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# *Interactive comment on* "Improved characterization of alpine permafrost through structurally constrained inversion of refraction seismic data" *by* Matthias Steiner et al.

**Anonymous Referee #2**

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Dear editor and dear authors,

the manuscript entitled "Improved characterisation of alpine permafrost through structurally constrained inversion of refraction seismic data" presents an interesting study investigating the potential of structurally constrained inversion by application to a series of permafrost-related conceptual models and a field data set from the Sonnblick summit in Austria. The paper is very well structured and written, and all figures are of high quality.

The general approach - investigating the potential for improved inversion of refraction seismic data for permafrost-related applications - is important, as seismic refraction



is in theory a very suitable method for permafrost applications, but still receives little attention in the community because of often unsatisfactory inversion results. The paper therefore clearly deserves consideration for publication, but I strongly recommend revisions regarding a) the setup of the conceptual models, b) an extended discussion with regard to the results from the extended constrained inversion (what new can we learn if we constrain layer structure and velocity?), as well as c) a better evaluation of the added value for permafrost interpretation.

GENERAL COMMENTS:

(1) Conceptual Models

- the level of detail of the presented conceptual models (6 layers) is very ambitious and seems not even necessary in comparison with the level of detail reached in the field study (3 layers). Even if an analysis of the different scenarios would without doubt be very interesting for a permafrost context, the conceptual models seem partly unrealistic for different reasons:

a) F2 can hardly be a seasonal evolution of F1: both thawing of the frozen active layer under unchanged snow conditions, and re-filling of the voids with snow are unrealistic scenarios (in a seasonal context)

b) the depth of the layers in the conceptual models is not explicitly given, but seem to vary around 2 m. I have some doubts if with the given velocities all of the conceptual models are theoretically resolvable with 2 m geophone spacing? I can imagine that similar to the blind layer case in F2 (velocity inversion), also layer 3 in T1 is not detectable (hidden layer). This can be tested with a simple forward model and if necessary the conceptual models should be adapted. Or the resolution capacity of RST in typical permafrost situations and its implications should be discussed in a separate chapter. Similarly, the partly small velocity contrasts of $\sim$200 m/s would be hard if not impossible to resolve in a field case without a priori information. Insofar the conceptual models seem to be much too ambitious and pose a somewhat unfair challenge for the standard inversion (Who would expect a standard inversion to resolve 6 layers with partly negligible velocity contrasts?).

- further, the initial models should be shown for each approach, and it should be discussed in more detail, what information is used as constraint, and what new information (in addition to the a priori knowledge) is or can be gained. - an explanation of the motivation to include a velocity inversion (which is by definition not resolvable by seismic refraction) in the conceptual models is missing and should be added (together with a discussion of the respective implications). A more appropriate example than F2 could be a refreezing scenario in early winter (freezing of active layer from the top), potentially causing a velocity inversion between the already frozen and the still unfrozen part beneath.

(2) Discussion/Conclusion

- it becomes not really clear, what exactly is the result of the "extended constrained inversion", i.e. beyond the a priori knowledge used as constraint. The authors should describe, what is the desired information apart from layer thickness and velocity (which is already prescribed). This could e.g. be achieved by a critical discussion of the deviations of the inverted velocities from the constrained initial model (as in the field study): what are the implications of significant deviations between the inverted v and the initial model? Do they point to incorrect structural constraints, which are then compensated by velocity (and could this be used to allow for changes in the layer structure)? Or are structural constraints based on GPR in any case expected to be superior to any structural information contained in RST data? - Further, it becomes not clear, where the authors see the main application of such an approach in the future? In cases with abundant a priori information or rather in cases with limited to no a priori information of the subsurface, and why?

(3) Permafrost

- the approach is motivated with the aim to contribute to permafrost-relevant research



questions, but the permafrost information obtained from the new approach (in addition to what was already known from GPR data) is missing. What can we learn about the permafrost on Sonnblick, which was not yet known before? From a single data set acquired in May under maximum snow cover and under fully frozen conditions of the subsurface, we can only obtain structural information of the subsurface (which may of course be useful), but without a comparison to another data set from summer (with unfrozen active layer) the permafrost-related information is very limited. I would recommend to add (if possible) further data acquired in summer, and compare a) the information contained in frozen and partly thawed profiles, b) the resolution capacity of subsurface layers with and without snow cover, etc. - the overall aim of the study, i.e. improved characterization of alpine permafrost (see title), is not yet reached in a convincing manner. The authors should point out the added value of the RST data inverted with the proposed approach

SPECIFIC COMMENTS:

P4L2ff: as mentioned above: how can you "reliably resolve for the actual geometry of the subsurface units and their corresponding seismic velocities", when exactly this information is already given as constraint? What exactly remains to be resolved? How can you validate the reliability?

P4L23-25/Fig.1a: The fact that "the computation of the initial seismic velocities depends only on the general slope of the surface" is certainly depending on software and not a universal law. I am not aware of a software, which would create an initial model as in Fig. 1a.

P4L28: better write "...physically more plausible..."

P7, section 2.2: this section is informative, but has unfortunately no expression in the discussion or conclusion of the paper. What structural information is contained in the boreholes, which could be used e.g. for validation of the approach? What is the thickness of the active layer, i.e. the depth of the permafrost table resolved in the boreholes? This could be used for interpretation of the RST data.

P7, section 2.3: what was the motivation to collect RST data in winter under maximum snow cover? Why not in summer with one layer less (snow) but potentially more information within the ground (thaw depth, etc.)?

P9L13: do you mean "permafrost table" instead of "ground water level"?

P11, Fig. 5: please add a depth axis

P12L13ff: "our results demonstrate that the inversion with a gradient model yields underestimated seismic velocities for the given synthetic data." –> this statement is a bit too general, as you say before, that near-surface layers are more or less well resolved. Maybe it would be more appropriate to say that mainly in the zone of maximum velocity gradient velocities are underestimated?

P12L19: consider writing "permits to better resolve" instead of "permits to resolve"

P12L25f: as mentioned before, it remains unclear what you mean here: Isn't it obvious "to accurately estimate the velocity structure (...) and to precisely resolve the interface depths", when both parameters are prescribed? If there is nothing else than resolving what you prescribed, what is the point about constrained inversion?

P12L27f: again: as velocity inversion is per definition not resolvable by seismic refraction, this result should be explained in more detail. The standard inversion is not wrong in not resolving situations which are known as limitations of the method, it is just not possible. And if the constrained approach resolves the velocity inversion, isn't that a proof, that the inversion result is only poorly constrained by the data and much more by the initial model?

P13, Fig.6: Please also show the used gradient and constrained initial models for comparison here.

P14L10ff: The analysis of variations in layer velocities is essential for this paper, but this



is only one aspect. What about variations of layer depths, and even variations of depth and velocity? Such a discussion would be a central point in the whole argumentation of the paper (e.g. regarding potential and limitations for the applicability of this approach for cases with little a priori knowledge).

P18L7ff: The "...interpretation of the site permafrost investigation" is short and mainly a repetition of what was already known from GPR, while permafrost-relevant information are missing. I see, that under completely frozen conditions in winter, there is not much permafrost-relevant information to obtain, but this again demonstrates that for a thorough permafrost-interpretation it would be highly beneficial to include an additional data set from summer.

P18L28ff: again: so far it was not yet convincingly demonstrated, what kind of significant new knowledge can be gained by this approach, i.e. beyond resolving (prescribed) layer boundaries and velocities. Further, to show the "robustness of the extended constrained inversion in case of errors of the initial model" (P19L3f) also other aspects of uncertainties in the initial model should be examined (i.e. variations in thickness and a combination of both).

P19L6f: "We showed that a collocated GPR data set provides sufficient information to constrain the inversion of seismic data" –> What do you mean here? Being similar to the GPR result demonstrates that it was strongly constrained, but this is not a validation of the approach. What about comparison with borehole data?

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