Anonymous referee #1

Referee: The explanation of the HVSR makes more sense now. If I understand correctly, the previously detected tremor had a distinctive high HVSR, so this distinctive feature was used to find other instances of frost tremor. That makes sense regardless of the reasons for the high HVSR. Furthermore, the authors' explanation of why the HVSR change is inconsistent with a change in the subsurface medium makes sense to me.

However, I am still confused by the elliptical polarization shown in Figure 9, which is identified as being typical of tremor. This figure appears to show larger vertical than horizontal amplitude. That would be inconsistent with the high HVSR and horizontal polarization indicated by Figure 3 and the associated text. Can you clarify this apparent discrepancy?

Authors: In the pilot experiment in 2019 we made observations using a single 3-component station. We just found such long-term trend in HVSR behaviour and dependence on temperature and this motivated us to make a new experiment. However, Figures 3 and 9 cannot be compared directly, as they correspond to different time scale. In Figure 3 we show a general trend in HVSR during a long time period, while in Figure 9 we show just a single example of frost tremor from the swarm composed of overlapping events. In that particular case the polarization is vertical, indicating that the event is close to the station. This polarisation can correspond to P-wave (see also the Figure 6 that shows that near the source we record mainly body waves). Such polarization in the near-field zone for this particular event is in agreement with evaluation of radiation pattern from the surface tensile fracture opening in ice (Dudko, 1999), although other modes of fracturing are also possible. Concerning the observed HVSR maxima at high frequences in Fig.3, it shows the HVSR calculated from overlapping signals from multiple sources arriving to the station from different distances. In the case of relatively large distances, the main energy of the signal would corresponds to surface waves and the energy from these sources is dominating in the signal recorded at this particular station. This is in agreement with our present result, in which we found multiple events originating from wetland located at distance of 300 m from the site of our 2019 station. However, we are not stating that the high HVSR in Fig. 3 is purely due to frost tremors, as it would be too much for interpretation of single station observations. The explanation proposed in Dal Moro (2020) that the HVSR is increased due to appearance of higher mode Love waves cannot be excluded. We have modified the text to clarify the difference in polarisation (lines 119-134 and 336-348)

Referee: Finally, in the text added on line 125 of the manuscript with tracked changes, you refer to "winter 2019-2019". Should this say "winter 2019-2020"?

Authors: The typo has been corrected

Anonymous referee #2

Referee: The authors have answered most questions raised by the other reviewer and myself, but they did only minor changes in the manuscript.
I think that questions raised by the reviewers maybe be the same as interrogations of the readers, and many answers should be included in the manuscript.

Even negative results (eg, absence of correlation with temperature) should be included.
For instance, I really think that showing a spectrogram of the signal is interesting, at least as supplementary material.

Authors: We have calculated spectrograms at the initial stage of our study, when we processed the data of our pilot experiment in 2019, but they really are not very informative, as the site in Talvikangas is located in urban area. So the specrograms are just dominated by typical diurnal variations of anthropogenic noise obereved at numerous urban sites around the globe (see, for example, Steimann et al., 2021) and no any distinct correlation with temperature are seen there. The correlation with temperature we noticed only at the HVSR plot, that is why we decided to include namely this result into the manuscript, in order to avoid overloading the text with non-informative figures. We would like to notice that the main scope of the paper was not to study seasonal variations of properties of the subsurface, but the fracturing in the subsurface during short-term rapid air temperature drop and associated hazard. We added some explanation concerning this to the text (lines 128-135)

Referee: The authors did not answer some suggestions: eg, looking at direction of first movement or computing magnitudes, saying that this is left for future work.

Authors: During our work on the manuscript we had to make some prioritisation what to include into the manuscript not to make it too long. That is why we decided to concentrate mainly on events locations and classification and on evaluation of associated seismic hazard that can be evaluated directly in terms of ground accelerations recorded in the near-field zone. By this, and by selection of the Cryosphere journal, we were trying to broaden our target auditorium and to make our paper interesting also for specialists studying extreme weather conditions (like sudden cold waves of various scales) and their effect on urban infrastructures and for geotechnical engineers dealing with operation of these infrastructures in cold climate. The latter specialists are mainly used to work with ground accelerations and ground velocities. This was also motivation why we decided to consider the episode of 6.01.2023 when extreme rapid temperature drop was observed simultaneously at two of our sites located at distance of about 250 km (see Fig. 5). Another motivation were concerns of local people in Talvikangas, who heared the noises during that period and noticed direct damages to roads etc.
We definitely will make analysis of records of frost quakes and frost tremors in details (including magnitudes, the first motions for those events where they could be analysed) for the whole experiment period and prepare more seismologically oriented paper that we are going to submit to the more seismologically oriented journal. But this need to be a separate work. The current mansuscript aims to report our observations about new type of seismicity originating from wetlands and also to evaluate associated seismic hazard.

Referee: What I just realized reading the revised manuscript, is that they recorded 6 months of data but only analyzed one day of data (2023/1/6) corresponding to the main temperature drop.
The analysis of the full dataset is left for future work. I personally don't like this practice of cutting work in many papers.
The authors did a good job at identifying different types of events (quakes and tremor) and locating them.
But the problem is that the origin of these events is not clearly established.
With only one day of data it is hard to establish a correlation between quake events and temperature drop.

They have shown that many events are detected on that day (2023/1/6), not that this seismic activity was particularly large.
The authors need to be much more cautious when interpreting the source of these events, or better, to analyze the data for a much longer time period.

Authors: We partly answered to this comments in our reply to the previous question. Our aim was to consider the particular extreme weather episode of rapid air temperature drop. The episode was of regional scale, as the same process was observed at two our observation sites (see Fig.5). In our Fig. 13 it is seen that the events appeared namely during the period of rapid temperature drop. However, our previous study (Okkonen et al., 2020) suggests that there is no direct correlation between extreme low air temperatures and large activity of frost seismic events, as the latter is depending on thermal stress in uppermost subsurface that, in turn, is depending not only on air temperature variability, but also on other factors (in particular, on snow depths and soil moisture, which are varying during winter period in our geographical area). In our next paper, which is now under preparation, we shall present results of thermal stress modelling for the whole observation period, but we prefer to publish details of seismic signal analysis and events detection algorithms in a separate paper. It is really not possible to put everything in a single paper.

Referee:
Details comments

The quake detection method is not described.
Signals are stacked on all sensors of the network, but how are these stacked signals used to identify quakes?

Authors: We processed the data manually, using visual inspection of seismograms. After location of some events we noticed that they are originating from wetlands and analysis of these interesting events cannot be just delegated to authomatic detection algorithms. We were picking arrivals manually and calculated source locations, using information about P and S waves arrivals and velocity model shown in Fig. 4a. In addition, visual inspection of seismograms helped us to recognise tremor-type signals and to develop an algorithm for their detection.

Referee: Rate of tremor events
The detection method is not clear. You analyze a time window of 0.8 s, but what is the time shift between successive windows?
Is there some overlap? If so, how do you remove duplicate events?

Authors: We used time shift of 0.3 s and 10% overlap. In case when events has been detected, we shifted time window to full its lengt (0.8 s) to avoid “double detection” of the same event. The correspondent text is added.

Referee: Location of tremor and quakes
The location of quakes is shown in Fig 7 and tremor are shown in fig 12.
It would be good to merge these figures in one pin order to allow a comparison of the location of quakes and tremor.

Authors: We illustrated these results on different figures to avoid overloading of a single figure with information. For us it was also important to show location of roads and irrigation channels. If all the sources (tremors and events) are in the same plot, some of source locations of tremors can be invisible in that case.

Referee: Polarization
I still don't understand the sentence in section 2.
"The results of our earlier observations (Okkonen et al., 2020) suggest that the seismic signal excited by fracturing in the uppermost soils would have mainly horizontal polarization, as the main fracturing mechanism in that case is vertical fracture opening. "
Do you consider only P waves? Could you add a reference?
Your figures 6 (quakes) and 9 (tremor) show on the opposite larger amplitudes for the vertical components.
Could you clarify this point?

Authors: In our original study of frost quakes swarm in 2016 (Okkonen et al., 2020) we had only recordings of frost events by single permanent station OUL located at distance of 14 km from the area in Talvikangas, where multiple fractures at the surface were formed as a result of vertical fracture opening (so we were just seen these fractures on the sutface). At such distance (e.g. in far-field zone) the main signal that could be identified was from surface waves, and body waves were not visible in seismograms. These “ground-truth events” and the waveforms recorded by OUL stations were the background of our suggestion that we need to search the waveforms with similar polarization. When processing the data of our experiment in 2022-2023, we found also body waves, in particular in near-field area that have different polarisation (see Fig. 6). See also our reply to the comment of the Reviewer #1 concerning different polarization.
We agree that this sentence is misleading and we modified that part of the text

Referee:
Source of tremor
In section 4.2 you write that "Comparison of waveforms of such events with that of the 245 single frost quake described in Okkonen et al. (2020) suggests the similar source mechanism, namely, vertical fracture opening"
Could you give more details? Which characteristic do you consider?

Authors: We used shapes of these signals. One of the features we used is absence of clear P and S arrivals as well as the higher horizontal amplitude than the vertical. Of course that is not always true, as we found later, analysing tremors (figure 9). But it was our original hypotesis, based on our pilot studies. We used this when experiment planning and on the first steps of data processing.

Referee: The fact that the signal has an elliptical polarization suggests that it is dominated by Raleigh waves and thus is rather shallow.
But why do you suggest that the source is vertical fracture opening?

Authors: This is our original hypothesis, made based on our previous studies and from direct observation of fractures on the surface. Dominated Raylegh wave might be because of propagation effect. We think that our hypotethis was reasonable taking into account that we had only single station records in our pilot experiment. This hypothesis proved only partially in the research, described in the current manuscript and we modified the text to explain this.

Referee: Fig 9 shows 30s of seismic signal. One tremor event is highlighted.
Is this the only event detected during this time window? This is surprising because this does not correspond to peak amplitude!

Authors: In this figure we show a typical swarm of tremors and we highlighed the parts of records, which we used for particle motion diagrams. This is just an example. There was no peak amlitude, but high correlation coeffcient (see description of our location algorithm). Practically tremors usually have been recorded as swarms of overalapping single impulses, but not like a single event.

References

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