

Reviewer 1 – Author Response

The following section provides a point-by-point response to Reviewer 1 (Dr Amber Leeson). The reviewer's comments are shown in black, followed by our response shown in blue.

1. General Comments

This manuscript quantifies the interannual (2017–2024) and seasonal (2018–2019 melt season) evolution of supraglacial channel networks (SCNs) on the Nivlisen Ice Shelf, East Antarctica, using Sentinel-2 and Landsat-8 imagery. Specifically, the authors derive key network metrics including surface water area, total channel length, number of SCNs and number of individual channels. They report strong interannual variability, and demonstrate progressive seasonal integration and late-season fragmentation during 2018–2019. Correlation analyses indicate strong coupling between SCN metrics and broad scale climate.

The study directly addresses a knowledge gap: while supraglacial lakes (SGLs) on Nivlisen (and indeed in Antarctica more broadly) have been extensively studied, SCNs have not been systematically quantified. This is important because SCN can efficiently route surface meltwater laterally across an ice shelf and, in some cases, off the ice front, thereby reducing the residence time of water on the surface (e.g. Leeson et al., 2020). By limiting widespread ponding, SCNs may reduce the potential for surface water to infiltrate and densify the firn layer, which would otherwise enhance future meltwater retention and decrease pore space. At the same time, the spatial focusing of meltwater within channels and connected lakes could locally increase hydrostatic loading and fracture propagation, leading to instability.

More work is required to understand SCNs and their role in the potential fate of Antarctica's ice shelves. The manuscript therefore makes a clear and valuable contribution to Antarctic ice-shelf hydrology and I recommend publication following revisions. Specific comments can be found below.

We thank Reviewer 1 for their constructive and encouraging comments, and for recognising the value of systematically quantifying SCNs on Nivlisen Ice Shelf. In the revised manuscript, we will address the reviewer's comments by clarifying the terminology and mapping workflow, adding stronger caveats regarding water-depth estimates and the short observational record, and presenting the climate and stability implications more cautiously. We believe these revisions will strengthen the clarity and balance of the manuscript.

2. Specific Comments

1). Methodological clarity

I find the use of use of 'network' and 'channel' a bit confusing. The manuscript defines SCNs as 'drainage networks including both lakes and channels' however, it is missing a definition of network. Fig 3 suggests that a single channel is classed as a network, if it is not connected to another, but this seems counterintuitive and at odds with the definition of SCNs.

We agree that the distinction between channels and SCNs was not sufficiently clear. In the revised manuscript, we will define the mapping units more clearly. Individual channels will refer to manually mapped supraglacial channel features visible in the satellite imagery. Channel segments will refer to mapped channels that form part of a larger connected drainage system. The output of the 8-neighbour connectivity analysis will be described as mapped drainage

components, which may include isolated channels, multiple connected channels, or channels connected to lakes.

To avoid implying that a single channel is itself a network, we will reserve the term SCN for connected multi-feature drainage systems, such as systems with more than one connected channel or channel-lake connections. Isolated single-channel features will be described as isolated drainage components. These components will still be retained in the analysis because they represent visible meltwater-routing pathways, and because narrower tributaries, shallow flow paths, firm-covered links, or transient connections may be unresolved at the image resolution. We will revise the Methods section and Fig. 3 to make these distinctions explicit, including labels in Fig. 3d. Where necessary, we will also relabel the metric previously described as 'number of SCNs' as 'number of mapped drainage components', because this metric is derived directly from the connected-component analysis.

I'm also not clear why NDWI is used since it seems that this is augmented with manual delineation. Why not exclusively manual delineation given the small scale of data?

We agree that the mapping workflow was not described clearly enough. In the revised manuscript, we will clarify that *NDWIce* was used to initially identify broad areal water bodies, such as lakes and larger surface-water areas, whereas supraglacial channels were manually identified and drawn using the *NDWIce* product and true-colour optical imagery. *NDWIce* was retained because it provides a consistent and reproducible basis for mapping surface water area across images. However, narrow and partially resolved channels are difficult to extract reliably from *NDWIce* thresholding alone. We will therefore revise the Methods section to describe this as a complementary raster-vector mapping workflow and will update Table 1 to specify the data type used for each metric.

I also suggest that you give water depth as relative rather than absolute given high uncertainty in depth detection methods (e.g. Melling et al., 2024).

We agree that optical water-depth estimates have substantial uncertainty and should not be interpreted as precisely validated absolute depths. However, we prefer not to convert these values into a newly defined relative index, because they were derived using a commonly applied optical water-depth method and retain a physically interpretable scale that allows comparison between images and with previous studies. In the revised manuscript, we will therefore retain the optically derived water-depth estimates but will emphasise that they are primarily used to assess relative spatial and temporal variations in water depth, rather than exact absolute depths, and we will add clearer caveats in the Methods and Discussion.

2). Topographic dependence

The distribution of features in space is explained with reference to local topography and firm conditions. I agree topography is likely the dominant control, and would like to see all your elevation maps given on a log scale so that small scale variations on the ice shelf can be seen in addition to broad scale grounded-to-floating gradients.

We will revise the elevation figures using a non-linear colour stretch to improve the visibility of local topographic features, especially over the relatively lower relief ice shelf area.

3). Interannual variability.

I understand that S2 was used for the interannual assessment and L8 was used for the seasonal comparison intentionally so that you are not mixing sensors, however I wonder if a bit of mixing might be useful. The interannual variability you see in your dataset is striking, and similar to signals that we see elsewhere in Antarctica (e.g. GVIIS, Barnes et al., 2020). It would be interesting to know how far you can go back, and what you might find, if you brought Landsat in for the inter-annual comparison. 8 years is not so very many given the high degree of variability you see. Even if this is qualitative, it would be interesting to know if the patterns you see in 2019

and 2020 are unique to that epoch, or have occurred regularly in the past. On GVIIS for example, extensive meltponding has been observed as far back as the 1970s.

We will do a qualitative historical Landsat check, for example, we will select several satellite images from 1990 to 2010 to quickly check whether the patterns in 2019 and 2020 occurred previously. We will then comment on this in the manuscript and likely add a Figure or Supplementary Figure.

Figure 8a seems to show frozen lake area downstream of the open-water lake. Could this be a remnant from the previous year?

It is possible that the frozen lake area downstream of the open-water lake could represent a remnant from a previous melt season. We will revise the interpretation to state this possibility more explicitly and cautiously. We will note that preliminary inspection of Landsat-8 and Sentinel-2 imagery from the 2019 melt season suggests that the open-water lake remained broadly similar in extent, while earlier observations from Kingslake et al. (2015) show that supraglacial lakes and surface drainage have occurred in a similar sector of Nivlisen Ice Shelf in previous years. These observations are consistent with the possibility that some frozen or refrozen lake surfaces may reflect carry-over water storage from an earlier melt season, although the available imagery does not allow us to confirm the wintertime evolution of this feature directly.

Kingslake J, Ng F, Sole A. Modelling channelized surface drainage of supraglacial lakes. Journal of Glaciology. 2015;61(225):185-199. doi:10.3189/2015JoG14J158

4). Seasonal evolution.

It would also be nice to see a comparison of contemporaneous images from both L8 and S2 to get a feel for what might be missing from the seasonal comparison. Since channels narrower than 20-30 m may be unresolved or only partially resolved in Landsat, what uncertainty might this introduce into your findings, especially in lower melt years?

We agree that a direct comparison between near-contemporaneous Landsat-8 and Sentinel-2 imagery is useful for assessing the uncertainty associated with the Landsat-based seasonal analysis. We will add such a comparison using Landsat-8 and Sentinel-2 images acquired on similar dates in the revised manuscript.

The discussion around seasonal evolution is interesting, although I would like to see greater clarity (and possibly stronger caveats) applied to the more speculative arguments including the 'unstable drainage' paragraph. I note that we see a transfer of water downstream on LBIS in the months before collapse (Leeson et al., 2020, Figure 5) so this seems plausible. I don't understand however why lakes would discharge laterally, and then refill later given that we would probably assume the overflow channel to persist throughout the remainder of the melt season.

We will revise the Discussion to apply stronger caveats to the interpretation of seasonal drainage reorganisation. We will remove the term 'unstable drainage' when describing our observations. Although this term was used in relation to the Kingslake et al. (2015) lake-drainage framework, our data do not resolve discharge, or channel incision. We will instead describe the observed pattern as transient lake-channel connectivity and seasonal drainage reorganisation.

We agree that lake discharge followed by refilling is unusual, and we do not have a clear explanation for how it may occur. Overflow channels may be shallow, intermittent, refrozen, snow-filled, or only activated when lake level exceeds a local spill threshold, allowing continued meltwater input to refill a lake after partial drainage. However, without continuous *in situ* observations or field measurements, and given the discontinuous satellite record, we cannot directly validate the full drainage-refilling sequence. In the revised manuscript, we will therefore present this interpretation cautiously and explicitly acknowledge these uncertainties.

5). Role of climate

A lot of weight is put on the correlation analysis between hydrological parameters and climate variables. I think this is perhaps unjustified given the short (8 year) time series and that as a consequence this could be streamlined. It is more likely that broader scale climatic patterns as indicated by ERA5 temp would show a statistically significant correlation on these timescales than specific small scale signals e.g. MAR snowfall. It is also probable that because of the short time series the correlation analysis is sensitive to single year outliers (e.g. 2019). I'd be interested to see you include the melt-over-accumulation ratio in your analysis (van Wessem et al., 2023) and a map of snow/firn depth somewhere (perhaps in fig 1).

We agree that the climate correlations should be interpreted cautiously because the interannual record contains only eight annual observations. In the revised manuscript, we will revise the climate section to make the interpretation more concise and cautious, and we will make clear that the correlations are used as exploratory climatic context rather than as definitive evidence of causal controls on SCN evolution. We will also reduce the interpretation of weak or non-significant relationships and add a clearer limitation statement on the small sample size.

To address the reviewer's concern that the correlations may be sensitive to individual years, we have examined scatter plots (Figure 1) for the strongest statistically significant relationships, particularly those between ERA5 mean summer temperature and key SCN metrics. These plots suggest that the main positive relationships are not solely driven by a single outlier year, including 2019. Instead, lower-temperature years generally correspond to lower surface water area, channel number, total channel length and SCN number, while warmer years show greater SCN development.

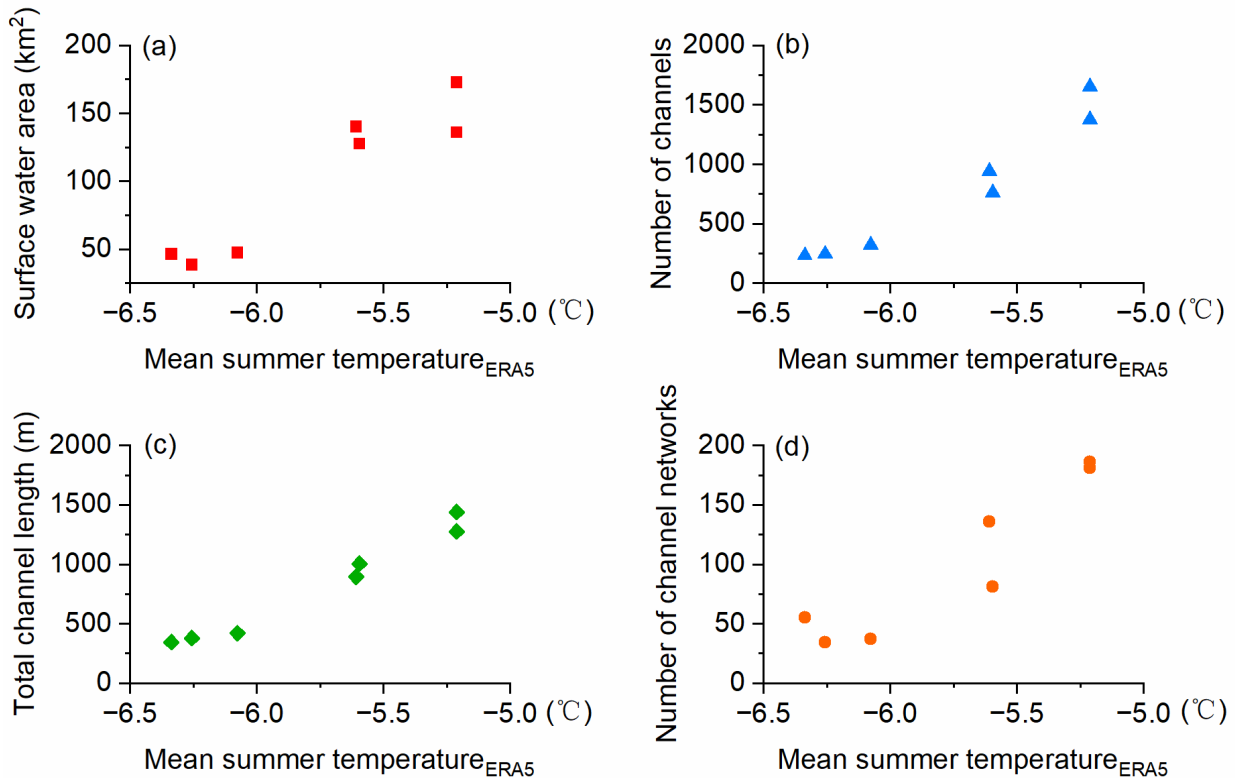


Figure 1. Scatter plots showing the relationships between ERA5 mean summer temperature and key SCN metrics. The plots are included to show the distribution of the annual observations and

to assess whether the strongest correlations are dominated by individual years. The data point of 2022 is not shown because no visible open-water features were mapped in that year.

Following the reviewer's suggestion, we will also include a melt-over-accumulation ratio, clearly defining the melt and accumulation terms and the period over which the ratio is calculated. Finally, we will also add spatial context for snow/firn conditions. We will first check whether a suitable publicly available snow/firn depth or firn-air-content product can be used for Nivlisen Ice Shelf. Possible sources include Antarctic firn or surface-mass-balance products such as RACMO/IMAU-FDM-derived firn air content, MAR-derived snowfall or firn-related variables, or other publicly available Antarctic ice-shelf datasets.

6). Implications?

The study frames the importance of SCNs as a mechanism for redistribution of meltwater on Antarctic ice shelves and, as a corollary, stabilising or destabilising them. It doesn't however then go on to comment on what the implications of their specific findings are for the future stability of the Nivlisen ice shelf, which is what I would expect from such framing. The authors may feel their contribution is more useful in terms of new process level understanding than impacts of stability. This is of course fine, and very useful, but means that the framing needs to be adjusted accordingly.

We agree that the original framing placed more emphasis on ice-shelf stability than was directly supported by the analysis. We will revise the Discussion to clarify that the primary contribution of this study is to provide process-level understanding of SCN evolution and meltwater redistribution, rather than to directly assess the future stability of Nivlisen Ice Shelf. However, we will also briefly discuss the potential implications of these findings for ice-shelf stability.

7). Data Availability and Reproducibility

Data availability statement indicates mapped SCNs are available upon request, I suggest that these are instead deposited in a public repository (e.g., Zenodo) upon publication.

We agree that making the mapped SCNs publicly available will improve reproducibility. The mapped SCNs and associated metric tables will be deposited in a public repository (likely the UK Polar Data Centre, with an alternate of Zenodo) and will be freely available upon publication.

New References:

Barnes, T. J., Leeson, A. A., McMillan, M., Verjans, V., Carter, J., and Kittel, C.: Changes in Supraglacial Lakes on George VI Ice Shelf, Antarctic Peninsula: 1973–2020, *The Cryosphere Discuss.* [preprint], <https://doi.org/10.5194/tc-2021-214>, 2021.

Leeson, A. A., Forster, E., Rice, A., Gourmelen, N., & vanWessem, J. M. (2020). Evolution of supraglacial lakes on the Larsen B ice shelf in the decades before it collapsed. *Geophysical Research Letters*, 47, e2019GL085591. <https://doi.org/10.1029/2019GL085591>

Melling, L., Leeson, A., McMillan, M., Maddalena, J., Bowling, J., Glen, E., Sandberg Sørensen, L., Winstrup, M., and Lørup Arildsen, R.: Evaluation of satellite methods for estimating supraglacial lake depth in southwest Greenland, *The Cryosphere*, 18, 543–558, <https://doi.org/10.5194/tc-18-543-2024>, 2024.

van Wessem, J.M., van den Broeke, M.R., Wouters, B. *et al.* Variable temperature thresholds of melt pond formation on Antarctic ice shelves. *Nat. Clim. Chang.* **13**, 161–166 (2023). <https://doi.org/10.1038/s41558-022-01577-1>