

## Reviewer 2 – Author Response

The following section provides a point-by-point response to Reviewer 2. The reviewer's comments are shown in black, followed by our response shown in blue.

### 1. General Comments

This paper uses remotes sensing and the output of a regional climate model and a reanalysis product to examine the surface drainage system that forms on the Nivlisen Ice Shelf. The authors map the channel network over eight melt seasons, derived metrics related to development of the drainage system, and compare these to weather conditions. They also examine the evolution of the drainage system over one melt season (2018-2019).

The paper could be a useful extension to the growing literature on similar systems and different aspects of the same system. This work is novel in its detailed focus on surface channel networks on Nivlisen; other papers have worked on Nivlisen Ice Shelf, but have either performed only a preliminary analysis of the drainage system or have focused on the lakes rather than the whole drainage network, while others have performed detailed analysis of drainage systems on other ice shelves. As such, I consider the current work a relatively minor advance over those previous efforts. It has the potential to be a useful addition nonetheless.

A couple of main comments are below that the authors should consider before the paper can be published in my opinion, followed by more minor comments.

We thank the reviewer for their careful assessment of the manuscript and for recognising that the study provides a novel, detailed focus on supraglacial channel networks on Nivlisen Ice Shelf.

### 2. Main comments :

Efficiency is mentioned in the abstract and in several other places in the manuscript. However, I do not think the paper presents any direct observations of efficiency. In fact, I am not sure what efficient means in this context. It seems like the way it is being used in this paper could be close to 'well-connected'. Is that correct? If so, maybe use a term like that which is more closely tied to the observations, rather than efficient, which is something that is vaguely associated with how fast/easily water moves through a system, which of course we do not know from these remote sensing observations alone. Alternatively, a clearer argument should be made linking the observations to efficiency specifically, but if the route is taken, I would like to see this link introduced, proposed and discussed in a critical way, i.e. in a way that considers the arguments for and against linking the observations to efficiency in this way.

There is little discussion of limitations. A major limitation that has the potential to undermine many of the inferences made from derived statistical relationships is the very short times series analyzed. Only eight melt annual snapshots of the drainage systems are considered and only eight snapshots of the system are considered during the analysis of seasonal evolution during the 2018-19 season. This must be critically discussed.

We agree with the reviewer that 'efficiency' was not sufficiently defined and that our observations do not directly measure how easily water can move across the ice shelf. In the revised manuscript, we will remove or replace the term 'efficiency' in most places and use observationally grounded terms such as 'connectivity', 'meltwater redistribution'. We will add a clarification that drainage connectivity is a precursor to hydraulic efficiency, but that hydraulic efficiency also depends on water velocity and discharge. Consequently, it is not possible to provide a direct measurement of hydraulic efficiency from optical satellite imagery alone.

In addition, we will add a discussion of limitations and revise the climate-related discussion to explicitly acknowledge the short interannual and seasonal records. The revised manuscript will state that the eight-year interannual record limits the statistical power of the correlation analysis and that the correlations should be interpreted as exploratory and contextual rather than causal. To address the reviewer's concern about sensitivity to individual years, we have examined scatter plots for the strongest climate-SCN relationships (see Figure 1 in response to Reviewer 1). These suggest that the main positive relationships are not solely driven by a single outlier year, although the small sample size still warrants caution. We will include these scatter plots in the revised manuscript or Supplementary Material.

### 3. Line-by-line comments:

Line 38: An additional relevant citation is Spergel et al 2021. [This citation will be added in the revised version.](#)

L49: I am not sure that an increase in hydrostatic pressure is a good summary of the stress changes involved in the mechanism proposed in Banwell et al. (2013). I suggest editing this sentence. [We appreciate the reviewer's attention to this detail. This will be amended to read as suggested.](#)

L50: Point 3 is not an example of a primary mechanism of hydrofracturing, as is suggested by its presence in this list. [This section will be reworded.](#)

L56: Lai et al. (2020) is a relevant citation here. Particularly given that they talk about spatial variations in ice-shelf vulnerability to meltwater and SCNs move water around. [This citation will be added in the revised version.](#)

L104: 'during the past 20 years', Kingslake et al. (2015) only mentioned the 2002-2009 in their paper. [We thank reviewer for the correction. This will be amended to read as suggested.](#)

L111: Is this the citation for the speed, and coastline and GL products? If not, add all required citations. [The Antarctica Ice Velocity dataset is from Rignot et al. \(2017\). This citation will be added in the revised version.](#)

[Rignot, E., J. Mouginot, and B. Scheuchl. 2017. MEaSURES InSAR-Based Antarctica Ice Velocity Map, Version 2. Boulder, Colorado USA. NASA National Snow and Ice Data Center Distributed Active Archive Center. doi: 10.5067/D7GK8F5J8M8R.](#)

L122: Delete 'very much'. ['very much' will be removed as suggested.](#)

L136: Delete 'by'. ['by' will be deleted as suggested.](#)

L142: Would it be simpler to just use 'lake' instead of SGL and 'networks' instead of SCNs? [We retain SGL and SCN because these terms are widely used in the literature, but we will reduce unnecessary abbreviations and use "lake" or "network" where it improves readability.](#)

L152-153: "Furthermore, while observations suggest that these networks often spatially merge into a small number of large channels that may terminate in a large lake" If this is something that the previous work highlights, I suggest noting that above when you discuss the individual papers. This will strengthen the case for focusing on this question specifically in this paper. [We agree that this point should be introduced earlier when discussing previous studies on Nivlisen Ice Shelf. In the revised manuscript, we will expand the discussion of previous studies to note that earlier observations indicate that surface meltwater features can become spatially organised into larger downstream drainage pathways and associated lakes.](#)

L155: The last sentence of this paragraph takes on an urgent tone that seems out of place and is unsupported by the preceding literature review. It is unclear from reading the preceding text what is urgent about the need to quantify channels on Nivlisen specifically. Is there an indication

that collapse of the Nivlisen is imminently if meltwater delivers water to specific areas, for example? We agree that the original wording overstated the urgency. We will revise this sentence to frame the study as addressing a specific observational gap rather than implying imminent instability of Nivlisen Ice Shelf.

L190: imagery → image 'imagery' will be removed as suggested and replaced with 'image'.

L190: There is a change in tense here. We will revise this sentence.

L203: Rephrase 'Red band on optical imagery' 'Red band on optical imagery' will be revised as 'the red band of the optical imagery'.

L239: delete 'the' 'the' will be removed.

L241: Do not capitalize 'Density' 'Density' will be revised as 'density'.

Table 1: replace 'n' in the units column with '-' or something similar to indicate no units. The letter 'n' in the units column in Table 1 will be replaced with '-'.

L277: 'stands for' 'is' The 'stands for' will be replaced with 'is'.

L293-294: Is there a typo here or is it the 'accumulated melt' This seems a bit confusing to combine 'melt' and 'accumulation'. This was not intended to refer to accumulated melt, rather, 'November to January accumulation' referred to accumulated snowfall over the November-January period. We agree that the original wording was unclear, and in the revised manuscript we will replace this phrase with 'November-January accumulated snowfall' to avoid confusion with melt or surface mass balance.

L371-372: Is there a missing word in this sentence? We will rephrase the sentence as: 'The interannual variability in drainage density is shown in Fig. 7, where areas with high drainage density ( $>2200$  m km<sup>-2</sup> of cumulative channel length) are represented by dark blue colours.'

L375-377: I thought a decrease in the number of networks would be consistent with less fragmented and more connected networks. This sentence seems to say the opposite. Please elaborate. We agree with the reviewer that the original interpretation was unclear and that a decrease in the number of networks does not, by itself, indicate greater fragmentation. In the revised manuscript, we will remove the statement and will instead describe the result as a decrease in drainage density and mapped SCN number relative to 2018.

L456: This reads as saying that the mean depth became more uniform. I am not sure I understand this. Are you saying that the depth became more uniform spatially? If so, rephrase this and point to a different figure which quantifies this. In the revised manuscript, we will clarify that the distribution of optically inferred water-depth estimates became less variable across the mapped water features, as indicated by the narrower interquartile range and whiskers in the box plot.

L469-470: This phrase implies channels connect the GL and the ice-shelf front. This is not the case. Rephrase. We will rephrase this sentence and clarify the extent of the densest supraglacial networks.

Figure 12: The key is very confusing. I suggest using a normal colorbar. Typo in the caption. We will replace the key in Figure 12 with a standard colourbar, labelled with the channel-density metric and units, and we will correct the typo in the caption.

L490: "along the northwest direction" Awkward phrasing. In the revised manuscript, we will rephrase 'along the northwest direction' as 'towards the northwest'.

Figure 13: Do the colors match between the large panel, the key and the other panels? The thick green lines that extend the furthest downstream to the location of the large terminal lake do not appear the same as the lines in (e) and (i), which I think are intended to be the same color. We have checked the original figure and confirm that the same colour definitions were used for the

corresponding dates in the large panel, legend, and subpanels. However, we will revise Figure 13 using more distinct fixed colours, consistent line widths in the legend and panels, and a higher-contrast colour scheme to avoid ambiguity.

L507: 'Particularly the narrow tributaries': I am not sure what this is referring to. In the revised manuscript, we will clarify that this refers to the narrower upstream or lateral channels within the mapped SCNs that feed into larger downstream channels.

Figure 14b: The horizontal axis label should be latitude not longitude. We will revise the figure accordingly.

L574-576: I am not sure that I understand/agree with this statement. Comparing Figures 17a, b and d, suggests quite a bit of disagreement between the melt time series and the PDD time series.

For example, while all three peak then drop to lower levels, the PDD time series drops from near its peak significantly later than both the melt time series.

Can you expand upon this point, adjust the figures to make the comparison easier to see, and discuss the similarities and differences in more detail. In the revised manuscript, we will replace 'closely track' and 'confirming' with more cautious wording. We will describe PDD as a temperature-based melt-potential index that shows broad seasonal similarity to modelled melt, but also clear differences in timing, particularly the later decline in PDD relative to melt. We will revise the figure to make this comparison clearer, for example by adding a normalised time-series comparison, and we will expand the discussion of why PDD and modelled melt may diverge.

L622-624: Or could these backscatter values suggest a refrozen unit that acts as specular reflectors that reflect away the radar energy? Low backscatter may indicate smooth open water, wet surfaces, or refrozen/specular surfaces depending on timing and surface condition. Therefore, we interpret SAR backscatter cautiously and do not use it alone as definitive evidence of liquid water.

L639: Is it an ice rise? Or an ice rumple? It seems very small for an ice rise, which require a flow center independent of the ice-shelf flow. In fact, looking at figure 1, it seems unlikely that the flow speed goes to zero here, which is what is required to make this an ice rise. We agree that 'ice rise' was not the most appropriate term here, because the feature is relatively small and the available velocity field does not indicate an independent flow centre or near-zero flow speed, which would be expected for an ice rise. In the revised manuscript, we will replace 'ice rise' with a more cautious term, such as 'local topographic high' or 'possible ice rumple'. And we will revise the text to avoid implying the dynamic characteristics of a true ice rise.

L657: "Although individual channels do not evolve at the same rate": Does this mean they do not evolve at the same rate throughout the season, or as each other? In addition to this confusion, I am not sure how this phrase relates to the rest of the sentence. In the original manuscript, we intended to refer to spatially variable timing of channel activation and connection within the SCNs, rather than to a quantified rate of channel evolution. In the revised manuscript, we will remove or rephrase this sentence. We will also revise the surrounding paragraph to replace terms such as 'efficient', 'stable', and 'optimising' with observation-based wording, focusing instead on increased connectivity and network integration during peak melt.

L666: I do not think these are the most appropriate references for meltwater patterns on Larsen C Ice Shelf? In the revised manuscript, we will replace them with more directly relevant Larsen C meltwater/ponding studies, such as Luckman et al. (2014), and will rephrase the sentence more cautiously.

*Luckman A, Elvidge A, Jansen D, et al. Surface melt and ponding on Larsen C Ice Shelf and the impact of föhn winds. Antarctic Science. 2014;26(6):625-635. doi:10.1017/S0954102014000339*

L726-727: I am not sure how useful it is to be speculating on physical processes to explain a weak correlation which is not statistically significant. [In the revised manuscript, we will remove this explanation and simply report that the relationship between SF\\_14 and surface water area is negative but not statistically significant.](#)

L731: “less energy is required to melt snow than it is to melt ice” This is true for a given depth of snow vs the same depth of ice, but all quantities are in mm w.e. so I am not sure I understand this statement. In fact, fresh snow increases albedo, reducing melt compared to the same weather conditions over ice. This needs more discussion or removal. [We agree that this explanation was unclear and potentially misleading, particularly because the quantities are expressed in water-equivalent units and because fresh snow can increase albedo. We will remove this interpretation and shorten the discussion of this non-significant relationship.](#)

L760: I do not think this is the best citation for this sentence about how channel formation should precede lake formation. The paper cited here is focused on the opposite - lakes feeding channels. [In the revised manuscript, we will remove this interpretation and rephrase the sentence more cautiously to state that channels and lakes are hydrologically connected and may evolve together during the melt season.](#)

L767-768: This sentence seems to imply that channels operate all the time. In contrast, they must either empty or their water must freeze during the winter months. [In the revised manuscript, we will replace ‘continuously transport meltwater’ with more cautious wording, stating that SCNs provide seasonal pathways for meltwater redistribution during the melt season. We will also avoid using ‘drainage efficiency’ here and will instead refer to drainage connectivity.](#)

L772: I think this statement is based on an argument made earlier that I do not understand that snow is “less energy is required to melt snow than it is to melt ice”. This needs revisiting in my opinion. The argument should be elaborated on or removed. Given that the correlations this agreement is trying to explain are found not to be statistically significant I think this can mostly be removed without losing much of value. [We will remove this interpretation and shortened the discussion of this non-significant relationship.](#)

L793: “(>100%)” it is unclear what this means precisely. [In the revised manuscript, we will remove this percentage and rephrase the sentence to describe the interannual variability more clearly using the reported drainage metrics. Where quantitative changes are discussed, we will specify the relevant metric, comparison period, and baseline explicitly.](#)

L794: should interannual be seasonal? [This sentence will be revised.](#)

L798: “near the ice front”: This is qualitative. Personally, I wouldn’t consider any of the terminal lakes to be ‘near the ice front’ but it is subjective, so quantify this or remove it. [In the revised manuscript, we will quantify the distance between the large downstream lake and the contemporaneous ice front, rather than using the term ‘near’. Because this sentence refers specifically to the 2018-2019 seasonal evolution, we will report the distance for the largest downstream lake observed during that season.](#)

Data availability section: I strongly think these data should be archived and freely available, rather than available upon request. [We will revise the Data Availability section. The mapped SCNs and associated metric tables will be deposited in a public repository with a persistent DOI.](#)

Lai, C.-Y., Kingslake, J., Wearing, M. G., Chen, P.-H. C., Gentine, P., Li, H., Spergel, J. J., and Van Wessem, J. M.: Vulnerability of Antarctica’s ice shelves to meltwater-driven fracture, *Nature*, 584, 574–578, <https://doi.org/10.1038/s41586-020-2627-8>, 2020.

Spergel, J. J., Kingslake, J., Creyts, T., Van Wessem, M., and Fricker, H. A.: Surface meltwater drainage and ponding on Amery Ice Shelf, East Antarctica, 1973–2019, *J. Glaciol.*, 67, 985–998, <https://doi.org/10.1017/jog.2021.46>, 2021.