The authors of the manuscript “Long term analysis of cryoseismic events and associated ground thermal stress in Adventdalen, Svalbard” performed a study on a large temperature and seismic database of Adventdalen valley on the island of Spitsbergen, gathered in the past two decades. The seismic data are then evaluated using STA/LTA and MFP approaches to a) filter out cryoseismic events and distinguish them from the mine activities and b) figure out the activity source location. The spatiotemporal temperature data are used to compute the stress history at different depths of the ice layer. Elastic, thermal and viscous strains drive the stress calculations. A simple fracture model is used to predict the possible cracks and cryoseismic events and compare them to the recorded seismic data. The authors concluded that there is good agreement between the model predictions and the recorded data.

In my opinion, the current manuscript lacks enough novelty and depth to get published in The Cryosphere journal. I do not have enough expertise to judge the MFP calculations section, but I hope the comments I made for the thermal stress and fracture sections help the authors to elevate the existing manuscript to The Cryosphere journal-level quality.

We disagree that the manuscript lacks novelty. The relatively recent publications by Okkonen et al. (2020) and Podolskiy et al. (2019) are perhaps the closest in scope and were an important inspiration for this study. However, the present manuscript diverges in numerous significant aspects from these previous studies, particularly with respect to the use of a measured rather than modelled ground temperature record and the use of a large catalogue of thousands of individually detected and located events spanning many years. The result of these fundamental differences is that the degree of overlap with previous studies is quite small.

We do appreciate the feedback and have used the review comments as a basis to identify a number of improvements that can be made to the revised manuscript.

The Introduction is not coherent. I could not find a clear bridge between paragraphs, and also the relation between written paragraphs and the paper’s goal is not clear to me.

We will revise the introduction to improve the bridge between paragraphs in the revised manuscript.

Figure 2 needs more description. I assume each sub-plot corresponds to a certain year; you need to show that in the figure or caption.

In response to this and a similar comment from RC1 we suggest replacing Figure 2 with the following updated version.

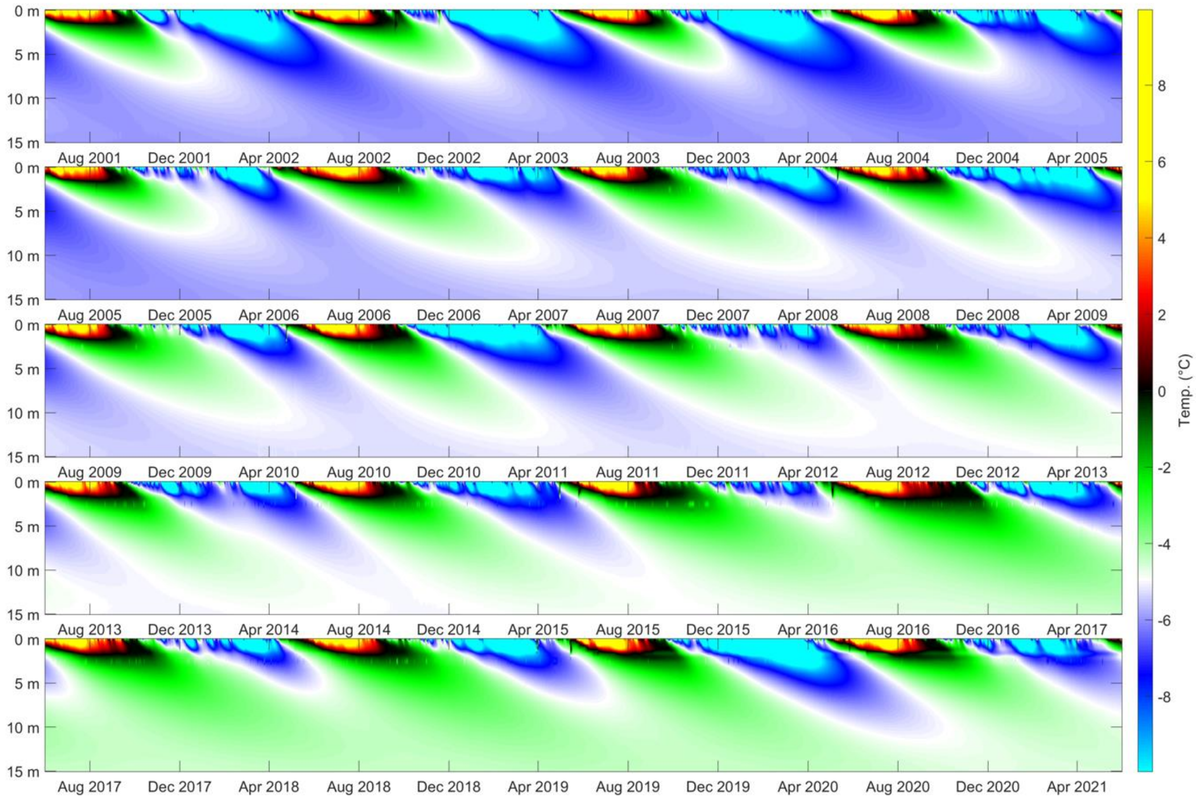

Figure 2 – Illustration of spatiotemporal borehole temperature recorded at the PACE P11 borehole on Janssonhaugen. A long-term warming trend is observed below the active layer that is subject to seasonal freeze-thaw. The continuous timeline is split across multiple figure panels.

Figure 3: It would be nice if you zoom in into one of the detected events for better clarity of your method.

We suggest including the following updated version of Figure 3 in the revised manuscript.

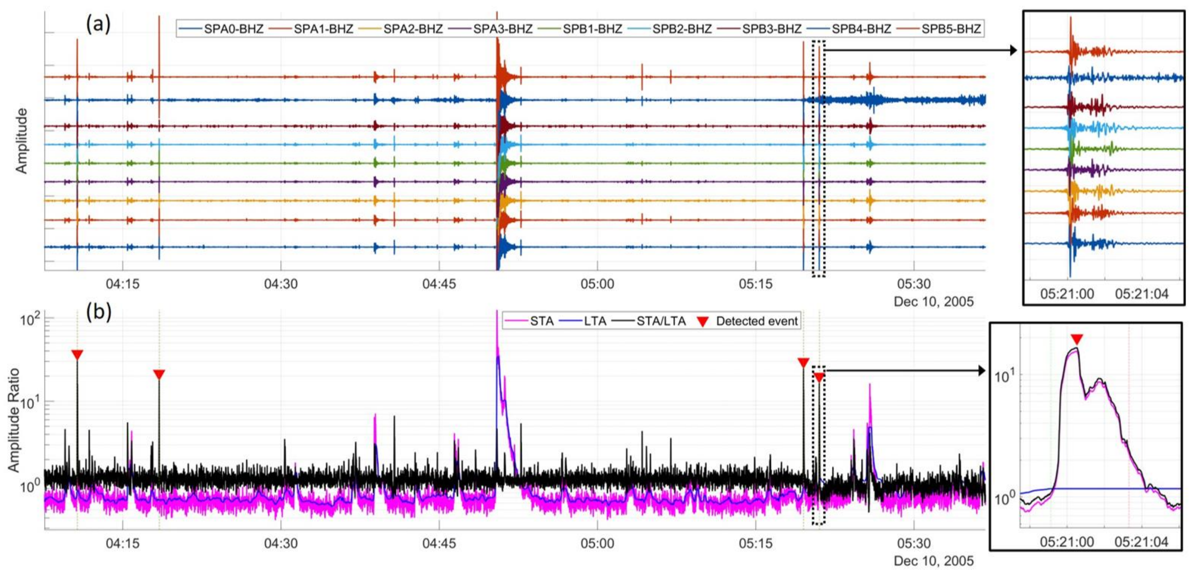

Figure 3 – Example of (a) timeseries of vertical component ground motion and (b) event detection using the STA/LTA (short term/long term average) detector. Short duration events with sufficient amplitude and array coherence are selected while longer events such as the high amplitude example at 04:50 are ignored. Inset boxes show detailed views for a specific detection.

Figure 6: It is hard to distinguish differences between seasons only by checking these contours. Adding numbers to either image or in the caption would help readers to notice the fluctuations across seasons.

This is a good suggestion; we can add the number of events corresponding to each subfigure. We will also do the same for Figure 5 for consistency.

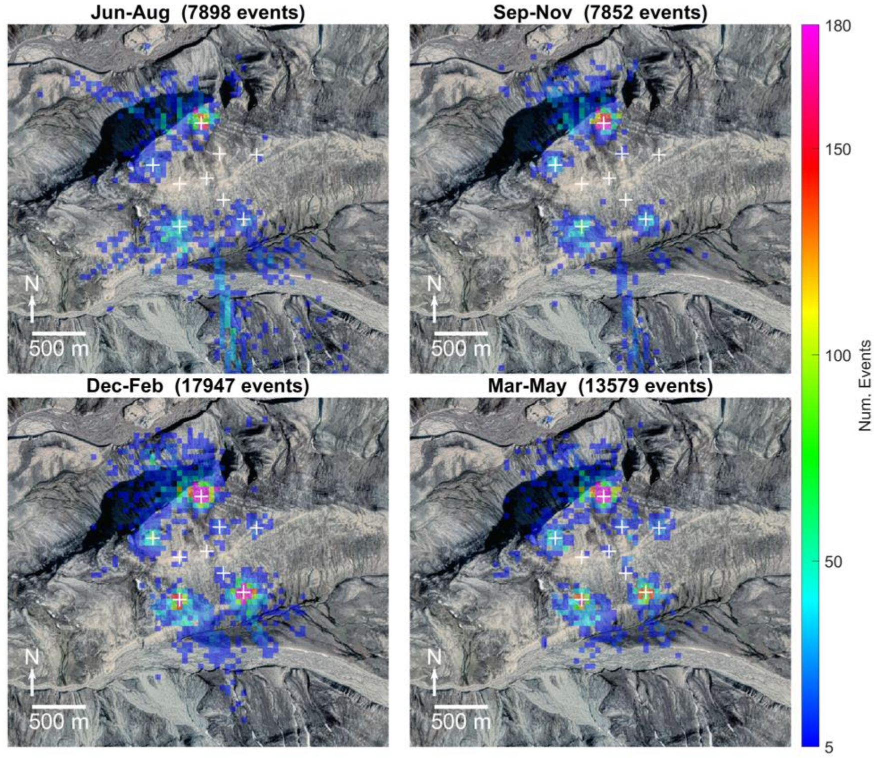

Figure 6 – Detail view of Janssonhaugen overlaid with spatially binned (50×50 m) distribution of coherent MFP inferred seismic source positions plotted by season for events recorded between August 2004 and July 2021. Positions of SPITS seismometers are indicated by white crosses. Orthophoto © Norwegian Polar Institute (npolar.no).

Page 16, 325: Your justification here to exclude summer-autumn events from your study does not seem sufficient to me. I am looking for better justification in the rest of your paper…

This seems to be related to a misunderstanding based on a poor choice of words on our part. To clarify, summer-autumn events are not excluded from the study. Note that figures 5, 6, 9, 10, 11 & 12 all include summer and autumn seasons. A specific, local, spatial domain is excluded from the spatially delineated Class I event cluster. This domain corresponds to a river valley that is particularly active during the summer-autumn. InSAR results have also confirmed that this area is highly dynamic in the summer (as cited in the manuscript). This activity is likely related to processes of fluvial erosion and river bank oversteepening, rock glacier movement etc., which do not belong to the same class of events as those which we interpret as cryoseisms. Put another way, event Class I was isolated using a simple radial distance cut-off of 1500 m from the centroid array, excluding the river valley south of the array that overlaps this zone (where another, minor event class resulting from different dynamic processes dominates).

We agree that including the phrase "summer-autumn" to describe a spatial cluster, was unfortunately rather ambiguous. To improve clarity, we suggest rephrasing from:

> "By selecting the subset of events with inferred source positions within ~1500 m of the array centroid and excluding the cluster of summer-autumn events south of the array, we isolated a total of 42,432 class I events recorded between July 2004 and July 2021."

To the following:

> "By selecting the subset of events with inferred source positions within ~1500 m of the array centroid, excluding the river valley/rock glacier area south of the array, we isolated a total of 42,432 class I events recorded between July 2004 and July 2021."

Page 16, 330: Again, the justifications in this paragraph are not enough and lack scientific statements. At least, I as a reader, expect to know what type of data you need to draw a more accurate conclusion.

No direct conclusion is to be drawn here; we are simply delineating the spatial extent of the cluster of seismic events that we categorise as event Class I and the broad seasonality associated with this cluster. In response to RC4 we will add a more detailed interpretation of the anomalous seismicity of the three identified areas so that the following sentence, that this comment relates to, will be removed from the revised manuscript:

"These areas may be associated with enhanced ground heat loss, thin or absent snow cover or elevated ground moisture/ice content (e.g. Abolt et al., 2018; Matsuoka, 2008), though we lack the field observations necessary to support this explanation for the anomalous seismicity of these areas."

Figure 8: I suggest reducing the legend of the plot to -0.5-1.5 for better contrast. I do not see values below -0.25 in the contour plots.

The observation that the values mostly lie within the range -0.5×10^7 Pa to 1.5×10^7 Pa is correct. However, for readability it is very convenient that zero stress is white. One can observe that we have assigned a range of colours to the positive range of stress, while the values below $\sim-0.25\times10^7$ Pa are uniformly black (so figure contrast will not be affected by the suggested change). It is desirable to convey that the magnitude of tensile stresses associated with thermal contraction during periods of cooling far exceed the magnitude of stresses associated with thermal expansion. The included colour scaling also makes clear this asymmetry.

Section 4.2: What are the initial and boundary conditions for solving equation 12?

This is a first-order differential equation with respect to the time variable, so we only need an initial condition. There are no boundary conditions for first-order temporal models. The initial condition is stated on line 242 of the manuscript:

"In order to solve Eq. (12) for $\sigma(z,t)$, we specify the initial condition $\sigma_0(z) = \sigma(z,t = 0) = 0$."

Page 17, 345: I do not understand how you associated the 20-30cm regolith to the peak stress in the ice above it. How the peak stress in the ice could lead to high stress in the rocks beneath it?

In the borehole we have temperature measurements from sensors installed at 0.2, 0.4, 0.8, 1.2, 1.6, 2, 2.5, 3, 3.5, 4, 5, 7, 10, 13 & 15 m depths. We also have a record from 0.1 m depth at the Janssonhaugen Vest meteorological station. Of all of these temperature records, the largest thermal stress is associated with the 0.2 m deep temperature record (as shown in Figure 9-b). Cracking is most likely to occur where the stress is highest. The regolith layer at Janssonhaugen is 20-30 cm thick. This gives an indication that thermal stress weathering/cryoturbation may be an important control on regolith depth when allowed to act over a long time. Aggradation of weathering material would be another explanation, but this less likely the case since the top of Janssonhaugen is a mountainous plateau where erosion is expected to dominate over deposition. If we had modelled the largest thermal stresses at a depth of 1 m, for example, we would expect the ground to be heavily fractured to this depth and that over thousands of years a 1 m regolith layer might be formed.

We can add the key details from this discussion to clarify this in the revised manuscript, but will not include the previous paragraph in its entirety for the sake of brevity.

Figure 9: I am interested to see the contribution of each strain portion (elastic, thermal, viscoelastic) into the total stress where ever you report the stress value (Figs 8-11).

These components are interconnected through a differential equation (Eq. (12)), so one cannot simply decompose the resulting total stress into separate components in an additive manner.

Page 20, 395: This paragraph suits better in the conclusion section.

We think it is important to point out that the spatial variability of the subsurface temperature field is not constrained by the borehole temperature measurements used in this study, as this paragraph discusses. However, we don't think this topic fits as a main conclusion of the study.

Section Conclusion: This section is better to be named Summary rather than Conclusion. To enrich your paper's conclusion section (which should be the most important section) I suggest discussing pros/cons of your thermal and MFP model, potential improvements of your work, and maybe possibilities to apply your model to other geographical locations…

It is a perhaps a stylistic choice, but we prefer a brief conclusion summing up the most important results of the study. The possibility to apply the study methodology to other geographic locations doesn't need to be stated explicitly, but we can add the detail that future calibration experiments using controlled sources in known locations would improve the utility of SPITS for MFP studies.

I am curious if you noticed any pattern in the recorded quakes for daytime versus night times (heating vs. cooling periods)?

Janssonhaugen is situated on Svalbard in the high Arctic. Here the polar night, where no shortwave solar radiation is received at the ground surface, lasts from around 1-Oct to 28-Feb each year. During the summer, the sun does not set between 19-Apr and 23-Aug and solar insolation received at the ground surface also depends on local factors like snow cover and topography. We certainly observe that frost quakes were more frequently recorded during the polar night than during the period of midnight sun. It is, however, impossible to generalize that day and night correspond to periods of heating and cooling in the high Arctic if one assumes the typical definition of day and night as representing ~12-hour phases in the diurnal cycle. Interestingly, the diurnal temperature range on Svalbard is actually greatest during the winter (Przybylak et al., 2014), despite the complete absence of solar irradiation. This is explained by the intensity of winter storms and the advection of warmth to the region, driven 95% by atmospheric circulation and 5% by oceanic circulation (e.g., Bednorz, 2011). The complexity of the surface energy budget in this region (e.g., Westermann et al., 2009), further reinforces a key strength and novelty of the manuscript, i.e., that we utilize measured ground temperatures rather than modelling ground temperatures based on measurements of air temperature. We will add some details about insolation and the importance of synoptic weather systems in driving temperature variation to the description of the study area in the revised manuscript.

Bednorz, E. (2011). Occurrence of winter air temperature extremes in Central Spitsbergen. *Theoretical and Applied Climatology*, *106*(3), 547-556.

Przybylak, R., Araźny, A., Nordli, Ø., Finkelnburg, R., Kejna, M., Budzik, T., ... & Rachlewicz, G. (2014). Spatial distribution of air temperature on Svalbard during 1 year with campaign measurements. *International Journal of Climatology*, *34*(14), 3702-3719.

Westermann, S., Lüers, J., Langer, M., Piel, K., & Boike, J. (2009). The annual surface energy budget of a high-arctic permafrost site on Svalbard, Norway. *The Cryosphere*, *3*(2), 245-263.