The comments from the editor are shown in blue text and our responses are provided underneath in black.

Based on my own reading of the manuscript, I also feel that the corresponding descriptions of the main text (Section 3.3) might be unclear: i.e., the thermal stress model explanations do not explicitly state what was the material model or its geometry. The temperature profile is considered down to 15 m, which covers the active layer and the permafrost (thus, it is written in Line 240: "we model the frozen soil"). Before that, it has been mentioned that there is a sediment crust on top of sand-/siltstone bedrock (Line 125).

We have clarified this so that it is clearer that we ignore ground layering and apply the simplified representation of the ground as a homogeneous viscoelastic solid. From the available descriptions of Janssonhaugen and the Ullaberget Member sandstone we assume that the elastic properties of the bedrock may not be so different to the regolith, at least when the ground is frozen solid during the winter. Lacking detailed mechanical test data that could have provided the additional data needed to constrain a layered ground model, it therefore seemed most reasonable to simply assume a homogeneous solid (containing planes of weakness in the form of ice veins/wedges). The Young's modulus is in the range we might expect for either frozen regolith or soft sandstone and we assume it depends primarily on the degree of ice saturation, which increases with increasingly negative temperature. An additional point of the assumed homogeneous viscoelastic solid is that we can speculate that cryoturbation processes may play an important role in forming the regolith layer, whose depth corresponds approximately to the peak modelled thermal stress. If we assumed a softer regolith crust overlying bedrock we would enhance the thermal stress peak at the interface depth, but would lose the ability to connect that the depth of the regolith may be explained by cryoturbation processes.

However, the reader is informed about the viscoelastic rheology without learning how the heterogeneous double-layer structure was simplified in the first place (or without clarity about what were the materials of such frozen soil, which is apparently a composite material). This is particularly confusing since Table 1 is a mix of parameters corresponding (primarily) to non-composite materials, especially as it concerns the viscous parameters. Please clarify these aspects, perhaps, by mentioning how your rheological model relates to Fig.1.

This is a good point and we have clarified that we imposed the simplification of assuming a homogeneous viscoelastic solid containing planes of weakness in the form of ice veins/wedges. Lacking detailed mechanical test data from the bedrock and regolith at the study site, we think that accounting for the double-layer structure, while entirely possible, would simply add an additional degree of uncertainty without adding significantly to our ability to explain the seismic observations.

In addition, I am not sure about the logic flow in Section 3.3.1. In the beginning, the fracture of frozen soil is discussed but a reference is immediately made to the tensile strength of pure ice (Table 1). Only at the end, it is added that actually, the aim is to model ice cracking, i.e., not of frozen soil, which are two different materials.

You're right, we found that the logic flow could be improved by changing the structuring of the paragraph so that we start with the statement of what we seek to model.

Finally, please also consider the following technical corrections from my side.

Thanks for these, we have made all changes suggested.