Thank you to the reviewers for your feedback and suggestions. Please find the response to your comments in blue.

Reviewer #2

The paper "Modelling of temporal and spatial trends in soil conditions in Finland using HydroBlocks model" uses outputs from a land surface model to estimate thermal stress in frozen soil to analyze the co-occurrence with frost quakes in Finland. The analysis includes comparing model outputs of snow water equivalence, soil water content, soil temperature and soil ice content between the model and observations for several sites in Finland. The paper is well written and has a logical organization that makes it very comprehensible. The subject matter is interesting and provides a unique application for land surface model outputs. The paper can be approved by addressing inconsistency in terminology, revising several plots, and strengthening the conclusions. Specific comments for revisions are given below.

Revisions:

Line 48: There is a lot of text devoted to the description of the thermal stress and even including an equation for thermal stress. This made me think that the analysis would be analyzing thermal stress in the soils. However, this paper is really about validating a land surface model for the potential use of estimating thermal stress. It could be helpful to cut back some of the text describing the thermal stress and focus more on how land surface models estimate the key inputs needed for thermal stress.

We agree with this comment that it is not necessary to go so deeply into describing thermal stress at this point. The reason for including the equation is to establish a connection between thermal stress calculations and using a land surface model; the need to obtain temperature distribution in the soil and frost depth. The overly detailed description of frost quakes will be removed from this paper.

Line 67 – Here you first introduce soil moisture content (SMC) and soil water content (SWC). It is not clear what the difference is here and is likely a model specific designation and sometimes the two are used interchangeably in broader hydrology. The readability of the paper would improve if you define specifically what you mean by SMC and SWC and how the two are fundamentally different. Also, from later analysis it is clear that what you really want is soil ice content, so it may make sense to start with that and describe that first.

We agree that the paper would benefit from a clearer description of these. The model outputs the total soil moisture content (SMC), which comprehends both the liquid and frozen water in the soil. It also outputs the soil water content (SWC), which is the liquid part of the soil moisture, and inherently there is also the soil ice content (SIC) which is the part of soil moisture that is in frozen form. For frost quake research, it is true that we want the soil ice content. The reason why we are focused on the soil water content, is the observational soil stations which, in addition to soil temperature, measure soil water content. We will make the distinction clearer by only focusing on SWC and SIC, which are relevant for this paper.

Line 72: The word paring "multiple different" is redundant and should be changed to either "multiple" or "different".

Line 78: "soil water equivalent" should be "snow water equivalent".

Line 322: To help avoid confusion, make the column headers Table 4 consistent with how they are described in the paper and the caption (i.e. SWC-5cm, not W5cm). Same for Table 5 and 6.

Thank you for pointing these out, the wording of these three points on lines 72, 78 and 322 should and will be changed accordingly.

Line 105: It is unclear what the sentence that starts with “The boundaries of the subdomains” is trying to communicate. It would be helpful if this was revised.

We agree this can be difficult to understand solely in a written context. To make this more understandable, the next sentence in line 105 will be followed by an additional sentence and reference to figure 2b.

…*The boundaries of the subdomains are adjusted in a way that no watershed is split between polygons, and they only belong to the polygon where they fall the most (Figure 2b).* This becomes more evident in figure 2b, showing the clusters of watersheds, where the watersheds defined by the model exceed the boundary (red square) of the subdomain.

Line 276: Figure 7 adds very little information that cannot be assessed from Table 3. In particular, the scale of the x-axis is such so that it extremely difficult to see differences between the models and the observations. One way to improve this is to show a short time period for the gauge that does the best (Vaala) and the gauge that does the worst (Konnevesi). This will give the reader more insight as to why the model performs well at one location and not so well at another.

This is a great comment. Showing additional shorter time periods for the best and the worst SWE stations would also help to see the effect of snow cover melt time to soil temperatures. The reason for using longer timescales was to show the amount of observational data and modelled decades to better see the variability between years and stations. The paper will benefit from showing shorter time periods as well.

Line 326: As mentioned above, the description of SMC, SWC, and SIC is very confusing. This seems to be more of nuance from the specific modeling framework. It would be clearer if in the methods section you say that you use SMC and SWC from the model to get SIC. Then just treat SIC as another model output. Given that most of the analysis is focused on SWC, then you can remove SMC from the paper entirely. This could help clean up the terminology and make the analysis clearer.

Yes, we agree that this terminology is overly complicated and SMC could be removed completely, leaving only the liquid and frozen water contents to discuss.

Line 392: Conclusion 5 is not really a conclusion. It just uses two outputs from the model to create an output that the model doesn’t current output.

Following the above suggestion of removing SMC and describing only SWC and SIC, this will be re-worded to focus on how the model gives us a country wide estimation of frost depth over the last two decades.

5. Additionally, SIC gives estimation on the frost depth and its spatial coverage across Finland, hence giving information on winter soil conditions over the past decade.

Line 394: Conclusion 6 is too broad for what is analyzed in this paper as there was no analysis of thermal stress or predictability and risk of frost quakes. This paper did validate the inputs need to calculate the

thermal stress and demonstrates that there could be potential for calculating thermal stress and frost quakes, but it does not actually validating the model against observed thermal stress and predicting frost quakes. Rewording this to focus on what was shown in the results would be beneficial.

The sentence in the line 394 should be reworded for example as follows:

6. Furthermore, the outputs from HydroBlocks, soil T and SWC validated against observations and SIC, show potential to be used to calculate thermal stresses following Okkonen et al. (2020) in soils across Finland and further to estimate the risk of frost quakes.