Dear Reviewer #2,

we want to thank you for your efforts to improve our manuscript with your comments. Below you will find detailed answers. You raised some points that were also raised by RC1 and your might be interested in our answers to RC1 as well.

Many thanks again,

best wishes,

Angelika and all co-authors

RC2: 'Comment on egusphere-2024-1151', Anonymous Referee #2, 05 Sep 2024

Humbert et al. present a detailed study into the long-term drainage history at a previously unreported lake at N79 glacier in northeast Greenland. The study integrates a wide-ranging array of highly technical methods with a high degree of competence. The paper is dense and technical, and clearly the work that went towards it is commendable. The two most novel parts of the study, in my opinion, are: (i) the observation of a system than, within the satellite observational record, developed from no lake at all into a lake that displays repeated rapid drainage to the bed; and (ii) the first (as far as I am aware) radar observations of the annual fracture history caused by repeated lake drainage.

My comments on the paper largely align with that of Reviewer #1. I feel that my understanding of the paper was limited by some strange terminology choices and a dense and confusing structure, which has made me feel as though I might be missing the main thrust of the story. I hope that this can be fixed largely within the terminology and structure, rather than implying any deficiencies in the work itself.

General Comments

It is a key part of the study, but I am still unclear as to what the authors mean when they refer to ‘gullys’, beyond the general dictionary description of a water-incised channel. I initially read the paper following my interpretation of the dictionary definition, whereby the ‘gullies’ are the three drainage channels extending E-NE from the three ‘branches’ of the lake (e.g. Fig. 1a), and ‘drainage through gullies’ would refer to the lake overtopping and draining due to the incision of these channels (as described in Tedesco *et al.* 2011, doi:10.1088/1748-9326/8/3/034007). However, as I read the manuscript, it became increasingly clear that ‘gullies’ appeared to refer to point features that might be analogous to moulins. Most obviously this includes: Figure 3b (as triangles); L236 (“a hole… is the first feature we identify as a gully”); L254 (“a gully [can be] inferred due to ponding … which has a form consistent with a drainage pathway”); and Figure 7 (the ‘gully’ here seems explicitly to be the point depression). Gullies are also frequently referred to as ‘triangular’, although it is not clear in what context (plane? profile?).

We think, Figure 7 does indeed show a triangular shaped feature and all panels show either satellite imagery or airborne data and hence a top view of the horizontal plane. However, if this did not become clear, our text lacks a proper introduction of that, which we will include in the revised version.

To me, if these point features are what the authors are referring to, I cannot see why they are not referred to as moulins. The authors take time to explicitly seem to reject this in paragraph beginning L75 as ‘are neither formed by melting nor round in shape’. I have never encountered the necessity that a moulin must be formed by melting, nor strictly round (beyond the fact they can be generally modelled as point rather than linear inputs to the en/subglacial system). Post-drainage surface-to-bed (or, at least, surface-to-englacial-environment) connections are commonly called a moulin in the drainage literature. Looking at the drainage pathways visualised in the study (Fig 5a; 7; 8c, 8d), I cannot see how they are different from those referred to as ‘moulins’ in previous drainage studies, which also show rivers terminating into thermomechanically-maintained holes along the relict fracture (Das *et al.* 2008, Fig. 1 inset, doi:10.1126/science.1153360; Doyle *et al.* 2012, Fig. 4a, doi:10.5194/tc-7-129-2013; Chudley *et al.* 2019, Fig. 5e, doi:10.1073/pnas.1913685116).

We have included a more general discussion on gully versus moulin terminology in our answer to RC1, so we want to focus here on the last parts of this comment:

In the paper of Das et al. (2008) it is not easy to see clearly the fracture geometry, but what is clear from that figure is that the shape is not triangular and by far lager in size than what we denoted gully. But the Fig1 inset there and the Doyle-Fig4 and Chudley-Fig5 are significantly different. The Das-feature is neither triangular nor arch-shaped as the Doyle and Chudley features. We found in numerous instances at other lakes fetures similar to Doyle’s and Chudley’s.

If I am right that the gullies are the point features, then - to me - these drainage events bear a close resemblance to previous events: (i) background fractures exist oriented according to the principal stress (L337); (ii) which, aided by transient flow acceleration (L454-455), the water can then exploit (or reactivate) via hydrofracture (as in Christoffersen *et al.* 2018, doi:10.1038/s41467-018-03420-8). The authors say that hydrofracture isn’t occurring at L78-79, although I can’t see any evidence to reject Occam’s razor, especially as no other mechanism is proposed for the ‘propagation’ referred to at L343-346); (iii) this fracture opening results in drainage to the bed (or, at least, englacial environment); (iv) full-depth fracture will close elastically along most of the length (L426-427); (v) apart from where rivers intersect the fractures (L334) and surface-to-bed connections (moulins) can be maintained through thermo-mechanical erosion of the vertical drainage pathway. If this is the case, then ‘gully refilling’ may be similar the relict moulin refilling previously noted by Chudley *et al.* (2019) in the literature (cf. Fig 11 of the manuscript with, e.g. Fig 5c insets in Chudley *et al.*), although they attribute this overtopping to the ‘top-down’ filling of a closed moulin rather than ‘bottom-up’ water rising from the bed in this paper.

When we stated that the fractures that lead to drainage are not formed by hydrofracture, we intended to explain, that the overall stress situation of this area is leading to initiation of new fractures. The background fracture are shallow and narrow. But the stresses governed by bedrock topography changes are in our perspective the ones who are governing the location of fracture formation.

I do not think that similarity to previous studies is a bad thing: instead, the choice to choose different (and poorly explained) terminology for relatively ambiguous reasons is confusing when trying to place this study into the context of other work (at least, it is for me!). If I have been misled by my interpretation, then perhaps the authors might need to take more time to more carefully explain the new terminology, and how and why this lake drains differently from previous studies.

We have elaborated on our view of this choice of terminology at below and in the answer to the other reviewer in detail and kindly like to ask to consider those answers, too.

Specific Comments

[Abstract] Include ‘79˚N Glacier’ as alternative name within abstract if there is space?

Yes, this shall fit and is certainly useful for the reader.

[Introduction] I found it surprising how dense some of the introductory material is, covering quite technical aspects of flow dynamics, englacial hydrology, and fracture mechanics that are not touched upon again in the rest of the paper. Some work could be done to remove unnecessary content here. Perhaps this relates to Reviewer 1’s comments about a lack of clear story - a more cohesive set of key findings would in turn help to identify the necessary material for the introduction.

Many thanks for this feedback. Yes, we agree, this could definitely be shortened and more streamlined to clarify the story. We will work on this intensively for the revised version.

[L24-27] Is this level of detail necessary for the introduction? Could just say that AF is well-established to have occurred in this region (citing Khan, Humbert, Zeising).

We certainly can reduce the four lines to three lines if that is helpful.

[L27] ‘In this study’ - perhaps a misrepresentation of what this study is doing? It is perhaps not surprising to find how rising surface temperatures could lead to a meltwater lake.

We agree that this sentence was missleading. We rephrased it to: “In this study, we will show that the formation of a massive meltwater lake coincides with changes in surface temperatures.“

[L29-35] Given the well-established lake literature (and how none of this really comes up in the rest of the paper?), this is a lot of words to say that lakes occur where surface melt collects in topographic depressions.

Line 29-35 discuss how runoff occurs and how local melt can straight reach the bed. In these six lines, lakes are not the topic.

[L75-78] I am quite confused as to why this text is located here, in between to other completely standard final-paragraph-of-the-introduction prose.

We intended to reiterate the terminology, but we will revise this text.

[L85-92] I do not think this section needs to be three separate sentence-long paragraphs. This applies elsewhere as well - the abrupt appearance of short interspersing pargraphs gives the impression the sentences/paragraphs have been cut-and-pasted together without much regards for coherent structure or flow.

We apologise and will edit the text accordingly. It was not meant as individual paragraphs.

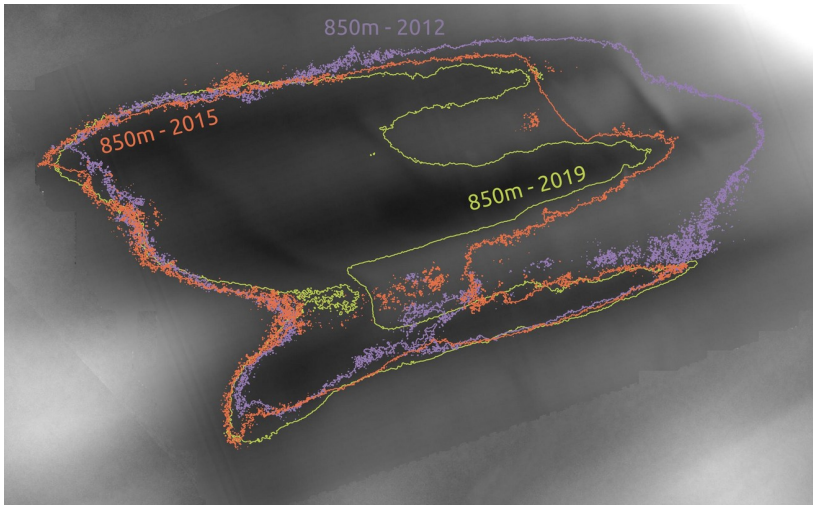
[L94] Perhaps ‘single-polarization (hereafter single-pol)’ at the first instance.

Many thanks for this suggestions! We will change the text accordingly.

[L103] Was there significant difference in the relative basins (barring vertical difference due to melt, presumably) between the three DEMs? This would be of interest as, if the three

DEMs gave broadly similar results, it would mean that long-term lake volume studies could be produced from confidence using only one good DEM of the empty lake basin (from e.g. the ArcticDEM mosaic product)

Thank you for raising this point as indeed this would be nice for global studies on supraglacial lake volumes – but unfortunataly the shape of the basin changed quite a bit between the three time stamps. To visualize this we show the 850m contour line derived from the three different DEMs. As evident from this image using a single DEM would also have a large impact on the derived lake volumes.



[Section 2] Could this be further separated into 2.1 Satellite Methods, 2.2 Airborne Methods, and 2.3 Modelling for clarity, with current subsections as subsubsections?

Yes, this is a very good idea and we will include this in the revised version.

[Table 1] could be moved to a/the supplement for brevity

We recognised that R1 even wants this table to be extended with even more information and we found within the team of co-authors that it is useful to flip back- and forth between the figures/text and this table. Our suggestion is to leave it with slightly more information (resolution) in the main part of the manuscript.

[L211-217] could be methods?

This is indeed a great suggestion that we will incorporate in the revised version.

[L223-4] Once again, is this really its own paragraph? I note at this point in the results there start to be single-spaced line breaks and double-spaced line breaks. This is again confusing - what are paragraphs and what aren't?

We intended to separate the years as paragraphs, but in that particular year, little information could be gathered, so it is extraordinary short. But you are definitely right and we will rearrange this in the revised version.

[P13] Could a timeseries plot be a useful way of visualising this narrative? Noting e.g. maximum extents per year, with drainages marked.

This was also raised by RC1 and we fully agree that this makes a lot of sense. We are working on a draft version of such a graphic, also in order to replace the table.

[L231] Here and elsewhere, satellite imagery used to build the narrative is not shown. Perhaps as supplementary material, before/after satellite images of the drainages could be visualised?

This is definitely an option. We felt like the manuscript is already overloaded with imagery, but putting it into supplementary material is a very good idea. We will prepare this for the revised version.

[L253-257]. This is the first point in the text that locations A and B are referenced, and after finishing the manuscript I am still unclear as to exactly why the specific locations are important or what is happening in them that is special. Is it where the ‘gullies’ (moulins?) are located? Are they the same ones being reactivated and advecting along? This needs to be much more clearly explained. Also, it is confusing to track them between figures - can they be marked on all of them?

This is 46 lines into the results section and is in the section where the time period is discussed in which it can first been narrowed down to such a location. It has also been discussed and shown in Fig3b what why we mark this. We will go through the text again and check carefully where we can add more text to make it easier for the reader to follow.

[L258] ‘Gully A’ referenced here and, as far as I can tell, never again.

Indeed – this is a relict and we have changed that for the revised version.

[L293]. and elsewhere - present tense rather than past tense.

We are going to check this throughout the revised version. Many thanks for pointing this out.

[Section beginning L360] I agree with Reviewer 1 that the aerial data is remarkably lightly analysed considering - as I identify in my opening paragraph - it is truly spectacular and unique data with a wealth of opportunity within.

We fully understand the wish of the reviewers to apply more analysis on the radar data. We are exploring ways to enhance our analysis and present this in a useful way for the reader in the revised version. We will also include a discussion of the limitations of the radar surveying and its interpretation.

[Paragraph beginning L430] Other studies that discuss the elastic opening/closure of full-depth crack closure/opening suggest it must be dependent on continued flow of water to remain open, and closes rapidly after the main drainage event is complete (e.g. Doyle et al, Chudley et al, Stevens et al). However, here it is implied that, with just the hydraulic head alone, (i) full-depth cracks can remain open; and (ii) water does not freeze (cf. e.g. Hubbard et al 2021). My instinct here is that this is quite unrealistic without further evidence?

The simulation results in Figure 16 show that after removing the water pressure from the boundaries of the cylinder, the radial displacement of the boundary still increases, with a positive mean value. This can be attributed to the different boundary conditions at the outer boundaries of the simulation domain. On the left side, we applied the upstream velocity of 3.8 m/d and on the right side the downstream velocity of 3.9 m/d. Both velocities are in accordance with remote sensing observations, as stated in L203. Because of the velocity difference, the simulation domain not only faces a rigid body motion, but an elongation, which appears to be a driving force for the opening of the channel in this model. We will incorporate this in the revised version.

[L433 - 441], and potentially elsewhere: does 'head' refer to the hydraulic head?

Yes, indeed head refers to hydraulic head. We will use consistently the term hydraulic head in the revised version.

[L456-457]- Post-drainage vertical displacement along the crack face was also reported by Doyle *et al.* (2013)

Many thanks for raising this. We will incorporate this into our revised version!

[L455-465] I agree that it is likely the 2005/06 event is likely the earliest event (within the satellite observational record), but surely the radargram data merely confirms that no drainage occurred from the date of the farthest-downstream-observation of the radargram through to 2012?

We assume that the reviewer refers to the last two sentences in this paragraph. Indeed the radargram is not the strongest argument. We flew in our airborne surveys along a centrally located flow line and the farthest downstream feature fits perfectly well with flow speed and time between the airborne survey and the drainage even. However, we might have just missed a feature that is slightly offset to this line. We will remove the last two sentences.

[Figure 3] maximum lake extent appears to be getting smaller through time? Is this significant?

Indeed this is significant. We shall incoporate this in the results discussion section in the revised version.

[Figure 3b] do the triangles represent the current (advected) locations of the gulleys, or the contemporary drainage locations? Triangle directions aren't explain within caption and require reference to main text.

The triangles represent the location of gullies at the time given in the legend.

[Fig 8a] The shades of the principal (stress? strain?) crosses are very hard to differentiate easily. Perhaps colour-blind-friendly colour contrast would be better here?

Many thanks for raising this point. As one co-author is colour-blind, we were able to test the suitability of the color choice directly and the result is that the diffence in shades of the crosses is visible.

[Figure 14] explicitly label the panels "3-4/7-8 weeks after drainage" as well as the interferogram dates for clarity?

This is a very good suggestion that we will incorporate in the revised version.

Citation: https://doi.org/10.5194/egusphere-2024-1151-RC2