help readers better understand.

6. The reference list does not acknowledge many recent progresses on warming-driver landscape dynamics in cold regions. In particular, the introduction and discussion section could thus be much improved by placing this study in a recent context. See example below and more relevant works from others:
https://www.nature.com/articles/s43017-022-00362-0
https://www.science.org/doi/full/10.1126/sciadv.adi5019
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL111536
https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2024GL112752

Reviewer #2 (Remarks to the Author):

Eschenfelder et al. introduce a new process, “coupled thaw-entrainment,” to argue that cold-region landscapes may respond more rapidly to climate change than their temperate counterparts. Although I am not an experimentalist, I appreciate the substantial effort invested in the experimental design and the thorough documentation of the methods. The results are of clear interest to permafrost and geomorphology researchers and contribute novel insights to the literature.

However, I found the study somewhat dense for a broad cryospheric audience, and I would have welcomed greater emphasis on the broader implications of the results (L330–373), which constitute much of the abstract and the stated contributions. As currently presented, the climate-related interpretations feel unconvincing and somewhat vague (similarly, the language throughout used to refer to ‘climate extremes’). I did find the example on L350–355 helpful, and additional quantitative examples of that kind would strengthen the manuscript.

Please see additional suggestions below.

L12 Word choice, I’d suggest ‘advect’ heat instead of ‘deliver’.

L15 Melting vs thaw?

L30-31 What is meant by ‘early season climate extremes’? Air temperature, ground temperature, snow depth, rainfall, etc? Be more precise.

L75, Ideally, you would refer to Figure 1d,e before Figure 1f,g. Please flip these subfigures in Figure 1 so you can refer to them alphabetically.

L85, Unclear how Figure 1d,e support this sentence, remove.

Figure 1e, I don’t understand this plot as presented. Please simplify.

Figure 1, Please reduce caption length.

L102, Again, watch referring to figures out of numerical order.

L106, What is meant by ‘wet’? Rain? Snowfall? Snow depth? Be specific.

L138, I see that TFU is thaw front undulation on L717, but I don’t see it defined where it is first used.

L155 and 157, What do these ‘jets’ represent in reality? High magnitude recharge events? I’m having trouble visualizing seepage entering the subsurface as a concentrated ‘jet’ of water. I see this is short shortcoming that is particularly discussed on L944-947.

Figure 4d, I find this figure hard to digest with so many axes. It is referred to many times; I would expand it into multiple subfigures that could be simplified and include more quantitative examples similar to Supplementary Figure 7.

L325-326, A field photo to illustrate the discontinuous channelization would be beneficial here. The discontinuous channels are not obvious to me in Figure 4b. Maybe a UAS image?

L330-373, I find this section to be the crux of the paper, but it seems to have little experimental backing other than using the hypotheses proposed in Figure 4d to suggest what may happen under different climate conditions. Could you explore these scenarios in the model setup or mathematically?

Line 351-352 is a great example, but it’s unclear to me where this shows up in Figure 4d, Supplementary Figure 7 is more fitting. Also, I may have missed this, but how are air temperatures translated to ground temperatures considering snowpack insulation?

L357, “increased climate variability” is vague; please revise.

Methods, How did particle size and porosity affect experimental results? How does having glass beads vs loess or organic-rich particles common to permafrost settings affect results? Why were glass beads selected? I may have missed this, but were any sensitivity analyses run to address substrate materials?

Reviewer #3 (Remarks to the Author):

In “Seasonal freezing increases High Arctic erosion and landscape response to climate extremes” the authors demonstrate that partially frozen riverbeds can experience faster erosion rates than completely thawed riverbeds due to jets of pore flow. These jets were observed in beautifully executed frozen flume experiments that traced undulations in the thaw front formed by flow between glass sediment grains. The authors used experimental results to propose a new quantitative model for how focused subsurface flow increases particle lift forces, locally enhancing erosion. They then plot up their experimental results in a new phase space designed to separate flow and thermal conditions when thaw front undulations will grow or shrink versus when sediment entrainment will be uniform or large but sporadic. This phase space is then used to investigate what would happen under changing climate conditions, revealing that thaw front undulations tend to persist through time and cause streams to remember conditions from earlier thaw seasons and be very sensitive to early summer temperatures.

First, I want to commend the authors on their experiments. Frozen flume experiments are extremely difficult to accomplish, so very few are published, and inevitably involve tradeoffs between scaling hydraulic and thermal nondimensional parameters. These experiments minimize those tradeoffs to produce a novel and surprising dataset. The authors are clearly familiar with existing quantitative methods to scale their results to natural systems, and their novel phase space for spatial variability in thermal versus erosional processes is a concise way to display their results.

While I think the experimental results and interpretations are robust, there are improvements that could be made in the following areas:

1. Connecting the flume experimental results to the field scale. It is not completely clear where these results are applicable. The experiments were conducted under “pick up” conditions, where downstream sediment flux is far below transport capacity. Depending on sediment supply to a given channel reach, this may or may not be the case, particularly for major rivers with large, persistent taliks. Maybe there are other rivers with enough information to plot in Fig 4d? (e.g., McNamara et al., 2008; Scott, 1978; Smith, 1975; Wankiewicz, 1984)
2. Making the results clear with the methods at the end. I found it challenging to get an intuition for where streams should plot in Fig 4d just from the main text.
3. The paper is missing some key references, some of which I list below. In particular, the Roux et al. (2017) experiments on river talik thaw rates should be discussed.

With relatively minor edits to more comprehensive frame the experimental results and theory, this paper will be an invaluable contribution to the permafrost literature.

Line comments

Line 60: Other permafrost erosion experiments should be referenced as well (Costard et al., 2003; Dupeyrat et al., 2011; Roux et al., 2017).
Lines 163-165: Here, the local variation in topography is approaching 1/2 the flow depth. Do you think this could cause morphologic drag and decrease skin friction, therefore decreasing sediment transport in the unfrozen case?
Line 138: Please define TFUs here since it's the use of the acronym.
Line 322: How do floods affect this phase space? I am curious if the channel and dimensions of TFUs are mostly shaped by infrequent floods or day-to-day conditions.
Lines 330-334: I found these thought experiments a bit confusing because many processes could alter the temperature of rain after it falls to the ground. For instance, warmer air temperatures could melt more ground ice and cause water near freezing to flow downstream. I would specify exactly which variables you are changing then give a range of scenarios that could cause that variation.
Lines 351-355: This is a very interesting result! But it is not clear to me from just looking at the figure. What specific "landscape response" do you expect?
Figure 4a-c: I find this figure kind of confusing since it implies that the thaw front undulations parallel the channel bed but there is spatially uniform erosion. Is that the case? It would be helpful to plot the topography and thaw front to show if those undulations are in or out of phase.
Figure 4d: Can landscape memory only go one direction on this plot? Also, it seems like the localized erosion you found in your flume experiments likely doesn't happen in the channel you visited. Which natural channels would plot with phi < 1?
Line 486: Supercritical flows! Do you expect this to have any affect on the evolution of your thaw front? Is the relation for pore flow you are using appropriate for this case? Presumably the flow goes subcritical when it enters the pore space but then has to go supercritical again when it exits, and I am not sure how that would affect your energy balance calculations.
Line 489: This is a pretty high critical Shields number, even given the 5% slope. Any idea why?
Lines 729-731: I think psi and delta psi are both defined to be undulation amplitude here… is that correct?
Lines 745-748: How do these values compare with heat produced by turbulent dissipation? I don't expect that to change your results, but it would be good to include in your order-of-magnitude calculations.
Line 872: How did you pick a Manning's n? Please present field data or cite a reference (e.g., Arcement & Schneider, 1989).
Lines 897-898: I don' t see the grey ellipse on Figure 4d.
Extended Data Fig 1: V_w should be 25 cm/s based on main text.
Extended Data Fig 3: Please define all variables in the caption.
Extended Data Fig 6c: What do the dashed versus solid red and blue lines mean?
Extended Data Fig 7: Please label plot axes.

References cited in review
Arcement, G. J., & Schneider, V. R. (1989). Guide for selecting Manning's roughness coefficients for natural channels and flood plains (Water-Supply Paper No. 2339). Water Supply Paper (p. 38). Denver, CO: U.S. Geological Survey. https://doi.org/10.3133/wsp2339
Costard, F., Dupeyrat, L., Gautier, E., & Carey-Gailhardis, E. (2003). Fluvial thermal erosion investigations along a rapidly eroding river bank: Application to the Lena River (central Siberia). Earth Surface Processes and Landforms, 28(12), 1349–1359. https://doi.org/10.1002/esp.592
Dupeyrat, L., Costard, F., Randriamazaoro, R., Gailhardis, E., Gautier, E., & Fedorov, A. (2011). Effects of ice content on the thermal erosion of permafrost: Implications for coastal and fluvial erosion. Permafrost and Periglacial Processes, 22(2), 179–187. https://doi.org/10.1002/ppp.722
McNamara, J. P., Oatley, J. A., Kane, D. L., & Hinzman, L. D. (2008). Case study of a large summer flood on the North Slope of Alaska: Bedload transport. Hydrology Research, 39(4), 299–308. https://doi.org/10.2166/nh.2008.006
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Smith, M. W. (1975). Microclimatic influences on ground temperatures and permafrost distribution, Mackenzie Delta, Northwest Territories. Canadian Journal of Earth Sciences, 12(8), 1421–1438. https://doi.org/10.1139/e75-129
Wankiewicz, A. (1984). Hydrothermal Processes Beneath Arctic River Channels. Water Resources Research, 20(10), 1417–1426. https://doi.org/10.1029/WR020i010p01417

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Version 1:

Decision Letter:

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Dear Mr Eschenfelder,

Your manuscript titled "Seasonal freezing increases High Arctic erosion and landscape response to climate extremes" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Nicola Colombo, PhD
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

TThank you for the opportunity to review this revised manuscript. The authors have done well to comprehensively address my previous concerns and comments on the manuscript, and I sincerely thank them for their efforts in that regard. The manuscript is, in my opinion, now acceptable for publication. I would recommend an editor's highlight for this paper for its broad interests to the community of hydrology/geomorphology and water resources stakeholders.

Reviewer #2 (Remarks to the Author):

Eschenfelder et al. experimental design suggests that periglacial fluvial erosion is sensitive to early-season weather events, and cold-region landscapes may respond more rapidly to climate change than their temperate counterparts. The authors have thoroughly responded to reviews, and I appreciate their detailed response to my prior comments. I especially found the revised Figure 4 and expanded discussion that includes how different weather events modulate a channel's erosional and thermal evolution helpful for contextualizing this experimental study. The only minor comment I noticed in my re-read, which was focused on the revised text, was that the 'p' in pressure in the new Figure 4a is cut off. It is also unclear what the top wavy blue line is in Figure 4a (I assume the surface water boundary?), and suggest changing it and the color of its associated text. The results of this study are of clear interest to permafrost and geomorphology researchers and contribute novel insights to the literature.

Reviewer #3 (Remarks to the Author):

The authors did a nice job in their revisions, and the addition of field data in the Discussion section and splitting Figure 4 into Figures 4 and 5 made it much clearer how the experiments relate to field observations.

My only request is that the authors add a sentence in the Discussion or Methods explaining how differences in Fr and Re between the experiments and field should not significantly change the resulting TFUs. The authors clearly responded to my previous comment on Fr>1 and rapid pore flow velocities (referencing original submission line 486), but did not make any revisions to the manuscript. I think the Fr and Re scaling is important to mention in the paper since supercritical flows can form antidunes and transitional flows have lower sediment transport capacity than fully turbulent ones. I am happy for the editor to administer this minor change and/or discuss with the authors then send the paper directly for publication.

RE: Decision on manuscript COMMSENV-25-5839-T

Dear Mr Eschenfelder,

Your manuscript titled "Seasonal freezing increases High Arctic erosion and landscape response to climate extremes" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Nicola Colombo, PhD
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability

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Response: First, thank you for the opportunity to address the comments raised by the peer reviewers, and for handling our submission. We appreciate your time and support. We found the three reviews constructive and very helpful, and below we detail our responses and the revisions made to address the comments and suggestions. Before presenting the detailed responses, we first provide a summary of the key and more significant revisions completed in order to help you efficiently assess our revised submission:

- **Readability of the original Figure 4:** A key concern raised by all three reviewers was the readability of the original Figure 4. We agree with the reviewers that the original version was information dense, making it difficult to follow and understand the thought experiments we present. As a result, we removed the original Figure 4 and replaced it with two new figures. First, a new Figure 4 that shows how the landscape memory effect may conceptually work across spatial scales. Specifically, the new Figure 4 includes an image from a trial experiment we ran which included pre-formed surface step to explicitly evaluate step effects to shallow subsurface flow dynamics, a drone image from our Tallurutit field site, and an airborne image from a helicopter flyover of Muskox valley on Axel Heiberg Island (Chartrand et al. 2023). Secondly, a new Figure 4 that shows individual regime diagrams for each scenario separately. We think it is easiest to follow and understand the thought experiments if the regime diagrams occur together in one figure and the conceptual model is presented separately. We hope that combined with the expanded discussion of each scenario in the main text the revised and new figures improve the interpretation and understanding of our ideas and helps readers to more easily follow the thought experiments we present.

- **Expanded extreme weather event analysis and discussion:** Both Reviewers 2 and 3 raised comments around our discussion on the impact of extreme weather events, asking for more clarification and expansion of the discussion on this point. We have addressed these comments by significantly expanding the discussion section in conjunction with the new and revised figures. We expanded our treatment of heatwaves and rain events to explicitly take into account the duration of the event and have updated the figures and numbers presented in the text accordingly. For this we added a new Methods section titled "Exploring the effect of extreme weather events on $\Phi - \Gamma$ evolution" that explains how we apply each extreme event, with special emphasis on heatwave calculations and rain events. We illustrate how the new heatwave model works in a Jupyter notebook provided as Supplementary Code available on the FigShare.

- **Applicability to field conditions:** Our work is motivated by the relatively rapid formation of new first-order streams in the Canadian High Arctic (a context we hold as the most directly applicable prototype setting/conditions) and a research gap in the current literature surrounding grain-scale physics which modulates riverbed erosion in permafrost environments. As such, we have designed our experiments focused on the near threshold conditions and with no upstream sediment supply to isolate net transport of sediment out of the flume. In an effort to isolate the effect of ground ice on riverbed erodibility and identify the key physical processes responsible, we abstracted the problem in our experimental design. Since we are starting into the stated problem from the grain-scale with very little prior literature on the topic available, we believe our experimental approach provides a solid foundation from which to build further understanding by incrementally adding more complexity to the problem. We are presently pursuing this research goal.

  We added context to both the introduction (L76-84) and discussion (L324-330) to make this motivation clearer. However, the physics presented here is surprisingly not necessarily at odds with application to larger rivers in the presence of talik. The experiments by Roux et al. (2017) are an important step in understanding heat transfer between rivers and their beds. However, it is important to note that these experiments were run with air temperatures above 0 °C throughout and did not explore the thermal state of a riverbed during the winter. Somewhat surprisingly, field observations show that, whereas talik remains unfrozen throughout the cold season at depth, the riverbed can freeze during winter and remain frozen until after ice breakup (e.g. Fig. 9 Wankiewicz 1983). Similarly, numerical simulations by Liu et al. (2022) show how talik can become hydraulically isolated from the free surface flow as the riverbed freezes. These prior results suggest that the observations of surface water injections during our experiments and the resulting pore-water buoyancy forces can arise in larger river systems. Notably, we did not expect nor anticipate the possible (landscape) scaling up of our experimental results, but the two prior studies referenced above suggest it is plausible based on what is presently understood. Last, since we did not vary sediment supply in our experiments, we cannot discuss whether or how coupled thaw-entrainment may manifest in larger rivers, but we are planning on exploring this parameter space further as we expand upon and elaborate our ideas with additional experiments, fieldwork and numerical modelling.

- **Temperature effect on particle flux:** To adequately address the comment by Reviewer 1 that they were surprised about the colder water temperature experiments yielding a greater total mass eroded, we analysed two additional frozen experiments with 8 °C surface water. We added two supplemental figures based on this analysis. Supplementary Figure 8 showing how a slower thaw rate concentrates mechanical energy in a shallower thawed layer which allows for more erosion work to be done. Supplementary Figure 9 comparing the particle flux data for the cold-water and room temperature experiments used in the main body, showing how normalising by the Stefan depth takes into account water temperature.

Reviewer #1 (Remarks to the Author):

This impressive work represents a significant advance beyond Chartrand et al., 2023, Nature Communications. By employing technically impressive flume experiments, this study demonstrates that frozen riverbeds in cold regions can incise more rapidly than their unfrozen counterparts, driven by localized riverbed melting and intensified convective overturning in the early thaw season. The experiments are carefully designed and well executed, and the theoretical framework is coherent and compelling. The manuscript offers a valuable new perspective by showing that coupled thermal and erosional processes in frozen riverbed have strong control on riverine landscape evolution in cold regions, which will be of broad interest to geomorphology and fluvial research communities. However, I have several concerns about how well the experiments represent real-world processes, as some of the experimental setup may not fully align with actual conditions in cold-region rivers. These issues, together with recent literature context, need further clarification and discussion.

Specific comments:

1. Line 517 to 528, the experiments use water at room temperature (18 °C) to thaw the frozen particle bed, which is greatly warmer than typical river water in cold regions (general < 10 °C). Such high temperature would be expected to accelerate thawing of the frozen riverbed. However, I am surprised that the experiment with 8°C water results in greater erosion than the 18°C runs. Additional discussion or sensitivity analysis is needed to clarify the physical mechanism by which colder water leads to a higher bed-erosion rate than warmer water. Furthermore, it would be helpful to justify why the 18 °C experiment is presented in the manuscript, as the 8 °C experiment appears more representative of cold-region rivers.

Response: Thank you for this constructive comment. We agree that, at first glance, warmer water would be expected to enhance thaw and erosion, and the observation of greater erosion under colder water therefore merits careful explanation. Below, we first clarify the experimental rationale for using 18 °C water and then explain the physical mechanism for the enhanced erosion in the cold-water experiments.

The 18 °C water temperature is indeed warmer than typical field conditions, especially in the High Arctic where we have focused our work, although similar water temperatures have been observed in the Arctic during the Summer (e.g. Wankiewicz 1984). As discussed in the Methods (L624-633), we decided to run the experiments at room temperature to accelerate the thawing and reach deeper thaw depths while keeping the lower boundary conditions consistent throughout. Importantly, we designed the experiments to thaw from the bed surface down, and not from the base of the flume up. We enforce this boundary condition by running coolant in a heat exchanger that forms the base of the flume. The thick low-thermal conductivity acrylic that forms the sidewalls provides an approximately insulating boundary condition at the sidewalls over the duration of our experiments. On the basis of many test experiments, slow heat transfer from the room, across the thick sidewalls and into the experiment has no effect provided the experimental duration is less than about 60 minutes. This limiting duration works well with experiments completed using ~18 °C water. This is the practical reason for selecting this water temperature. By non-dimensionalising experiment time using a 1D Stefan solution, we explicitly take into account varying thaw rates due to different water temperatures as we show in the new Supplementary Data Figure 9.

The greater total mass eroded during the 8 °C water temperature experiments is related to the dynamic pressure effects captured in $\mathbb{F}_{dyn}$, which is inversely proportional to the thaw depth (Methods Eq. 16). In the 30-minute experiment time, $\mathbb{F}_{dyn}$ remains larger throughout compared to the 18 °C water temperature experiments, enhancing particle entrainment and resulting in an overall greater mass eroded. To illustrate this point we added new Supplementary Figure 8 that shows the thaw depth, $\mathbb{F}_{dyn}$, power density, and total energy delivered per unit area of the particle bed for a 30-minute experiment. While less thermal energy is supplied in the cold-water experiments, the mechanical energy supplied to the bed is the same in both cases (Supplementary Figure 8d). However, because the thaw front is, on average, shallower in the cold-water case (Supplementary Figure 8a), the supplied mechanical energy is concentrated in a smaller layer (Supplementary Figure 8c). The resulting stress concentration drives relatively enhanced erosion. This result is consistent with the energetic argument for $\mathbb{F}_{dyn}$ and equivalent to the dynamic pressure gradient explanation presented in the main text of the paper. We added a brief discussion regarding this energetic argument in the results (L186-193). See the relevant passage below:

"In an unfrozen bed, the momentum flux supplied by these jets is dissipated throughout the permeable matrix without doing any erosional work. By contrast, the thaw front in the frozen bed is an impermeable boundary that redirects impinging jets and concentrates the supplied mechanical energy within the thawed layer, to do erosive work."

We furthermore added the new Supplementary Figure 9 comparing the median particle flux across two 30-minute experiments with cold water to the median flux in the room temperature experiments. When converting experiment time to at Stefan depth for each temperature shows that the two set-ups collapse after approximately 4 particle depths.

2. Although the field photos on Tallurutit (Fig.4) show step-like morphology that is consistent with the flume experiments, such features could also be produced by other geomorphic processes such channel narrowing and local slope breaks. Moreover, the thaw-depth measurements (Table 3) do not provide convincing evidence for localized TFUs. Thaw depths around the channel appear shallower than those farther away, which seems to contradict the proposed mechanism of deepening TFUs beneath the channel. More field evidence should be provided to support the interpretation presented in Fig. 4.

Response: Thank you for raising an important point regarding the interpretation of the field observations at Tallurutit. We agree that step-like channel morphology can arise from multiple geomorphic processes, including channel narrowing and local slope breaks. We show that the step-like channel morphology is consistent with our hypothesis. The thaw depth measurements shown in Supplementary Table 3 were taken along the midline of a channelised segment, which we state in the Methods (L963-972). To address the reviewer's concerns, we added a new Supplementary Figure 3 to show the undulating thaw front in the Transect and cross-section sketches to clarify the sampled geometry. We also added a second thaw depth transect we took between two connected pools and the over-steepened section shown in Supplementary Figure 2c, which shows a deepening thaw depth underneath the step going into the pool.

3. The 2-cm flume width constrains the system to 2D behavior, which may limit its ability to capture the full lateral transport of water and heat that occurs in natural river channels. In real rivers, beds along the outer and inner banks experience markedly different hydraulic forcing and thermal exposure, might result in different balances between thermal and mechanical erosion.

Response: We agree that the narrow width of the flume limits the effects of lateral heat and momentum transport that will occur in a natural river. However, the goal of these experiments was not to recreate a full frozen river at lab-scale but to explore the grain-scale processes that drive sediment transport and thawing. We chose such a narrow flume following other small-scale experiments such as the ones presented by Ancey et al. 2006 and Deal et al. 2023 to focus the problem as much as possible and capture the key physical processes. The narrow width also allows us to assume that the thaw front geometry and subsurface water flow we observe through the transparent side walls are representative across the flume width. We are working on expanding on these initial experiments and hope that our work will help guide other groups to explore the seasonal variability of riverbed erosion through the lens of subsurface flow and heterogeneous heat and momentum fluxes as well.

4. In addition, permafrost riverbeds commonly contain taliks formed by the heating effects from relatively warm river water, leaving little or no frozen substrate in which TFUs could develop. Therefore, the accelerated incision described in the manuscript may be restricted to small, shallow, or seasonally ephemeral streams, rather than typical rivers with well-established taliks. A clearer discussion of the field conditions under which this thermal-controlled incision is expected to occur would enhance the manuscript's generality.

Response: This is also an important comment, and one which we ourselves thought about prior to receiving feedback from the 3 reviewers on our original submission, and our subsequent reading of additional select literature contributions. It appears that the evidence suggests it is possible for a riverbed to freeze in between talik and an overlying free surface flow (which may be capped by river ice). Please see our general response above where we specifically address this comment.

5. I suggest improving the readability of Fig. 4d. Although the caption provides very extensive detail, the figure itself remains difficult to read because too many elements are presented. Separating key components into subpanels would help readers better understand.

Response: Thank you for this important comment – we agree with the reviewer. Please see our general response above where we specifically address this comment.

6. The reference list does not acknowledge many recent progresses on warming-driver landscape dynamics in cold regions. In particular, the introduction and discussion section could thus be much improved by placing this study in a recent context. See example below and more relevant works from others:
https://www.nature.com/articles/s43017-022-00362-0
https://www.science.org/doi/full/10.1126/sciadv.adi5019
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL111536
https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2024GL112752

Response: Thank you for the suggestion of additional papers to consult and incorporate into our work. We have added Sha et al. 2025 and Zhang et al. 2023 to the introduction (L64).

Reviewer #2 (Remarks to the Author):

Eschenfelder et al. introduce a new process, "coupled thaw-entrainment," to argue that cold-region landscapes may respond more rapidly to climate change than their temperate counterparts. Although I am not an experimentalist, I appreciate the substantial effort invested in the experimental design and the thorough documentation of the methods. The results are of clear interest to permafrost and geomorphology researchers and contribute novel insights to the literature.

However, I found the study somewhat dense for a broad cryospheric audience, and I would have welcomed greater emphasis on the broader implications of the results (L330–373), which constitute much of the abstract and the stated contributions. As currently presented, the climate-related interpretations feel unconvincing and somewhat vague (similarly, the language throughout used to refer to 'climate extremes'). I did find the example on L350–355 helpful, and additional quantitative examples of that kind would strengthen the manuscript.

Response: Thank you for these comments and a chance to expand more on the implications of this work. In order to be brief, we used 'climate extremes' as a shorthand for talking about the various effects of extreme weather events. Specifically, here we have looked at heatwaves, rainstorms, and cold snaps that cause refreezing. However, we appreciate that this made our paper too vague at times. We changed from using 'climate extremes' to 'extreme weather events' where applicable and expanded on the discussion more.

Please see additional suggestions below.

L12 Word choice, I'd suggest 'advect' heat instead of 'deliver'.

Response: Changed it to 'advect'.

L15 Melting vs thaw?

Response: We changed it to thawing to be consistent with the rest of the manuscript.

L30-31 What is meant by 'early season climate extremes'? Air temperature, ground temperature, snow depth, rainfall, etc? Be more precise.

Response: We now specify heatwaves and rainstorms as the examples we discuss in this manuscript.

L75, Ideally, you would refer to Figure 1d,e before Figure 1f,g. Please flip these subfigures in Figure 1 so you can refer to them alphabetically.

Response: Flipped the panels in the figure and updated the figure references accordingly

L85, Unclear how Figure 1d,e support this sentence, remove.
Response: We removed the reference to the figure.

Figure 1e, I don't understand this plot as presented. Please simplify.

Response: The aim of this figure is to show how the particle flux and advection of heat evolve together and that the break in the $1/\psi$ particle flux relationship coincides with when the Nusselt number drops below 1 again, indicating diffusive heat transfer dominates, as this is how we define the early versus late time regime. We appreciate that this panel is information dense, however we think it is important to show both the particle flux and thermal evolution together to show this relationship clearly. We changed the colour of the secondary y-axis to be the same as the associated Nu curves to make it easier to read.

Figure 1, Please reduce caption length.

Response: We removed some information that was duplicate to the main text to shorten it.

L102, Again, watch referring to figures out of numerical order.

Response: We added a reference to Fig. 3 above (L114).

L106, What is meant by 'wet'? Rain? Snowfall? Snow depth? Be specific.

Response: We removed the 'warm and wet' to specifically mention heatwaves and early season rainstorms.

L138, I see that TFU is thaw front undulation on L717, but I don't see it defined where it is first used.

Response: Thank you for catching this, we added in the definition here (L159).

L155 and 157, What do these 'jets' represent in reality? High magnitude recharge events? I'm having trouble visualizing seepage entering the subsurface as a concentrated 'jet' of water. I see this is short shortcoming that is particularly discussed on L944-947.

Response: At the grain-scale, the jets are actual waterflows that are forced into the subsurface through the processes discussed. In general and as is well known, there is a continuous exchange of surface and subsurface water through a riverbed in the so-called hyporrheic zone via seepage flow (Sophocleus 2002). However, we avoided using the term 'seepage flow' to describe the injections we observe in the experiment because seepage flow is typically described by fully viscous Darcy flow whereas the jets that get deflected at the thaw front also have an inertial component to them, especially at early times when $\Phi >> 1$. We use the term "jet" to refer to surface water injections in the paper to acknowledge their inertial component as they deliver excess momentum flux to the bed and because their flow direction is not along the large-scale hydraulic gradient (downslope) as we would expect from fully viscous Darcy flow (see Figure 4b for one visual example). To further address this comment, we hope that the new Figure 4 helps visualise how these concepts could scale to the landscape.

We included the uncertainties discussion in the Methods (L1172-1199) to acknowledge that in our experiments we had continuous surface water flow and as such a fully saturated bed throughout, while in small, intermittent channels the bed may not be fully saturated throughout the thaw season, especially in polar deserts that we are focusing on in our work. As such, there are potentially other effects that modulate how much erosion a given magnitude-duration rainfall event will cause across a thaw season that are not yet captured by our regime space.

Figure 4d, I find this figure hard to digest with so many axes. It is referred to many times; I would expand it into multiple subfigures that could be simplified and include more quantitative examples similar to Supplementary Figure 7.

Response: Thank you for these suggestions, which are consistent with suggestions from other reviewers. We have followed these suggestions by splitting up Figure 4d into subpanels in the new Figure 5 . We also expanded our extreme weather event analysis by now taking event duration into account explicitly. The new analysis is now in the Methods as a separate section titled "Exploring the effect of extreme weather events on $\Phi - \Gamma$ evolution" (L1041-1169).

L325-326, A field photo to illustrate the discontinuous channelization would be beneficial here. The discontinuous channels are not obvious to me in Figure 4b. Maybe a UAS image?

Response: Thank you for this suggestion. In the new Figure 4 we now show an aerial image from Muskox Valley on Axel-Heiberg Island (Chartrand et al. 2023) as an example of the larger-scale discontinuous channelisation we discuss in the text.

L330-373, I find this section to be the crux of the paper, but it seems to have little experimental backing other than using the hypotheses proposed in Figure 4d to suggest what may happen under different climate conditions. Could you explore these scenarios in the model setup or mathematically?

Response: Thank you for giving us the opportunity to expand on these thought experiments. Our goal with this section is to highlight how we can use this new regime diagram to explore how erosion and thawing across a landscape might change due to extreme weather events without needing computationally expensive numerical models or extensive field data. The deviations from the 'typical' thaw season presented are meant as further hypotheses to motivate further work and we clarified this in the introduction (L115-122) and discussion (L396-404). Please see the revised text passages below:

L115-122: *"Our work motivates two hypotheses that we explore using the regime space. First, coupled thaw-entrainment results in the formation of discontinuous channel networks over multiple thaw seasons (Fig. 4). Second, the thermal and erosive response of a channel is most sensitive to early season extreme weather events, such as heatwaves and rainstorms (Fig. 5)."*

L396-404: *"Next we compare how different extreme weather events potentially modulate a channel's erosional and thermal evolution over a typical thaw season (Fig. 5b-d). To do this, we consider three events: a cold snap that causes refreezing, a heatwave and a rainstorm. Specifically, we explore how each event changes TFU geometry, mean thaw depth and water velocities, respectively."*

With respect to the question/comment, please see our discussion after the Editors remarks regarding Figure 5 (the subject of this comment). We expanded the referenced section by introducing the variables we are considering, which variable is changed in the calculations of Φ and Γ for each scenario (L396-404, see above), as well as added headings to each thought experiment in the new Figure 5 in order to make it easier to track the explored ideas. The ideas explored are plausible under the changing Arctic climate marked by increased variability (Zhang et al. 2025) and illustrate how the mathematical framework presented can be applied to plausible future scenarios. We have extended the analysis of extreme weather events by taking event duration explicitly into account and are accounting for the effects of varying catchment area in the rainstorm experiment as well. However, this is as far as these ideas can be taken at present. Going beyond this requires more work to explicitly link riverbed erosion in a dynamic sense to carrying climate conditions, and importantly, incorporating the experimental effects we report here at larger spatial scales. This will require numerical modelling or extensive field measurements, which is the focus of our present and ongoing efforts, however outside of the scope for this paper.

Line 351-352 is a great example, but it's unclear to me where this shows up in Figure 4d, Supplementary Figure 7 is more fitting. Also, I may have missed this, but how are air temperatures translated to ground temperatures considering snowpack insulation?

Response: Thank you, we revised Supplementary Figure S6 to show the effect of an early versus late season heatwave on the thaw depth for the remaining season visually. We did not consider snowpack insulation in our thaw depth calculations. As our main objective in these thought experiments was to show the relative differences between scenarios and not recreate field conditions, we used a simple 1D thaw depth model in which we assume that changes in air temperature are equivalent to changes in ground temperature.

L357, "increased climate variability" is vague; please revise.

Response: We reworded it to specify the increased magnitude and intensity of extreme weather events.

Methods: How did particle size and porosity affect experimental results? How does having glass beads vs loess or organic-rich particles common to permafrost settings affect results? Why were glass beads selected? I may have missed this, but were any sensitivity analyses run to address substrate materials?

Response: Particle size and porosity factor into the permeability of the porous medium, modulating the speed of subsurface flow as such changing the strength of heat advection. In addition, porosity changes the solid volume fraction of the bed which appears in the normalisation of both $\mathbb{F}$ terms. The glass beads are also relatively smooth compared to real grains, which further increases the subsurface flow velocity. Combined, this likely means that the advective effects are amplified in our experiments relative to real substrates. However, we ran the experiments with a relatively small Reynolds number meaning that we had transitional surface waterflow instead of fully turbulent, which reduces the frequency of water injections relative to fully turbulent conditions. We take this into account in the regime diagrams shown in Figure 5 by using permeability and porosity values representative of field conditions.
We chose glass beads to simplify the geometry of the problem to better identify effects of ground ice and identifying the key physical processes driving erosion. This is a common experimental approach with the intent to introduce more complex conditions as the focus of present and ongoing work. The glass also made imaging the thaw front easier and tracing subsurface water flows using dye. These simplifications are why we do not present a generalised transport function for a thawing bed here, as this will require further experiments with different substrates and under more flow conditions.

Reviewer #3 (Remarks to the Author):

In "Seasonal freezing increases High Arctic erosion and landscape response to climate extremes" the authors demonstrate that partially frozen riverbeds can experience faster erosion rates than completely thawed riverbeds due to jets of pore flow. These jets were observed in beautifully executed frozen flume experiments that traced undulations in the thaw front formed by flow between glass sediment grains. The authors used experimental results to propose a new quantitative model for how focused subsurface flow increases particle lift forces, locally enhancing erosion. They then plot up their experimental results in a new phase space designed to separate flow and thermal conditions when thaw front undulations will grow or shrink versus when sediment entrainment will be uniform or large but sporadic. This phase space is then used to investigate what would happen under changing climate conditions, revealing that thaw front undulations tend to persist through time and cause streams to remember conditions from earlier thaw seasons and be very sensitive to early summer temperatures.

First, I want to commend the authors on their experiments. Frozen flume experiments are extremely difficult to accomplish, so very few are published, and inevitably involve tradeoffs between scaling hydraulic and thermal nondimensional parameters. These experiments minimize those tradeoffs to produce a novel and surprising dataset. The authors are clearly familiar with existing quantitative methods to scale their results to natural systems, and their novel phase space for spatial variability in thermal versus erosional processes is a concise way to display their results.

While I think the experimental results and interpretations are robust, there are improvements that could be made in the following areas:

1. Connecting the flume experimental results to the field scale. It is not completely clear where these results are applicable. The experiments were conducted under "pick up" conditions, where downstream sediment flux is far below transport capacity. Depending on sediment supply to a given channel reach, this may or may not be the case, particularly for major rivers with large, persistent taliks. Maybe there are other rivers with enough information to plot in Fig 4d? (e.g., McNamara et al., 2008; Scott, 1978; Smith, 1975; Wankiewicz, 1984)

Response: First, thank you for recommending these papers, which in the best-case scenario could provide for additional data to the regime space. However, given that both Φ and Γ depend on TFU geometry, we do not think that they provide enough information to add them to the regime space in a meaningful way. Second, and more to the focus of your comment, we agree that the end goal of this thread of our research is to connect experimental conditions and mathematical representation to field conditions. In follow-up to our 2023 publication identifying relatively rapid and spatially discontinuous channelisation in the High Arctic (Chartrand et al., 2023), we aim to develop a new model for seasonal erodibility. This paper represents the first step in this endeavour by identifying the key physical processes at the grain-scale driving erosion of thawing beds. We have bridged our ideas to field conditions consistent with the data that is available to do so and see important next steps to expand efforts to explore connections with current day High Arctic field conditions. Importantly, the work we present here sets us up well to focus more on applicable field conditions because we understand the field-based measurements that would be most useful to test our ideas directly. Please also see our general response above regarding general "field applicability" following the Editors remarks.

2. Making the results clear with the methods at the end. I found it challenging to get an intuition for where streams should plot in Fig 4d just from the main text.

Response: We decided to focus our discussion on the grain-scale mechanics we observe in the experiments and lay out hypotheses for channelising High Arctic landscapes that we are planning on testing. As explained in our response to your first point, the goal of this work is to highlight the key physical processes at the grain-scale and set us up to be able to connect these processes across scales in future projects.

3. The paper is missing some key references, some of which I list below. In particular, the Roux et al. (2017) experiments on river talik thaw rates should be discussed.

Response: Please see our general response regarding general "field applicability" following the Editors remarks. We added a paragraph to the discussion about talik and bed erodibility (L324-330). Please see the added text below:

"Whereas we only focus on newly developing channel networks here, beds of larger rivers underlain by a perennially thawed layer (talik)[41,42] can freeze during the winter season[42–44], potentially changing the bed erodibility in the early thaw season while this frozen layer thaws by the same mechanisms described here."

With relatively minor edits to more comprehensive frame the experimental results and theory, this paper will be an invaluable contribution to the permafrost literature.

Response: Thank you for your support of our work, and the thorough and constructive review provided. Your comments have helped to clarify and strengthen the presentation of our work/ideas significantly.

Line comments

Line 60: Other permafrost erosion experiments should be referenced as well (Costard et al., 2003; Dupeyrat et al., 2011; Roux et al., 2017).

Response: We added Costard et al. 2003 and Dupeyrat et al. 2011 to the references here. However, as the sentence is specifically talking about erodibility, we don't think Roux et al. 2017 is an appropriate reference to add here.

Lines 163-165: Here, the local variation in topography is approaching 1/2 the flow depth. Do you think this could cause morphologic drag and decrease skin friction, therefore decreasing sediment transport in the unfrozen case?

Response: Thank you for the comment and focus on these lines. Reading the lines again, we believe they were not clearly written. We only observe the largest topographic variations in the frozen experiments, which in the early stages of the experiments can be up to one or two particle diameters in magnitude. In the unfrozen case, we did not observe significant topography develop throughout the length of the flume. What we were trying to say here is that injections of surface water into the subsurface happen as a result of turbulent interaction with the local topographic variation of a rough bed. We avoided the term "bed roughness" in this sentence because roughness is a bed averaged measure, while the deflection is due to the local topographic variation at the injection point specifically. We changed the wording to not specify the size of the initial topographic depressions and moved the size of the topographic variations in the frozen experiments to the start of the next paragraph (L184-209). Please see the revised text passage below:

"However, once bed thawing begins, local topographic depressions deflect flow downward. In an unfrozen bed, the momentum flux supplied by these jets is dissipated throughout the permeable matrix without doing any erosional work. By contrast, the thaw front in the frozen bed is an impermeable boundary that redirects impinging jets and concentrates the supplied mechanical energy within the thawed layer, to do erosive work. At $\psi_{st}/d_p \approx 2$, the thawed layer is sufficiently thick that surface water jets penetrating into the bed create vertical as well as lateral dynamic pressure gradients as the jets decelerate towards and along the thaw front (Fig. 2b,c). Consequent pore pressure gradients generate corresponding upward body forces that reduce cohesion and act against stabilising particle buoyancy forces where pore pressures are relatively high. The net particle flux is therefore enhanced during early time consistent with $q_f \propto 1/(\psi_{st}/d_p)$ (Fig. 1g).

Over time, small topographic variations of up to one or two particle diameters and differences in pore ice distributions localise water injections to form nascent thaw front undulations with amplitude $\Delta\psi \approx 0.5d_p$ (Figs. 2b and 3a)."

Line 138: Please define TFUs here since it's the use of the acronym.

Response: Thank you for catching this, we added a definition here.

Line 322: How do floods affect this phase space? I am curious if the channel and dimensions of TFUs are mostly shaped by infrequent floods or day-to-day conditions.

Response: This is a great question. We begin exploring this in the rain event thought experiment, but to answer this question fully we will require further modelling at the landscape-scale (which we plan to pursue in the next year or so). In the field, we observed deposits of gravels and sands in the oversteepened sections that are fining downstream over approximately 2m (old Figure 4a, now Supplementary Figure 2a), suggesting that the deposits are the result of surface-flow based sediment transport. However, while in the field in August 2024 we witnessed a relatively large rainstorm but did not observe active sediment transport or significant overland flow during it. We think that, in addition to the enhanced erodibility discussed here, overland flow and runoff in developing channel networks is largely constrained to the early thaw season (in general and at the discussed field location) and is marked by overspill events between pools within polygon troughs found along channelizing segments of the landscape. We are working on exploring this numerically for a future project.

Lines 330-334: I found these thought experiments a bit confusing because many processes could alter the temperature of rain after it falls to the ground. For instance, warmer air temperatures could melt more ground ice and cause water near freezing to flow downstream. I would specify exactly which variables you are changing then give a range of scenarios that could cause that variation.

Response: Thank you for seeking clarification here. The intent of these thought experiments is to illustrate the, in our opinion, most likely scenarios that will influence the thermal and erosional evolution of a channel segment within a thaw season and not to be a comprehensive accounting of possible scenarios. We expanded on the discussion to make clear what we varied in each case and added a short title to that extent to each scenario in the new Figure 5. Because the relevant changes are embedded in a longer paragraph, please see the revised manuscript L396-447. We also introduced a new section to the Methods titled "Exploring the effect of extreme weather events on $\Phi - \Gamma$ evolution" (L1041-1169) to explain the thought experiments more thoroughly.

Lines 351-355: This is a very interesting result! But it is not clear to me from just looking at the figure. What specific "landscape response" do you expect?

Response: Thank you, this is another helpful question. We used the term "landscape response" to be concise, but appreciate that this is too vague at times. In general, we used the term landscape response to broadly refer to how an extreme weather event, such as a rainstorm or heatwave, changes the thermal and erosional dynamics of the stream for the rest of the thaw season. We expanded the paper here to be more specific on what we mean (L429-440). We also added this scenario visually to Supplementary Figure 6 and added reference to the figure. Please see the revised text passage below:

"Crucially, for both heatwaves and rainstorms early season events result in the greatest shift in late season channel evolution, suggesting that disturbances in the early thaw season have the greatest impact on the long-term erosion dynamics. For instance, a heatwave with an 8 $^\circ$C increase in air temperature[46] within the first 10 days of the thaw season increases the end of season thaw depth by the same amount as lengthening the average thaw season by 60 days would (Fig. S6)."

Figure 4a-c: I find this figure kind of confusing since it implies that the thaw front undulations parallel the channel bed but there is spatially uniform erosion. Is that the case? It would be helpful to plot the topography and thaw front to show if those undulations are in or out of phase.

Response: We added Supplementary Figure 3 showing the potential thaw front geometry underneath channelised segments based on our thaw depth measurements. The thaw depth measurements suggest that there are larger scale thaw front undulations connecting pools which drive the formation of the oversteepened sections and pools. We extended the discussion and added the additional Figure 4 to the paper to expand on this hypothesis.

Figure 4d: Can landscape memory only go one direction on this plot? Also, it seems like the localized erosion you found in your flume experiments likely doesn't happen in the channel you visited. Which natural channels would plot with phi < 1?

Response: We hypothesise based on the laboratory experiments and field observations that the 'memory' effect from one thaw season to the next is due to the successive growth of surface steps and resulting larger TFUs below, which results in the trajectory shown on Figure 5. However, it is of course possible that other processes that change the surface topography or thawing dynamics across thaw seasons result in a different trajectory.

It is important to note that surface steps can still form while $\Phi > 1$ (see Supplementary Video 1). The buoyancy forces arising from both hydrostatic and dynamic pressure variations act together to enhance erosion and Φ is just a measure of which one is most important.

The 'typical thaw season' trajectory we plot in Figure 5 is based on the relatively small-scale undulations that we measured in the tributary channel segment at the Tallurutit field location and assuming that they are constant in time. As this predicts $\Gamma > 1$ for a significant part of the thaw season prior to our visit, it is possible that the TFUs are larger initially, pushing the channel to $\Phi \sim 1$ or $\Phi < 1$. We did not vary the TFU geometry across a thaw season because we do not have enough field information to constrain this.

Another thaw depth profile that we took between two connected pools with a larger surface step (~0.5 m) suggests that the thaw depth deepens towards the downstream pool. Based on this, we hypothesise that there are larger undulations present with amplitudes on the order of 10s of centimetres and 10s of metres wavelength, which would result in smaller Φ values. For example, a TFU with amplitude of 0.5 m and wavelength of 25 m would result in $\Phi < 1$ for the last month of a 90-day thaw season. Because we were not able to continue the thaw depth measurements into the pooled area we could not confirm this, which is why we initially excluded this from our calculations and the paper.

Line 486: Supercritical flows! Do you expect this to have any affect on the evolution of your thaw front? Is the relation for pore flow you are using appropriate for this case? Presumably the flow goes subcritical when it enters the pore space but then has to go supercritical again when it exits, and I am not sure how that would affect your energy balance calculations.

Response: We do not expect there to be significant differences in thaw front evolution between supercritical and subcritical flow conditions, as the key is the existence of surface water injections which will happen in both cases. At critical flow standing surface water waves can form, which we would expect to result in larger and more stable TFUs as jets would now be stationary in space.

In the regime space calculations for the experiments reported, we used the measured pore water flow velocities. Using a Darcy relationship here would likely not be appropriate as the pore scale Reynolds number is ~35 and Darcy flow is only applicable when $Re \sim 1$ or less. At field conditions, using Darcy flow is likely appropriate as discussed in our Methods (L1006-1017).

Line 489: This is a pretty high critical Shields number, even given the 5% slope. Any idea why?

Response: Thank you for pointing this out, we initially calculated the shear stress using just the water depth as length scale. However, looking back at other experiments with relatively narrow flumes (e.g. Deal et al. 2023) using the hydraulic radius is more appropriate. This provides a critical Shields stress ≈ 0.062 which is closer to expected values. We have corrected this in the manuscript and added the formula for the Shields stress as well to clarify how we calculated it (L592-599). Please see the revised text below:

"Assuming the flows are predominantely hydrostatic, we calculate the shear stress $\tau = \rho_f gRS$, where $g = 9.81\ ms^{-2}$ is the gravitational acceleration, R is the hydraulic radius of the flume, and $S = 0.05$ is the slope. Given that our unfrozen experiments are at threshold, we report a critical Shields number $\tau^*_c = \tau/[(\rho_f - \rho_g)gd_p] \approx 0.062$ at a granular Reynolds number $Re_g \approx 89$."

Lines 729-731: I think psi and delta psi are both defined to be undulation amplitude here… is that correct?

Response: Thank you for catching this, that is a typo. We corrected it to be delta psi.

Lines 745-748: How do these values compare with heat produced by turbulent dissipation? I don't expect that to change your results, but it would be good to include in your order-of-magnitude calculations.

Response: Since we do not have measurements on the small-scale turbulent structures in our experiments that would be necessary to account for turbulent dissipation at the bed surface, we have neglected it here. However, turbulent dissipation is unlikely to affect our results. Heat generated that way at the bed surface will be split between the porous medium and the water in the bed (plus some loss to the surface water flowing above). As such, it will either be conducted downwards to the thaw front over a diffusive timescale or it will be advected by a jet over the advective timescale, replicating the same balance that we already capture in Γ.

Line 872: How did you pick a Manning's n? Please present field data or cite a reference (e.g., Arcement & Schneider, 1989).

Response: Added a citation.

Lines 897-898: I don' t see the grey ellipse on Figure 4d.

Response: We removed this reference.

Extended Data Fig 1: V_w should be 25 cm/s based on main text.

Response: Corrected it in the figure.

Extended Data Fig 3: Please define all variables in the caption.

Response: Added the definition to the caption.

Extended Data Fig 6c: What do the dashed versus solid red and blue lines mean?

Response: The gaps in the blue line between 100 and 800 s is because the interpolation and finding of the 0 degree isotherm failed at times due to issues with the sensors. We decided to not fill in the missing data to show that there are gaps using this method. We added a note to the caption to clarify.

Extended Data Fig 7: Please label plot axes.

Response: We folded this figure into the new Figure 5 with labels.

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

TThank you for the opportunity to review this revised manuscript. The authors have done well to comprehensively address my previous concerns and comments on the manuscript, and I sincerely thank them for their efforts in that regard. The manuscript is, in my opinion, now acceptable for publication. I would recommend an editor's highlight for this paper for its broad interests to the community of hydrology/geomorphology and water resources stakeholders.

Thank you for your kind words and efforts on reviewing our work.

Reviewer #2 (Remarks to the Author):

Eschenfelder et al. experimental design suggests that periglacial fluvial erosion is sensitive to early-season weather events, and cold-region landscapes may respond more rapidly to climate change than their temperate counterparts. The authors have thoroughly responded to reviews, and I appreciate their detailed response to my prior comments. I especially found the revised Figure 4 and expanded discussion that includes how different weather events modulate a channel's erosional and thermal evolution helpful for contextualizing this experimental study. The only minor comment I noticed in my re-read, which was focused on the revised text, was that the 'p' in pressure in the new Figure 4a is cut off. It is also unclear what the top wavy blue line is in Figure 4a (I assume the surface water boundary?), and suggest changing it and the color of its associated text. The results of this study are of clear interest to permafrost and geomorphology researchers and contribute novel insights to the literature.

Thank you for your efforts on reviewing our work and catching the mistake in the figure.
We moved the labels around and changed the surface water boundary and associated text to a lighter blue to distinguish it from the thaw front. Based on this comment we also closely checked the other figures and made small adjustments to other label locations.

Reviewer #3 (Remarks to the Author):

The authors did a nice job in their revisions, and the addition of field data in the Discussion section and splitting Figure 4 into Figures 4 and 5 made it much clearer how the experiments relate to field observations.

My only request is that the authors add a sentence in the Discussion or Methods explaining how differences in Fr and Re between the experiments and field should not significantly change the resulting TFUs. The authors clearly responded to my previous comment on Fr>1 and rapid pore flow velocities (referencing original submission line 486), but did not make any revisions to the manuscript. I think the Fr and Re scaling is important to mention in the paper since supercritical flows can form antidunes and transitional flows have lower sediment transport capacity than fully turbulent ones. I am happy for the editor to administer this minor change and/or discuss with the authors then send the paper directly for publication.

Thank you for your efforts on reviewing our work.
We added a discussion of the Froude number in the Methods (L 485-498).