RC1: 'Comment on egusphere-2025-962', Wojciech Dobiński, 19 May 2025
The work submitted for evaluation presents interesting and original research material obtained from field studies conducted using complementary ERT and seismic methods used for decades in studies on the occurrence of permafrost.

The area selected for the study is already very well-known from similar permafrost studies, of which a great many have been conducted in this area since the 1970s. Neither the choice of methods nor the choice of the area is therefore particularly original and rather fits into the trend of research conducted for many years.

ERT and seismic refraction are certainly the most traditionally used geophysical methods for characterizing rock glaciers (Hauck and Kneisel, 2008). However, the potential of the Multichannel Analysis of Surface Waves (MASW) has never been fully explored to characterize high mountain permafrost environments like those in the European Alps. To our knowledge, the only study on this subject published in the scientific literature is Kuehn et al. (2024), where, the MASW method only leads to the characterization of the rock glaciers' active layer. Additionally, Guillemot et al. (2021) used MASW in combination with other techniques to constrain a reference S-wave velocity distribution for the unfrozen conditions at the Laurichard rock glacier, with the real purpose of tackling seasonal variability using ambient seismic noise.

In our work, the MASW method plays a central role in fully reconstructing the structure of the Flüela rock glacier (active layer, frozen layer, unfrozen basal till, and bedrock). MASW overcomes the limitations of SRT, which does not reveal velocity inversions (thus, it does not allow to define the thickness of the frozen layer) and may not reveal the characteristic velocities of the frozen layer when, according to our interpretation, a thin saturated supra-permafrost layer exists. Consequently, relying solely on ERT and SRT models could challenge the interpretation and understanding of the rock glacier structure. On the other hand, MASW provides more reliable results, leading to a more accurate model of the subsurface.

Our novel and original findings, open new perspectives on the possible use of MASW for the permafrost characterization in rock glaciers. Through this work, we aim to encourage the mountain permafrost research community to collect active seismic data using low-frequency geophones (4.5 Hz) whenever possible. Although this data is typically used for refraction analysis, recording it with low-frequency sensors also enables the application of the MASW method. This dual use can help minimize interpretative ambiguities or even improve the overall interpretation, as we successfully demonstrated in our case study.

The article can be divided quite clearly into a part concerning permafrost and a part concerning methods. The authors focus strongly on the latter, because its specific application brings the most interesting scientific result. However, I will start with the issue related to permafrost.

Here, a very sensible approach to permafrost in general is worth noting, in which the authors avoid terms such as 'permafrost creep', 'ice-rich' or 'ice-poor permafrost', 'permafrost hydrology', etc. This is a big advantage for the work, because these very simplified and in fact incorrect terms are still and quite often used in permafrost research. It should be emphasized here that for many years there has been a general agreement regarding the definition of permafrost, which describes it as a state of the ground. Therefore, since permafrost is a thermal state, it is impossible to assign a material expression to it. The authors seem to understand this well by avoiding incorrect terms, but they do not do it consistently and unfortunately use some incorrect terms interchangeably. I have noted some cases of such use in the reviewed work, which I am sending as an attachment and which is part of the review.

Indeed, using the correct terminology is essential in science and, therefore, in permafrost and geophysical research. We will ensure consistency with the terminology and modify any incorrectly used terms according to the glossary of permafrost and related ground-ice terms published by the International Permafrost Association (IPA).

The introductory part also lacks at least a short critical review of geophysical studies of permafrost in the studied region and a short review of the application of the MASW method in the study of permafrost in mountain and Arctic environments. Such a text would allow for better highlighting the achievement that the authors describe in the work. See for example:

Kula D, Olszewska D, Dobiński W, Glazer M, 2018. Horizontal-to-vertical spectral ratio variability in the presence of permafrost. Geophysical Journal International 214, 1, 219-231

We agree on the need to extend the literature review to other methods using ambient vibrations and surface waves, and to permafrost studies in general. However, site conditions in the Arctic generally do not typically present the challenges of rock glaciers in terms of rough topography, presence of very large boulders, lateral heterogeneity etc. that represent the main obstacles for the MASW application. In fact, several studies already exist in the literature where MASW is used, often in combination with ERT, to characterize permafrost in Arctic regions (Glazer et al. 2020, Liu et al. 2022, Tourei et al. 2024), and we will reference to them in the new manuscript. If we strictly refer to Alpine rock glaciers, to our knowledge, only two MASW case studies exist in literature (Kuehn et al., 2024; Guillemot et al. 2021), that we already cited in the original manuscript and that we already commented on above.

The proposal to determine the presence of permafrost based on the results of original studies is very interesting, because the lack of agreement between ERT and seismic results is very well filled by MASW and this is an original and important result of these studies, most worthy of publication and testing also in other conditions and by other researchers.

However, I have the impression that the article focuses too much on methodological issues, which makes the article more engineering than scientific in nature. While characterizing the methodology and the results of empirical research well, it leaves the proposed models of permafrost occurrence without further discussion. As I noted at the beginning, we know that many similar research works have been carried out in this area since the 1970s. Therefore, in my opinion, it is also important to compare the obtained results with those that are already in scientific circulation. Against this background, the empirical model of permafrost occurrence constructed by the authors will be more credible, more universal and ready to be applied also in other permafrost occurrence environments. This may cause the work to become more universal and more widely cited in the scientific community.

We submitted to the special issue *"Emerging geophysical methods for permafrost investigations: recent advances in permafrost detecting, characterizing, and monitoring".* For this reason, we believe that all technical details provided in the manuscript regarding the geophysical methods are needed to address both the geophysics community and the permafrost community.

As for the previous research, the preliminary investigations that we describe in section 2 primarily relate to a different area of the Flüelapass rock glacier, where the conditions are likely very different. The only geophysical studies in the lower tongue were conducted by Haeberli (1975), Boaga et al. (2024) and Bast et al. (2025). We refer to all three publications in the manuscript. The work of Haeberli (1975), conducted 50 years ago, involves refraction seismic data only, which—as demonstrated in our study—can lead to ambiguities in result interpretation, especially when not combined with the ERT method. Furthermore, we do not know the exact location of the geophysical investigations. The publications of Boaga et al. (2024) and Bast et al. (2025) refer to the same profile as we used for our presented work. The authors focus primarily on methodological developments rather than on a comprehensive characterization of the rock glacier itself. There are no boreholes on the rock glacier, meaning there is no direct information (e.g., on temperature or the stratigraphy) about its internal structure. In conclusion, unfortunately no previous models or borehole data exist for the lower tongue of the Flüelapass rock glacier, which could prove or disprove our findings. That considered, our work has significantly improved both the understanding and the reliability of the structural model of the Flüelapass rock glacier.

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