Author response to Reviewer 1

Dear Dr Tedstone,

Thank you once again for your constructive feedback, which has greatly contributed to the clarity and focus of our manuscript. Our replies to your latest comments are in blue below.

I thank the authors for their engagement with my comments on the first version of their manuscript. In this revised version, their emphasis on mapping effectively all features of the surface drainage system for comparison between the two years of study is clearer; the main aspects on the analysis that I found problematic previously have been removed or revised.

I am a little confused about the large effort that went into answering my concerns around use of remotely-sensed velocities. None of these materials make it into the revised manuscript. Presumably this omission is intentional.

Concerning the scope only of the revised manuscript, I consider the results presented to be supportable by the methods employed, which seem solid even if they still seem incremental compared with previous studies on similar topics. Nonetheless, I was pleased to see a more cautious and detailed approach taken to segmenting drainage versus refreezing.

We appreciate your recognition of the improvements made in our manuscript and the steps taken to address your previous concerns. Regarding the materials related to remotely-sensed velocities, we apologise for the confusion. When we submitted our response letter to you, our intention was to keep the velocity data in the manuscript, but then when we came to revise our paper, we ultimately decided to remove those data, for the reasons you gave in your last letter. So yes, the decision to omit these details from the revised manuscript was indeed intentional. We are pleased to hear that you now find our results better supported by our employed methods, and we appreciate your acknowledgment of our now more cautious and detailed approach to segment drainage and refreezing.

We believe that our findings, though they may be incremental, are indeed novel and do provide a new and valuable contribution to the broader literature on this subject. In providing a more comprehensive and detailed mapping of the surface drainage system, we believe our study is, and will be, of interest to the readership of The Cryosphere. In fact, the preprint alone has garnered data requests and engagement from several readers, and has been cited several times already, highlighting its relevance and impact. Specifically, while our study builds on existing work, it addresses key gaps in the current literature by including previously underrepresented features, such as small SGLs and slush. Our comprehensive mapping approach also enables us to capture distinctions between SGL drainage and refreezing across different elevations and melt intensities, which we believe adds depth to the understanding of how these processes vary in response to changing climate conditions.

Major comments

An overarching theme/rationale that the authors refer to several times is that 2019 could/may be considered a proxy for surface hydrology under projected future warming. For instance, on L415 (start of Discussion), the authors state that "*By examining supraglacial meltwater behaviour in 2019 - which experienced warmer temperatures than average - we can infer how the GrIS might respond to ongoing climatic change.*" While in principle I agree, I am not sure that these potential inferences are really developed in this manuscript, yet this seems to what the impact of the manuscript hinges on.

I would therefore welcome some more detailed discussion and caveating about what such a proxy approach can tell us. For instance: Where is this proxy approach valid? (e.g. in relation to underlying facies)? Can we just consider big melt years in isolation, versus what might the impacts be of several successive large melt years?

As an example, studies of ice slabs which support runoff from above the ELA indicate that these features develop over multiple years (e.g. MacFerrin et al., 2019; Machguth et al., 2022; Jullien et al., 2023). So, a single warm year like 2019 cannot in isolation provoke runoff from the higher elevations, unless they are already underlain by slabs. Another question could the degree to which slush field extents are a function of only melt, or also dependent on snowpack thickness/ice slab continuity/some other process. Engaging with such questions will push this manuscript beyond its present form of predominantly providing maps/observations, opening doors to further process understanding.

Thank you for this comment. We agree that a more nuanced discussion on the rationale behind using 2019 as a proxy for future warming would indeed strengthen our manuscript, especially regarding its limitations and implications. As you suggest, relying on a single high-melt year like 2019 may not fully capture the multi-decadal changes anticipated with ongoing climate warming. In response, we have expanded this theme in the Discussion, adding a new paragraph (see below), and have moderated the language in the manuscript to avoid presenting 2019 as a direct proxy for future conditions. Rather, it offers insight into how the ice sheet responds to significant melt events, making a valuable contribution to our broader understanding of its potential evolution under future warming.

We will add a new section to the discussion, pasted below:

'4.5 Limitations of using the 2019 melt season as a proxy for future warming

Using the relatively high melt season of 2019 to investigate how the GrIS responds to intensified warming offers valuable insights into the impact that future changes in climate may have on the ice sheet's hydrology and dynamics. Nonetheless, it is clear that using a single high-melt year like 2019 to directly infer future conditions has inherent limitations, due to the fact that surface properties are dependent upon changes that occur over multiple seasonal cycles (e.g., Hanna et al., 2024). Beyond melt intensity and duration, for example, surface properties such as snowpack thickness, firn air content, and the presence of ice slabs partially control supraglacial meltwater and slush field extents. Ice slabs, for example, act as impermeable barriers that prevent the vertical percolation of meltwater down into the firn and instead facilitate supraglacial runoff (MacFerrin et al., 2019; Jullien et al., 2023), while a thinner snowpack leads to faster saturation and slush expansion (Harper et al., 2012; Machguth et al., 2016). Therefore, while the 2019 melt season provides a snapshot of potential future melt and supraglacial hydrological conditions, it is essential to consider the broader context of multi-year climatic trends and the development of subsurface features such as ice slabs when fully assessing the GrIS's response to ongoing climate change.'

Second, the manuscript needs more substantial editing to concerning the accuracy and readability of the manuscript. This concern is reflected in my relatively long list of minor comments.

We appreciate your list of minor comments, which will help us to enhance the manuscript's accuracy and readability. We will address each of these minor points carefully, as detailed below, and also undertake our own thorough review of the revised manuscript text.

Also concerning editing, the manuscript favours the use of present tense throughout when describing events which occurred in the past – this seems unusual compared to most complementary literature so I ask the authors to consider changing tense accordingly, enhancing readability.

Following your feedback, we have carefully reviewed the manuscript to ensure consistent and appropriate use of tense throughout. Although we recognise that tense usage can vary across different journals, we opted for present tense in the results section as this appears to be the most common tense used for 'Results' sections of papers in our field. As Reviewer 2 was satisfied with the manuscript, we will defer to the editor's guidance on whether any tense adjustments are necessary.

Minor comments

L24: 'refreeze' → refroze

Changed "refreeze" to "refroze".

L30-L32: This assertion, that ice-dynamical feedback processes can increase mass loss (i.e. the ‘Zwally effect’), is somewhat out of date. Rather, ice flow in this sector slowed down during a large rise in meltwater production. See e.g. Sole et al. (2013, GRL), Tedstone et al. (2013, PNAS), Tedstone et al. (2015, Nature), Joughin et al. (2018, TC), Williams et al (2020, N.Sci.Rep), Halas et al. (2023, RSE).

Thank you for pointing this out. We have removed the statement on ice-dynamical feedback processes contributing to mass loss to better reflect recent findings, and to focus on meltwater runoff as the primary contributor to ice loss in our revised text.

L39: 110 km further inland by 2060 relative to when?

We have clarified the reference period. Added:’*…with models suggesting that, relative to 2000 – 2010, meltwater features on the GrIS…’*

L76: A reference to Yang and Smith (2016, JGR) would be very appropriate here.

Added.

L87 ‘unique’ is subjective, remove.

Removed.

L103-116: I am not sure of the relevance of ice-sheet-wide melt values here – what is important here is rather the melt extent/duration in the catchment of interest. This section could be shortened.

Our intention in this section was to provide background context by summarising melt conditions across the GrIS based on existing literature. We believe that ice sheet wide values are relevant as it reinforces that the high/low melt years occurred across the whole GrIS, not just the Russell/Leverett Glacier catchment. However, this section has been shortened slightly.

L145-148: Why were rock masks created? To my knowledge, there aren’t any nunataks in this area of the ice sheet?

Although nunataks are not present in the Russell/Leverett Glacier catchment, rock masks were applied as a precautionary step to avoid misclassifying any exposed debris or marginal rock areas, which do exist in this area, and which may have similar spectral characteristics to meltwater.

Fig. 2: ‘Data preperation’ has typo (→ preparation)

Changed.

L166: Yang and Smith 2012 doesn’t exist, maybe intended 2013?

Thanks, yes - changed.

L171/172: The < 2 px threshold for L8 seems fair, but 18 S2 pixels seems like quite a high bar, i.e. these seem more likely to be real features? Did you consider this possibility?

Thank you for raising this point. We applied a minimum feature size threshold of 0.0018 km² for both L8 (2 pixels at 30 m resolution) and S2 (18 pixels at 10 m resolution) to ensure consistency in detectable feature size across sensors and to avoid the introduction of inter-sensor biases. This is described in the manuscript on lines 168 - 170. Although, in principle, the higher spatial resolution of S2 would allow for more detailed capture of smaller features, we chose this threshold to maintain consistency across our dataset. This approach also aligns with established methods in meltwater mapping, such as those used by Dell et al. (2020).

Added*: 'The same area threshold was used for both L8 and S2, despite their differing resolutions, in order to ensure that consistency was maintained between sensors. This threshold ensures consistency in detectable feature size across sensors.'*

L182: how did this optimisation proceed? How sensitive are the results to the choice of thresholds? (maybe answered by sect. 2.9 'Uncertainty analysis of meltwater feature area').

Threshold values were tested iteratively by assessing slush detection performance against visually inspected true colour images.

Added: *'These values were selected through iterative testing against true colour images, where a range of potential thresholds were systematically evaluated. The iterative process involved visually comparing the classified slush areas with their appearance in true-colour images, allowing us to identify thresholds that best matched the expected spatial extent of slush.'*

L192/193: please comment on why different thresholds were used for the two different tiles and why this is a reasonable approach.

Different brightness thresholds were applied for the two Sentinel-2 tiles to account for variations in environmental conditions, such as elevation and surface reflectance, which can impact brightness levels across different parts of the catchment. This approach is in line with methods of other studies (e.g., Zhang et al., 2023), and we tested a range of thresholds in our study to ensure optimal channel delineation in each tile.

Added*: 'We tested threshold values in our study against true colour images to optimize channel delineation in each specific tile, a process consistent with approaches in other studies where thresholds are manually adjusted to ensure consistent feature extraction (e.g., Zhang et al., 2023).'*

L224: “Water depth for each meltwater feature pixel” does not match the section title “Calculating supraglacial lake depth and volume”. This is an issue in general with this section – make it clear that the retrievals are only for lakes and not also other meltwater pixels.

Changed ‘meltwater feature’ to ‘SGL’ throughout this section.

L298: I can see the link of lake surface area to surface slope, but not to thicker ice – what is the process rationale for making this link?

We agree that although these may be correlated it is not particularly informative physically, and so we have removed reference to ‘thicker ice’.

Fig. 3, box plots of hypsometry of hydrological features: I don’t really follow this plot nor the inferences gained from it. Why don’t the channel elevations in 2019 extend to similar lower elevations as 2018? Is this an oddity of the box plotting or truly the case? If the former then it might be better to use some other form of plot? In other words, this implies an absence of such features at lower elevations, which perhaps is not what the authors are trying to suggest? See also my comment about L425, below.

Thank you for raising this concern. The purpose of this plot is to show the relative hypsometrical distribution of meltwater features in each year; i.e. not just the min, max and median, but also information relating to the entire distribution. To clarify, we have updated Figure 3 to include outliers, which clearly show that channels at lower elevations are present in 2019, even if these values fall outside the interquartile range.

The figure caption has been revised to explain that the boxplots summarise the median and interquartile ranges, with whiskers and outliers representing the full elevation distribution. This should help avoid any misinterpretation about the absence of channels at lower elevations.

Figs. 4 and 7: please increase size of all labels in these plots. Very difficult to read at normal zoom level.

Thank you for the feedback. We have increased the size of all labels in Figs. 4 and 7 to improve readability at normal zoom levels.

Fig. 4: consider making more use of the y axes in some of the subplots (e.g. slush, total area, melt).

Thank you for the suggestion. We have adjusted the y-axes in the relevant subplots (e.g., slush, total area, melt) to make better use of the available range.

Fig. 5: difficult to judge the temporal resolution of this figure, would it be possible to improve?

Thank you for your suggestion regarding the temporal resolution of Figure 5. We understand this comment to relate to the sampling frequency provided by the satellite measurements. To address this, we have therefore added distinct markers for each sample day to enhance clarity. Additionally, we have updated the figure caption to explicitly state, *"The black markers indicate the days on which measurements were acquired."* and adjusted the plot size for better visibility.

L401/402: both 'mean' and 'average' used in same sentence, please clarify confidence in SGL refreezing at lowest elevations in May? What is the subsequent evolution of these lakes?

This section now consistently uses 'mean'.

The patterns observed in our study suggest that some SGLs at lower elevations might undergo refreezing during May. By analysing subsequent satellite imagery, we confirm that these features do not simply melt out and reform as lakes later in the season. However, limitations in the temporal resolution of the imagery and the lack of ground-based observations prevent us from fully characterising their subsequent evolution —such as whether they remain refrozen or drain.

We will add the following to the results section: *'We observe that ~12 SGLs appear to refreeze at lower elevations in May 2019, but limitations in temporal sampling prevent full characterisation of these dynamics.'*

L419-420: filler sentence, can be removed

Removed.

L425: 'tend to form at different elevations'. Is this really the case? To my eyes, there is the distributions of each feature type overlap strongly in elevation. According to the results here, while it seems fair to say that slush is predominantly a higher-elevation phenomenon, lakes and channels are distributed across all elevations, so 'different' is not the right word here. This might sound pedantic but it is important to make it clear that all three features are basically capable of coexisting in a given elevation band, especially at high elevations. Maybe part of the challenge here is trying to use elevation as process, lumping the actual factors as play such as melt intensity and duration, surface slope/local topography, winter snowpack thickness…

Thank you for this observation. We agree that while slush is predominantly observed at higher elevations, all three meltwater feature types (slush, lakes, and channels) can coexist within similar elevation bands.

Changed text to: *'This is in keeping with previous studies that found meltwater features tend to reach increasingly higher elevations during more intense melt years (e.g., Sundal et al., 2009; Liang et al., 2012; Lüthje et al., 2006). While these studies primarily* *focused on SGLs, our results indicate that channels and slush can typically coexist with SGLs across similar elevation bands, although slush is more commonly observed at higher elevations.'*

L426-427: 'indicative of substantial surface runoff' – what is? Rivers? Or slush fields and lakes? Requires clarification.

We have clarified the statement: *'The formation of channel, slush and SGL features at higher elevations'*

L427-430: given the level of mapping in this study, was it not possible to identify the presence/absence of such moulins during the two years of study? Even more so as Yang et al. (2021) only used (30 m) Landsat images, not 10 m Sentinel-2 images.

We believe that a detailed mapping of individual moulins was beyond the scope of this study, which focuses more on the distribution and dynamics of surface meltwater rather than its access to the bed. Instead, we relied on the findings of Yang et al. (2021), whose analysis using 30 m Landsat imagery provided sufficient inferences regarding moulin presence and distribution for our purposes. While Sentinel-2's higher resolution (10 m) could offer finer detail, our study prioritised broader spatial and temporal assessments of meltwater features.

L425/426: do channels and slush actually form at different elevations to lakes, or is it rather more that there are overlaps in the distribution….?

Please see our reply to the comment above relating to L425, which addresses this point.

L431-433: update to take account of the literature relating to ice slab development. Clarify what types of meltwater feature the authors believe result in firn densification – how does this work?

We have reworded this sentence to read: 'The formation of meltwater features within the high elevation percolation zone may result in densification of firn underlying the meltwater feature, and possibly ice slabs (e.g., MacFarren et al. 2019; Jullien et al., 2023), reducing its meltwater storage capability and leading to enhanced supraglacial meltwater runoff (Machguth et al., 2016; Nienow et al., 2017).'

L437-440: There is something odd about the order of phrasing here. In particular, I don't understand why the slush limit is effectively defined twice (linkage of two sentences by the word 'Thus'). Please de-duplicate and clarify. What is the key message here?

Thank you for pointing this out. We have clarified and consolidated the definition to improve flow and eliminate redundancy.

The revised sentence now reads: *'In previous work, the 'slush limit' has been used as an indicator of the visible runoff limit, representing the upper boundary where meltwater runoff is directed to the ocean and contributes to mass loss (Gruell and Knap, 2000; Tedstone and Machguth, 2022; Machguth et al., 2023; Clerx et al., 2022).*

L444: "slush in the sub-surface" – can you help me to identify the intended message here?

The intended message is that while optical satellite imagery provides a view of surface slush, it likely underestimates the full extent of its presence as some slush may be retained within subsurface layers.

To clarify, we have revised the text: *'We note that while our study—and indeed others that have used optical satellite imagery—can only detect slush on the ice surface, slush may also exist within the subsurface snowpack (Clerx et al., 2022). As such, our study serves as a minimum bound for slush extent, because the total slush area may be even greater than what we can observe from optical imagery alone.'*

L445-452: This section requires full referencing to the literature. Note also that the Culberg et al (2021) paper does not explicitly deal with slush features (which is what this section of the present manuscript is about), instead it tracked the percolation of smaller amounts of surface meltwater into the firn, where it refroze, producing ice layers; these ice layers can be considered pre-cursors to ice slabs (e.g. Jullien et al., 2023, GRL).

We have revised this section to include full referencing to the relevant literature and to ensure the discussion remains focused on slush, we have removed the reference to Culberg et al. (2021). The revised text now reads:

'The refreezing of slush at high elevations may have a long-term influence on the runoff from the GrIS by forming near-surface ice slabs, which act as impermeable barriers, restricting meltwater percolation into the firn and reducing retention capacity (Machguth et al., 2016; MacFerrin et al., 2019; Miller et al., 2022; Jullien et al., 2023). As high melt years like 2019 become more frequent, it is likely that slush will become more prevalent, leading to the formation and expansion of impermeable ice slabs, preconditioning the ice sheet surface for greater ponding and surface runoff in future years. This is potentially already occurring at Humboldt Glacier in North Greenland, where a previous study identified the earlier activation of the supraglacial hydrologic system and longer melt-seasons in years following widespread slush events (Rawlins et al., 2023).'

L461-462: what studies are these, and then for these studies, does the exclusion of slush potentially affect their conclusions? I.e. to what degree has the exclusion of slush been a critical shortcoming?

As requested, we have updated the text to explicitly reference the relevant studies and clarify the potential impact of excluding slush on their conclusions. Revised text:

‘Most prior mapping studies of Greenland's supraglacial hydrology, such as those by McMillan et al. (2007), Selmes et al. (2011), Williamson et al. (2017, 2018), and Miles et al. (2017), have focused on SGLs, while others, including Smith et al., (2015), Yang et al. (2021), Lu et al. (2021), and Turton et al. (2021), examined meltwater channels and drainage patterns. However, these studies largely overlooked slush, which our findings show accounts for a significant proportion of the total meltwater area. The exclusion of slush in these previous studies likely led to underestimations of meltwater extent. We suggest that including slush in future mapping and modelling studies is essential to comprehensively capture meltwater dynamics. We further advocate for the development of regional climate models that account for slush's impact on albedo, which will enhance the accuracy of modeled surface meltwater production. ’

L471: channels cannot reach ‘*ever higher*’ elevations – the ice sheet is finite.

Removed ‘ever’.

L471-472: ‘…channel features will reach ever higher elevations, and thus meltwater routing will be more efficient’ – I'm not sure that I see the direct process linkage here. (a) There is no need for meltwater routing if there is no meltwater to route. (b) It migh be useful to distinguish conceptually between lower vs higher elevations here. Lower elevations have steeper gradients, less winter snowpack/no firn, crevassing, thinner ice (easier hydrofracture) and so on. On the other hand, higher elevations are very flat, underlain by slabs/firn, broadly with thicker overlying snowpack. Could you comment on the implications that these distinctions have for inland development of supraglacial hydrology in a warming climate?

We agree with this comment, and we appreciate the opportunity to clarify this point.

We will revise the text as follows: *‘At lower elevations, meltwater routing is more efficient than at higher elevations (e.g. Smith et al., 2015), due to steeper gradients, crevassing, and minimal surface snow cover. Nonetheless, as air temperatures increase, as observed in 2019, so may the density and extent of the drainage network, with channel formation also extending to higher elevations (e.g., Yang et al., 2023), even in flatter, snow-covered regions, as demonstrated in our study.’*

L486-487: this invokes *time as process*, which would preferably be avoided. Rather, the subglacial hydrological system is inefficient in the absence of meltwater input. Inputs of meltwater earlier in the melt season are therefore likely to speed up the transition of the subglacial hydrological system to a more efficient configuration – these meltwater inputs from small SGLs won't just passively move their way through the basal system, they will be involved in modifying it.

We have reworded this sentence to read: 'Our observations demonstrating that small SGLs tend to drain earlier in the melt season is important because this is when the subglacial hydrological network is generally inefficient due to an absence of preceding melt. (e.g., Bartholomew et al., 2010), and so early SGL drainage may speed up the transition of the subglacial system to a more efficient configuration.

L488: What makes small SGLs an 'important component'? Quantify what is meant by 'important'. E.g. Likely % of meltwater drained through these features, percentage of SGLs which are small…this starts to get resolved around L496, so maybe it would be useful to consider how to shorten and clarify this paragraph.

Thank you for raising this point. To address your comment, we have revised the following paragraph to clearly articulate and quantify why small SGLs are an important component of southwest Greenland's supraglacial hydrological system:'

'We have identified that small SGLs are an important component of southwest Greenland's supraglacial hydrological system. Indeed, while small supraglacial lakes contribute only a relatively small proportion (3.5% % in 2018 and 3.1% in 2019) to the overall drainage flux across the entire melt season, their importance lies in the timing of their drainage events, which typically occurs at the start of the season when the subglacial hydrology system is inefficient due to a lack of preceding meltwater input. The drainage of small lakes dominate the first month of each melt season (e.g., June 2018 and May 2019), when they account for the majority (>80%) of drainage events. This early-season activity may play a critical role in the initial evolution of the subglacial hydrological system, helping to control when and where an efficient drainage network is established. Furthermore, in both melt years studied, over a third of small SGLs drained in areas where ice thickness is less than 1 km, making it plausible that their drainage pathways extend to the bed, if they drained rapidly via hydrofracture. If so, moulin density at these lower elevations may be higher than previously reported (Banwell et al., 2016; Hoffman et al., 2018). This challenges the previous assumption that smaller lakes are unlikely to trigger hydrofracture through 1 km thick ice (Krawczynski et al., 2009), which was stated as the reason for why studies such as Williamson et al. (2018a) and Miles et al. (2017) restricted analyses to SGLs larger than 0.0495 km². Our observations suggest that small SGLs may play a more significant role in meltwater routing than previously recognised, particularly during high-melt years such as 2019, when small lakes were approximately 15% more abundant, formed earlier in the melt season, and occurred at lower elevations compared to 2018. We suggest that including SGLs smaller than 0.0495 km² in future remote-sensing and modelling-based studies is important for better understanding where, when, and how much supraglacial meltwater is routed to the ice sheet bed, and the associated implications for ice dynamics.'

L504: is there any evidence/observations for this proposition that the root cause is increased ice speeds? And, is this not a bit chicken-and-egg, given that SGL drainages would be needed to route meltwater to the bed and transiently increase sliding?

Dunmire et al (2024), which we reference in our paper, were the first to make the observation that lakes that drain in 2018 are typically deeper than those that don't drain in 2019, which they partly attribute to earlier season lake drainage in 2019. But they do not include velocity data analysis. And as we no longer include velocity data analysis in this paper (on the recommendation of this reviewer), we cannot comment on these velocities either. We agree that the relationship is complicated, and may be a result of 'chain reaction' style lake drainage, whereby one lake drainage event triggers other nearby drainages.

L506: This seems rather speculative. Is there evidence in the observations underlying this manuscript of lake drainage by overland flow? I didn't see them presented anywhere?

We acknowledge that our statement about overflow via basin-side incision is speculative, as we do not present explicit evidence of lake drainage via this process. However, our interpretation is informed by the observed increase in the interconnectivity of the supraglacial channel network in 2019 compared to 2018. We address this in the comment directly above.

L533, sea level rise implications: what are the implications precisely? I can't see them stated in the Conclusions and didn't spot anything elsewhere in the manuscript.

While our study doesn't explicitly quantify sea level rise, it provides insights into how rising air temperatures and more intense melt events impact GrIS hydrology, which is crucial for understanding future meltwater runoff, mass balance, and their potential contributions to sea level change. The revised text reads:

'As such, this work aims to contribute to an improved understanding of how rising air temperatures and more intense melt events may impact the hydrology of the GrIS. Ultimately, this research provides detailed observations of GrIS hydrology, offering new insights that can help to inform the representation of hydrological processes in the physical models that are used to make projections of future mass balance and sea level rise.

L537: 'modulate ice sheet surface runoff' – what is the directionality here? On the one hand the suggestion seems to be that bigger melt years = more runoff, on the other hand that bigger melt years = more slush = less runoff. Please clarify.

As requested, we have revised this sentence to clarify the proposed directionality of the relationship: *'Additionally, greater slush extents in these years, which lowers surface* *albedo and reduces surface permeability when it refreezes, may increase ice sheet surface runoff and hence decrease surface mass balance.'*

L539: 'potentially increasing ice flow velocities.' Over what time frame? At a magnitude that we should be concerned about? Please also see my comment about L30-32.

We agree that focusing on ice velocity changes may oversimplify the underlying processes, so we have deleted these words, and instead now focus on the processes of basal lubrication and sliding. Here is the revised text:

'Moreover, the formation of smaller SGLs during higher melt years like 2019 may enhance surface-to-bed connections, which has implications for the representation of subglacial hydrology in models and seasonal evolution of ice velocity.'

Code availability: Are the authors planning to lodge code alongside this study? Given the strong methodological bias of this study, this would constitute a highly relevant output and enable the adapted/extended analysis pipelines presented here to be extended to other years and sectors, even if code deposition is not specifically required by TC policies. (Not a criteria for acceptance).

Thank you for your interest in our code. Our analysis pipeline builds on the methods established by Corr et al. (2022), which focused on Antarctic conditions. Given the strong alignment between our code and this previously published version, we believe that Corr et al. (2022) provides a robust foundation that can be adapted for similar analyses. Additionally, the full MATLAB source code for the FASTER algorithm, a key component of our SGL drainage analysis, is freely available for download.

We will ensure that this is reflected in the Data Availability section as follows:

'The meltwater delineation code adapted from Corr et al. (2022) is available from https://doi.org/10.5194/essd-14-209-2022. The full MATLAB source code for the FASTER algorithm is available for download from https://doi.org/10.17863/CAM.25769 (Williamson, 2018b).'

References added to manuscript:

Jullien, N., Tedstone, A. J., Machguth, H., Karlsson, N. B., and Helm, V.: Greenland Ice Sheet ice slab expansion and thickening, Geophys. Res. Lett., 50, e2022GL100911, https://doi.org/10.1029/2022GL100911, 2023.

Author response to Reviewer 2

Dear Reviewer,

Thank you for your review and feedback. We appreciate your recognition of the improvements made based on the initial Reviewer 2 comments, including the updated methods, revised figures, and clearer definitions. As suggested, we have now included supplementary GIFs to enhance the visualization of seasonal meltwater evolution.

We are pleased that you are satisfied with the revisions and support the manuscript for publication as it stands. We appreciate your time and input.

Best regards,
Emily Glen (on behalf of all co-authors)