Dear Reviewer #1,

we want to thank you for your very helpful comments on our manuscript, that we have addressed in the detailed answer below. We are looking forward to your comments on the revised version of the manuscript.

Best wishes,
Angelika and all co-authors

RC1: 'Comment on egusphere-2024-1151', Anonymous Referee #1, 04 Sep 2024

The manuscript, 'Supraglacial lake drainage through gullies and fractures' by Humbert et al. seeks to characterize the ice surface and englacial behavior associated with repeated supraglacial lake drainage on Nioghalvfjerdsbræ Glacier using a wide range of in situ, satellite and airborne observations.

Overall, I found the individual observations interesting – particularly the radargrams. However, I believe that there are several fundamental issues with the manuscript. My major concerns are detailed below followed by line comments.

1. The manuscript is missing a story. Many datasets are presented, but by the end, I am still unclear about the manuscript's main conclusions. Those indicated in the final section of the paper aren't well supported by the analysis presented.

   This point was also raised by the second reviewer and we take it seriously. We have got stuck in so many details of the data, that we lost in the text the focus of the story. We will follow your suggestion as well as the one from RC2 and reduce the introduction to englacial features in Greenland, elaborate on the findings of other studies and than introduce our approach.

   We do think that our conclusions are very well supported by our analysis, but we admit, that we failed in presenting this in a proper manner. This will be improved in the revised manuscript.

2. Why not term these moulins? Particularly after they start receiving persistent meltwater and reactivate over time? I understand the argument that these structures aren't cylindrical (at least at the surface) but drainage to the bed along cracks is certainly a moulin. The imagery presented here are very similar (at least in my mind) to figures in Hoffman et al. (2018), Chudley et al. (2019), and Doyle et al (2013). Yes, the englacial geometry is complex, but that is not unexpected (e.g. Covington et al. 2020). Plus, I'll note that even Das et al. (2008) refer to established surface-to-bed flow as 'moulin flow'. These features are moulins and calling them something else complicates the story, particularly when modeling the deformation of a circle at the surface.

   We understand that the term gully is not very welcome. The terminology of hydrological features of ice sheets has evolved over time, not only with respect to moulins, but also other features. We suggested, that it is time to move away from the term moulin for any kind of drainage pathways and if one wants to keep a text consistent, moulin fracture is leading to substantial longer text. And indeed the englacial geometry is expected to be complex, but is it governed by melt features, like scallop shaped surfaces of moulins walls? We do not expect this to be the case. If water flows through moulins that are formed by melt features 'moulin flow' makes a lot of sense. But does the same term makes sense, if fractures of km's length open, water drains through in short time (e.g. Chudley et al. (2019), and almost closes again? The englacial features in both cases are of different nature. So, why not using a distinct terminology for both to make it more clear?

We want to give some background on why we have chosen this distinct terminology: gullies in streets are pathways for rain water into the underground sewage system. Often their cover acts as a mesh and after torrential rain, dirt, branches etc are filtered, staying on the surface while the water drains into the sewage system. Similar characteristics is shown in Fig. 5 c with ice blocks not drainaing through the conduit. When the sewage system is itself flooded, the water level raises and water is rising onto the street. Similar to what we found and visualied in Fig. 11. Lastly, the manhole cover is displaced when water raises from the sewage system and is lying close to the manhole, similar to the blocks we found. These are a number of similarities.

What is also clear to us: if both reviewers are rejecting the terminology so clearly, there is no chance that the community will use it. Which is not useful at all. Therefore, we surrender and will rephrase the text accordinly to moulins.

3. The analysis of the englacial features is limited to the visual inspection of a series of radargrams. It would be preferable to more carefully analyze these unique data and explore how the results link to surface behavior of both the supraglacial lake and crevasses/cracks.

   We do see your point and we will try our best, to give more details in this regard. But we want to stress, that the radargrams are acquired not straight after a drainage event. None of the airborne campaigns 2016, 2018, 2021, recorded the englacial features as they were formed, but at a later stage of existence. Our airborne flightlines were also not so densly spaced, that we could aim for a 3D reconstruction of 'the real' structure. This is something we anticipate for the future.

   For the revised version, we will enhance the discussion of the radargrams and we will also include a discussion of the limitations this surveying is objected to.

4. Not all the methods are described in the Methods section. The manuscript references GNSS derived ice velocities, inverse modeling, methods for displacement along a crack and subglacial modeling. In these cases, the methods are not clearly described. In other cases, where the method is described, there are missing references to the tools or software used.

   We will extend the method section in the revised version and include for each method an own section. We will also make sure tht references to tools and software is included.

5. There is an overall lack of referencing, particularly in the discussion. There has been a reasonable amount of recent work looking at the mechanisms, geometry and impact of moulins on the subglacial hydrologic system. I have included some of these references below, but there are a number of others.

   Many thanks for supporting us with the literature! About one third of the suggested papers were indeed cited in our manuscript and we are very happy to include more in the revised version.

6. The manuscript would benefit from a through proof reading, including grammar, punctuation and abbreviations.

Many thanks for pointing this out. We will work on grammar, punctuation and consistent use of abbreviations in the revised version.

L3 The first sentence is missing something.

Indeed, this is correct. We have completed the sentence now.

L30 The literature indicates that additional meltwater can both act to accelerate AND slow basal sliding depending on the structure and evolution of the subglacial hydrologic system. It would be worth clarifying that here.

This is a very good suggestion! We will cite the following paper in the revised version and add a sentence to clarify this.

Moon, T., I. Joughin, B. Smith, M. R. van den Broeke, W. J. van de Berg, B. Noël, and M. Usher (2014), Distinct patterns of seasonal Greenland glacier velocity, Geophys. Res. Lett., 41, 7209–7216, doi:10.1002/2014GL061836.

L38 How does lake overflow differ from feeding downstream lakes and streams?

We wrote 'Supraglacial lakes may either drain straight into moulins, or partially by overflow, or by feeding lower elevation lakes via rivers and streams.' not 'feeding lakes and streams' . Maybe there is a misunderstanding? We will reformulate this sentence and include here also two of the suggested papers below.

L40 Consider revising this section. I think the general mechanism of lake drainage is reasonably understood – in compressional lake basins, there needs to be a precursor tensile event (e.g. Hoffman et al., 2018; Stevens et al., 2015; Christofferson et al., 2018). What causes the tensile event is still up in the air and can vary from place to place and because these events are difficult to observe (Poinar and Andrews, 2019) there are outstanding questions.

We disagree with this comment. As long as the cause of the tensile event is not clear, the drainage mechanism is not understood. And also the cause of the triangular shaped features in our case or more arch-shaped features in Chudley et al. (2019) is not clear at all.

L45 "In Greenland, there are…"

Done.

L45 Gulley et al. (2009) provides a nice review of the mechanisms described in this paragraph and it would be beneficial to include some of the information included there.

This is a very good suggestion and we will follow this in the revised version.

L55 The primary difference isn't just scale. The current understanding is that most of the englacial structure in Greenland is formed via hydrofracture. Other mechanisms like cut and closure struggle due to ice temperatures and overburden pressures.

Not so in crevasse fields.

L85 Include a table of the satellite, image names, dates collected and resolution. Table 1 is close but doesn't have all the information to be reproducible.

To include the resolution in the tabel is a phantastic idea! We will do that for the revised version. As we literally checked each existing Landsat and Sentinel-2 image, there is no more information drawn from listing the dates of all of those. The other review suggested to turn this table into a figure. We will draft a figure and check, if that is better digestable than a table with even increasing information content.

L165 So, reading the CARRA documentation, it looks like skin temperature is “Average air temperature at the surface of each grid column.” Which is different than the temperature of the uppermost surface layer – which shouldn’t respond instantaneously to surface fluxes. Some clarity here would be beneficial.

Following the CARRA documentation (Parameter Database), the skin temperature is “the temperature of the surface of the Earth. The skin temperature is the theoretical temperature that is required to satisfy the surface energy balance. It represents the temperature of the uppermost surface layer, which has no heat capacity and so can respond instantaneously to changes in surface fluxes.“

L170 It would be beneficial to have a cross-sectional diagram as well as Figure 2 or clarity that the model is only for surface deformation. Also, this modeling framework is in direct contradiction to the argument in the introduction about not being circular, thus not being a moulin.

In L70 the modelling part is introduced as a case study for a drainage channel at the surface of a glacier. In L170 the model is described as a two-dimensional top view on the englacial channel, with a visualization of the model in Figure 2. However, the phrase “evolution of the englacial channel” may be to abstract and we will work on a more specific formulation and provide more details on the assumptions of the numerical model. In the revised version, we will clarify our aim to simulate the radial displacement of the channel on the top surface.

L213 Table 2 would be better as a figure!

This is a very good idea and we will consier including this into Fig. 3!

Further, how is the ‘begin filling’ and ‘filling complete’ dates determined? It seems that these would be difficult to determine and the ‘filling complete’ would just be the date that drainage started.

Filling complete has been determined from stagnant area of the lake.

L255 Inferring that the drainage paths were shut should be in the discussion, not the results.

This is a very good suggestion, which we will follow for the revised version!

L241 This is the first mention of ice-based GNSS position measurements. These measurements and the processing to velocities should be described before this section. 10% variation seems small when looking for the addition of meltwater to the bed. Also, the inference that there is meltwater at the bed should go in the discussion.

We added in the method section a subsection were we introduce the ground based GNSS data.

“To measure horizontal and vertical displacement of the glacier surface through time two dual-frequency NovAtel GNSS receivers were deployed in the 2017 field season. The receivers collected data for roughly two years with some data gaps during winter time. The flow trajectory was computed using the precise point positioning (PPP) post-processing option, which included precise clocks and ephemerides of the commercial GNSS software package Waypoint 8.90. The accuracy of the post-processed trajectory is better than 0.02\,m. Based on the trajectories we were able to estimate temporal variations in the ice flow velocity and vertical displacements after lake drainage events."

We agree hat the inference should be moved to the discussion and will do so in the revised version.

L255 This figure reference is out of order.

Yes, indeed the reference to the figure is incorrect. Many thanks for pointing this out!

L268 (& L289). How are ice block widths measured? Using 10-m Sentinel-2 imagery would not permit widths less than 10 m.

This has been measured in the WorldView imagery.

L274 If the 2020 'gulley' was still active, drainage did take place. Do you mean to indicate that no 'rapid' drainage took place?

Yes, indeed this is what we wanted to express. We are rephrasing this in the revised version to make that clear.

L298 These sentences are interpretation best left for the discussion.

We agree and shift this into the discussion section in the revised version.

L317 What is meant by 'shade'? Shading?

Yes, the dark shadowed area of an elevated obstacle. We will rephase this to shading.

L336 What inverse modeling? Such methods should be described in Materials and Methods. What velocity fields are used? This choice would drastically affect the derived stress fields.

This is a very good suggestion. We wll include this in the revised version as an own method section, give information about the velocity fields used. As a comment, the velocity field has not changed substantially in this area since 2008/09. So it will have at most a minor effect on the principle stresses used here. In other regions around Greenland, also in the close neighbourhood this will be very different.

L345 without more details about the inverse methods and associated choices, this statement is speculative.

As stated just above, we will definitely include more information in the revised version of the manuscript.

L347 Is there other evidence that Figure 10 actually shows uplift? In this case, I would expect one edge to be sharper. What is the sun angle and orientation?

Which edge should be sharper? One needs to keep in mind, that the surface of the ice before the fracture appeared inhibited some roughness, too. Therefore, we do not understand where the sharp edge should come from.

In the revised version the information on sun angle and orientation will be given.

L419 The modeling results need further description and justification. Is this meant to be a vertical cylinder in the ice? horizontal? Why only run the model for 20 days when the time between lake drainages can be several years? The figure seems to show results at or near the surface because there would be substantial creep closure at depth, but it is unclear to me if the englacial conduit is modeled as water filled or air-filled.

This corresponds well with your comment on L170. We are going to work on a clarification for the geometrical setting, as we try to simulate the surface deformation of a vertical cylinder. We simulated the deformation of the channel within 20 days since the modelling framework holds only for small deformation. However, the tendency to open is still predictable with small deformations during the immediate drainage event. Therefore, we chose to model the channel with 10 days water-filled and 10 days air-filled. Clarifying sentences will be added in the revised manuscript.

L430-435. This paragraph is not well justified and the reference an undefined subglacial model needs further description. I think it is conceivable that the water table within the conduits is identifiable in Figure 12a-b, but additional, careful justification is needed. Subglacial models are notoriously poor at capturing observed subglacial pressures, particularly if they do not include point supraglacial inputs and there is no modeling to support that an englacial conduit could remain open in the absence of supraglacial water inputs. I will note that Figure 12c could indicate a water filled moulins, which can be quite complex (e.g. Covington et al., 2020).

We did not intend to use this information to draw conclusions on the water pressure during the drainage events and we fully agree with the reviewer that our model does not capture the water pressure correctly during the drainage or shortly after. We intendend to use this as a plausibility check. And for this purpose, we find it interesting and useful information. But in order not to confuse readers, we will leave that out in the revised version.

L437-441 This paragraph is unclear. Consider rewriting.

We agree and will rephrase the entire paragraph in the revised version.

L442-444 The reflections in Figure 12 would be due to the difference between ice and air or ice and water – it's unclear how these reflectors can provide information about whether the moulins (or englacial conduits) have experienced any creep closure or thermally driven opening.

We do not say anything about creep closure or the reflection coefficient air/water versus ice/water. We simple compare the position of identified features over time. And this clearly shows displacement is not depending on depth.

L446 This behavior is consistent with moulin behavior – moulin reoccupation is common and dictated by surface gradients and moulin advection. However, what evidence justifies the statement of reoccupation? Figure 12? Why?

The text in L446 states that new gully features are created at the same locations (denoted A and B). Once formed they are advected, but this sentence just explains that they are created repeatedly at the same locations. Figure 3b justifies this.

L460 Or it could be that the moulins/gullies closed at depth between drainages and needed to fill in order to hydrofracture and reactivate.

Fig 11 shows the time scale at which this refilling is happening. We find it extremely unlikely that the closure happens on such a short time scale and that sufficient water supply is available (no water around the gully location in Fig. 11) to fill a crevasse in this form.

L466 The skin temperatures display a clear seasonality from 1991 (Figure 15). What exactly is meant here?

We meant seasonality with respect to formation of lakes, drainage and blister formation and the respective influence on the subglacial system. We will clarify this in the revised version.

L473 The statement that the englacial channel is due to increased surface temperatures is not justified here. Perhaps there is more frequent lake drainage due to higher meltwater production.

Correct, there is a sentence missing in the discussion that connects the first drainage with atmospheric forcing.

Figure 3. In panel b, what do the different orientation of the triangles mean? This isn't described.

There is a legend included in the Figure which clearly defines each symbol

Figure 4. What is the color scale in panel a?

Panel a shows a Pauli decomposition of a quad-pol image, which is a standard in satellite polarimetry. This question is similar to what is the color scale of an optical satellite image with RGB channels.

Figure 5. Having the panels and regions be the same letters is confusing.

We cannot really see why this should be confusing. The regions are denoted with A, B and the panels with (a), (b) etc.

Figure 7. No panel letters. The spatial resolution and orientation of panel c? is different. Why?

We will include panel letters in the revised verson. The orientaton is the same. We have zoomed into the area, to allow the reader to see the elevation in the triangluar-shaped area better. But we are happy to keep all figures the same scale.

Figure 12. What are the F's, T's and W's? They aren't referenced in the caption or text.

In the figure caption we added some more information:

“We identified englacial features (labelled as F1 to F5), which reoccur in all three panels (F3) or newly develop (F4, F5). A persistent wavelike feature is labelled as W1 and a triplet of features close to the ice bed are labelled as T1.”

Figures 12 and 13. Can the flight lines be added to a map?

The flightlines are shown in Fig 1 and the caption of Fig 12 and 13 describe that they are shown in Fig. 1.

References

Andrews, L. C., Poinar, K. and Trunz, C.: Controls on Greenland moulin geometry and evolution from the Moulin Shape model, The Cryosphere, 16(6), 2421–2448, doi:10.5194/tc-16-2421-2022, 2022.

Christoffersen, P., Bougamont, M., Hubbard, A., Doyle, S. H., Grigsby, S. and Pettersson, R.: Cascading lake drainage on the Greenland Ice Sheet triggered by tensile shock and fracture, Nature Communications, 9(1), 1064, doi:10.1038/s41467-018-03420-8, 2018.

Chudley, T. R., Christoffersen, P., Doyle, S. H., Bougamont, M., Schoonman, C. M., Hubbard, B. and James, M. R.: Supraglacial lake drainage at a fast-flowing Greenlandic outlet glacier, PNAS, 116(51), 25468–25477, doi:10.1073/pnas.1913685116, 2019.

This paper has been cited and discussed

Covington, M. D., Gulley, J. D., Trunz, C., Mejia, J. and Gadd, W.: Moulin Volumes Regulate Subglacial Water Pressure on the Greenland Ice Sheet, Geophysical Research Letters, 47(20), e2020GL088901, doi:https://doi.org/10.1029/2020GL088901, 2020.

Das, S. B., Joughin, I., Behn, M. D., Howat, I. M., King, M. A., Lizarralde, D. and Bhatia, M. P.: Fracture Propagation to the base of the Greenland Ice Sheet during supraglacial lake drainage, Science, 320(5877), 778–781, doi:10.1126/science.1153360, 2008.

This paper has been cited and discussed

Doyle, S. H., Hubbard, A. L., Dow, C. F., Jones, G. A., Fitzpatrick, A., Gusmeroli, A., Kulessa, B., Lindback, K., Pettersson, R. and Box, J. E.: Ice tectonic deformation during the rapid in situ drainage of a supraglacial lake on the Greenland Ice Sheet, The Cryosphere, 7(1), 129–140, 2013.

This paper has been cited and discussed

Gulley, J. D., Benn, D. I., Screaton, E. and Martin, J.: Mechanisms of englacial conduit formation and their implications for subglacial recharge, Quaternary Science Reviews, 28(19–20), 1984–1999, doi:10.1016/j.quascirev.2009.04.002, 2009.

Hoffman, M. J., Perego, M., Andrews, L. C., Price, S. F., Neumann, T. A., Johnson, J. V., Catania, G. and Lüthi, M. P.: Widespread Moulin Formation During Supraglacial Lake Drainages in Greenland, Geophysical Research Letters, doi:10.1002/2017GL075659, 2018.

Poinar, K. and Andrews, L. C.: Challenges in predicting Greenland supraglacial lake drainages at the regional scale, The Cryosphere, 15(3), 1455–1483, doi:10.5194/tc-15-1455-2021, 2021.

Stevens, L. A., Behn, M. D., McGuire, J. J., Das, S. B., Joughin, I., Herring, T., Shean, D. E. and King, M. A.: Greenland supraglacial lake drainages triggered by hydrologically induced basal slip, Nature, 522(7554), 73–76, doi:10.1038/nature14480, 2015.

This paper has been cited and discussed

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