Reviewer 1

C: In many places the revisions have substantially improved the work. The reorganization of the text and enhancements to the figures are most welcome. In other areas, the revisions fell short and were somewhat cursory. There is still not really a science question or hypothesis. Key conclusion content is left unsupported by content in the body of the manuscript. Interpretation of data in terms of physical process explanations and comparisons between the study plots is limited. As reviewers commented during the first round, the manuscript still reads much a like a report rather than a scientific manuscript explaining how a cryosphere process works.

The science question remains weak. It appears to be: "We therefore seek to unravel how much, where and when liquid water can be detected alongside the factors contributing to liquid moisture availability." Therefore, the question appears to be 'How much water can be detected?' This does not seem to lead to the compelling investigation of an environmental process. Furthermore, a hypothesis still is not present. The preceding sentence in the introduction describing the aim of the work appears ripe for designing an interesting comparative experiment, however a hypothesis exploring the cryosphere processes within the scope of measurements is not proposed.

R: We thank the reviewer for acknowledging the improvements we have done. Our work has been improved so that it does not read like a report, as was acknowledged by the other reviewers who had no further comments. The comparisons between the study plots are everywhere throughout the manuscript. We are not sure why the reviewer sees them as so weak in substance whereas we see them as quite conclusive in terms of what they are trying to show. In the light of this we are trying to push forward the following hypothesis (line 127): "we aim to test whether, in the context of our study site, the hydrothermal profile of sediments changes in relation to physical properties, location and topography, and if, with increased time since deglaciation, sediments exhibit a more gradual freeze-thaw transition and a slower melt water infiltration." We have added and edited portions of the text (e.g., Line 508-512, 603-618, 718-727) to reflect this and supported it with relevant explanations of the underlying processes. We believe that sediments deglaciated for a longer period of time have a higher thaw window during the summer season.

Finally, if the reviewer could be more specific about what conclusion content is unsupported, we would be happy to adjust it accordingly. Nonetheless, we have made adjustments to our conclusion section in order to better reflect our results and interpretation.

C: Line 325 – 350 & figure 8: This is a nice analysis, however it is stated earlier in the methods that the VWC probes are not actually calibrated to in-situ soils but rather use the factory calibration (Line 153). This being the case, what value is there to 'calibrate' the ERT data to the VWC data that are not tied to any reality at the site?

R: We understand the reviewer's concerns. However, we consider that the factory calibration used is generic enough to cover the type of sediment observed on our sites in Svalbard. We have now mentioned this as a potential source of error.

In figure 8 and related text we associate PRIME electrical resistivity measurements acquired independent of the point sensor measurements of volumetric water content. We believe it is

indeed a site-specific pedophysical relationship because properties of the sediments reflect in the shape of the Archie curves.

C: Line 447-453: The purpose of this text is not clear. What argument is it attempting to support? What explanation for the observations is provided? Referencing is inadequate.

R: The purpose of this paragraph was to discuss the importance of our findings, i.e. there is a difference of 13 days between the melt onset at the two different sites. We think there is a clear indication of variability in water availability across the forefield which consequently impacts the rate of microbial activity and ultimately soil evolution. These findings/timeseries can feed into future biogeochemical models. We have now rephrased the paragraph to make its message clearer.

C: Line 484: Is the boundary between active layer and permafrost at these sites known from other measurements? Could this be compared to the ERT data? Why would the 'vertical resolution" prevent observation of this transition? CALM data around Svalbard seem to indicate that ALT on the island can range from just over half a meter to in excess of 2m, so it is plausible that the entire ERT volume is in the active layer, if permafrost is present at all.

R: We understand how the sentence might be confusing. Yes, the interface between the active and the permafrost layer is known from other sources, which we considered beyond the scope of this work. We have now reformulated the sentence in order to remove ambiguity.

C: The analyses of the k-means clustering remains limited. While it is claimed that the three identified zones represent similar dynamics within each zone, the timeseries (Figure 12 b1 b2) cast doubt on their being any significant differences between the zones within each site. It is claimed on Line 551 that "The clustering algorithm provides an unsupervised method of selecting regions of the model that are more or less dynamic." However, there is no evidence of this in the timeseries (dynamic) data in figure 12. All three clusters at each site appear to have roughly the same scale of dynamics, though small variations in timing may be apparent.

R: Timeseries in figure 12 show the evolution of average resistivity values per cluster over time. We believe the timeseries do not overlap and are distinct enough, therefore showcasing differences between clusters. In our results and discussion sections we tried to point towards these differences in text (e.g., Line 461-463: "For Site 1, clusters 1 and 3 show similar average resistivity values over time during the winter, whereas cluster 2 shows resistivity values that are lower in comparison", Line 712: "Site 1 Cluster 2 represents the region with the smallest change" etc.). Nevertheless, we have added new discussion text to provide more substance to our analysis (Line 718-727), focusing on how sediments at the two sites observed experience thaw transitions of different lengths and how this is reflected into the electrical resistivity variance data.

C: Line 539: contact resistance units should be in ohms rather than kW.

R: Now amended in text.

C: Line 565 – 566: This is misleading because as stated in the manuscript manufacturer equations were used for the VWC data, so no site-specific calibration has actually been made.

R: We responded to this comment above.

C: Line 575 -576: This statement about the "older" versus "younger" sediments is not argued in the manuscript text. It only appears in the abstract and the conclusions and no evidence is provided to support it. Why would older or younger sediments have this physical effect on water dynamics in the subsurface?

R: We now added a few more references to the age of the sediments in our latest attempt to supplement the discussion with a paragraph about sediment evolution rate.

Editor's comment on Reviewer 3

C: Reviewer 3 raised no further objections to publishing the manuscript but noted concerns about overlaps with a previous publication in "Permafrost and Periglacial Processes." To address this, I suggest incorporating your earlier response to this comment into the introduction of the revised version and clarifying the aim and findings of the previous publication.

R: The response to that comment was added to the introduction and the difference in aim clearly stated.

Other comments:

C: Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly.=>Fig. 8

R: Line styles in Fig. 8 were changed to accommodate color blindness.