tc-2019-52: “Improved characterization of alpine permafrost through structurally constrained inversion of refraction seismic data”

Reply to referees

Editor comments to the authors:

Thank you for your submission to The Cryosphere Discussion. I have read your manuscript entitled “Improved characterization of alpine permafrost through structurally constrained inversion of refraction seismic data” (tc-2019-52). Your investigation of refraction seismic tomography, including a careful treatment of uncertainty and application to a real dataset, is compelling. At first I considered that this manuscript might be better situated in a journal specializing in near-surface geophysics, but the necessity to improve near-surface geophysical methods used specifically in permafrost research is critical. I recommend this submission for review and discussion.

A: We appreciate the assessment of the Editor and acknowledge the comments and suggestions of both referees. As suggested by the referee comments we plan to edit the introduction to clearly state the scope of the manuscript. We believe we need to better describe the relevance of our study regarding the improvement of the inversion of refraction seismic tomography. We will also explain the abundance of numerical models and clearly state that we use the field example to demonstrate the applicability of our approach to real data. This should help the audience to better follow our manuscript and avoid possible misunderstandings. Following the comments of both referees, we corrected and extended the conceptual models and developed more realistic synthetic models. As remarked by the Editor and both referees our initial version of the manuscript has a strong technical component and provides no new insight regarding the permafrost dynamics at Hoher Sonnblick. To overcome this, and to make our manuscript more relevant for the readership of The Cryosphere, we extend now the field study and present borehole temperature data as well as refraction seismic tomographic results obtained for a second data set acquired in August 2016. We believe that such edits keep the numerical character of the manuscript but also provide a more attractive manuscript for the audience of the Cryosphere. As part of our replies we present the proposed new models and the exemplary results for one of those, which clearly demonstrate the improvements in the results after application of our approach. We also provide a point-by-point answer to the comments of the referees and hope we properly addressed their concerns.

Sincerely yours,

Matthias Steiner, Florian Wagner and Adrian Flores Orozco

Anonymous referee #1

Comment #1: The manuscript “Improved characterization of alpine permafrost through structurally constrained inversion of refraction seismic data” by Steiner et al. provides a novel inversion approach of seismic data to identify alpine permafrost. For this purpose, the authors conduct a review of geophysical applications in permafrost studies. They collect Ground Penetrating Radar (GPR) data to get subsurface information, which they use to constrain the inversion of refraction seismic data (RST). The authors apply synthetic data to test the sensitivity of their approach to seasonal variation of the active layer. Furthermore, they apply their new inversion scheme to their collected seismic data and can validate one time step of their synthetic model. Unfortunately, this paper is very technical and addresses a geophysical audience, therefore, the manuscript should be more focused on permafrost data to address the cryospheric community reading “The Cryosphere”.

A: We thank the referee for this positive feedback and understand the concern regarding the target audience. Yet, we believe that the wide use of refraction seismic tomography (RST) in cryospheric geophysical applications necessitates a detailed investigation of the quantitative limitations and possibilities of this technique in the context of permafrost investigations. We believe that the findings of this study in conjunction with the use of open-source software permit instant reproducibility and applicability at different field sites, and hence are of direct interest for the readership of The Cryosphere.

Comment #2: The geophysical approach is interesting, however, (1) numerical examples incorporating topography should be improved and anisotropic effects addressed.

A: We have prepared new models, as we understand that the referee is asking for numerical examples with a larger heterogeneity in the seismic velocities (as opposed to microscopic anisotropy, which would go beyond the scope of this study). Such models consider both lateral and vertical variations in the seismic velocities, as expected from geologically complex scenarios as pointed out by the referee (Figure R1, below). Following the recommendation of the referee, our models now provide some insight on the limitations of smooth-constrained inversions to properly solve: (1) dipping layers, (2) different layer thicknesses, (3) varying layer characteristics, (4) lateral velocity changes, and (5) a vertical fracture.

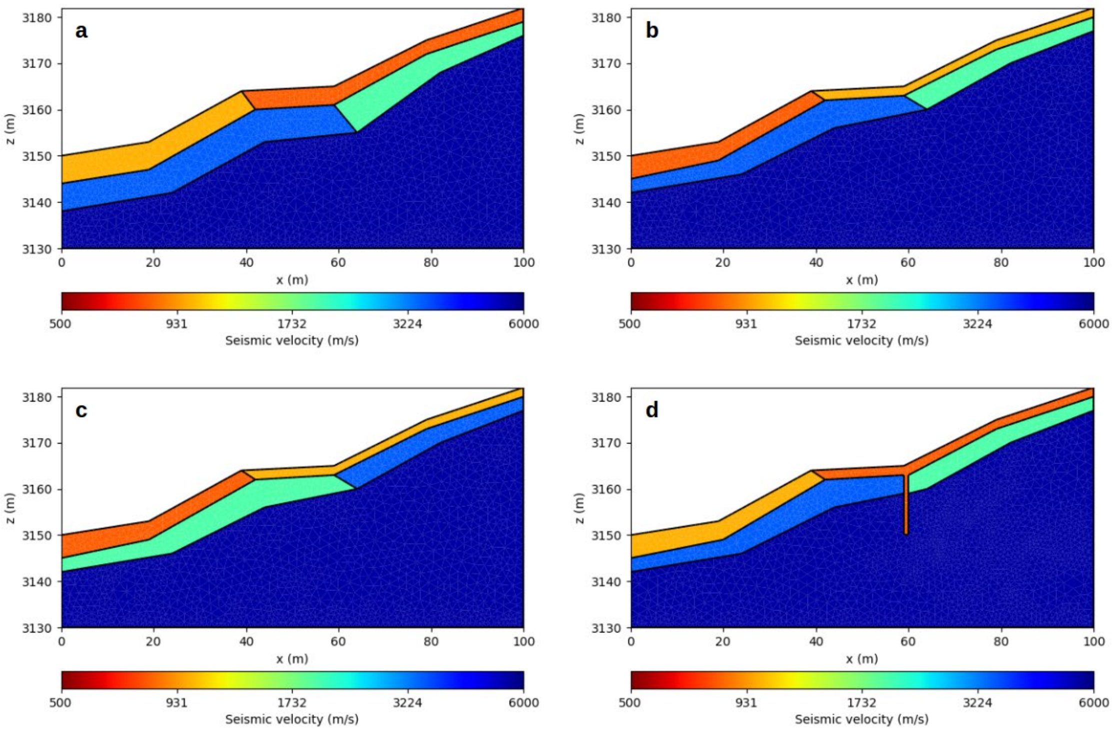

Figure R1: Proposed models for the revised version of our manuscript associated to: dipping layers, variable thickness and lateral velocities changes, fractures.

Comment #3: Furthermore, there are also substantial problems of manuscript. The paper aims to (2) address alpine permafrost, however, provides no new information on permafrost compared to the previous study by Schoener et al. (2012), which is due to the chosen time of data collection and research set up.

A: As stated in our introduction (P3L27 to P4L6), the objective of the manuscript is to quantitatively investigate the seismic velocities retrieved by means of RST and, in a second step, to improve the obtained seismic velocities by means of incorporating (structural) constraints into the inversion. The overall aim is to enhance the quantitative interpretation of the RST images in cryospheric geophysical applications. Until now, the RST method is mainly used as a qualitative tool to assess different lithological units or the contact of possible ice-rich materials. However, process based understanding of permafrost systems forms a vivid research field and requires quantitative interpretation of geophysical images. For example, the application of petrophysical models, such as the well-accepted four-phase model (Hauck et al., 2008; Hauck et al., 2011), which estimates volumetric fractions of water, ice, and air from RST and electrical resistivity tomography, requires the seismic velocities obtained through the inversion to be quantitatively well resolved.
Based on the synthetic model presented in Figure R1a we demonstrate the improved quantitative interpretation of the obtained RST images. Exemplarily, we present In Figure R2 results obtained from standard and constrained inversion of synthetic data subjected to Additive Gaussian White Noise of 1 ms (Figure R2). This example demonstrates that the standard inversion cannot resolve for the known synthetic model, while incorporating structural information permits to solve for lateral velocity changes. However, the seismic velocities obtained from the constrained inversion still underestimate the true velocities. Thus, the extended constrained inversion approach permits to incorporate information regarding the expected seismic velocities to further improve the resolved seismic velocities (as shown in Figure 6 in the submitted manuscript).

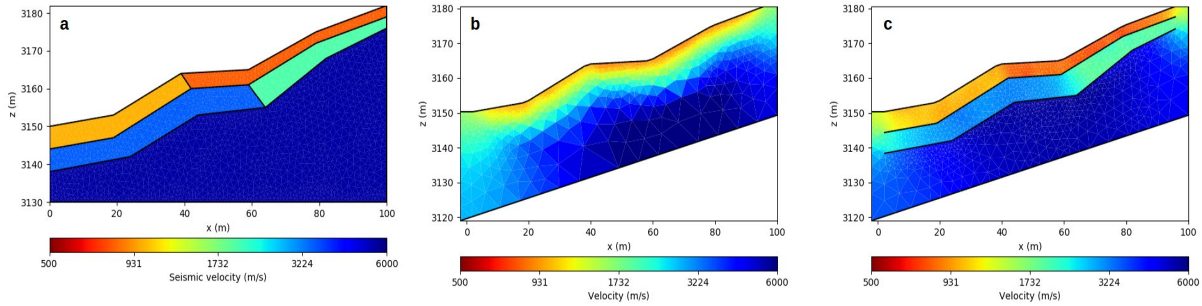

Figure R2: Synthetic model a proposed in Figure R1 (left-hand plot) and the obtained inversion results using smoothness-constrained inversions (b) and after the incorporation of structural constraints (c)

Comment #4: The paper could be suited for publication in Cryosphere if the authors can provide more data to validate their time step model (Fig. 5 and 6) and discus the observed and modelled permafrost dynamics.

A: The models presented in Figure 5 and Figure 6 of this manuscript correspond to merely conceptual models, which intend to illustrate contrasting physical properties of subsurface materials influencing the seismic velocities. Such models permit us to investigate (1) the quality of the inversion of the seismic data, and (2) the quality of the seismic velocities retrieved after the inversion. In response to the referee's comment, we plan to rephrase the corresponding passages in section 2.4 and section 3.1 to avoid the misinterpretation of the conceptual models as one time step model.

Comment #5: (1) The geophysical approach is very interesting but solely focused on a geophysical community.

A: The aim of our study is to present a discussion of the RST, which is commonly applied in the cryospheric community yet mainly used qualitatively. Hence, we intend to show that the resolved seismic velocities might strongly underestimate the actual velocities of the subsurface, which eventually could lead to an inadequate estimation of ice content or porosity. Considering the number of investigations using RST in permafrost related studies, we believe that the manuscript is suited for the readership of The Cryosphere, even if the details on the geophysical method and the inversion approach are abundant. Moreover, in our study we employ an open-source library for the inversion of geophysical data that could be applied by the entire community permitting to increase the comparability of data sets collected at different sites.

Comment #6: To generate starting models by incorporating structural data is a good but also an existing idea.

A: We believe that in the introduction we provided a fair revision of relevant references, and we will appreciate if the referee could point us out to a given reference that we may oversee. We introduced the term 'extended constrained inversion' to refer to the constrained inversion based on layered starting models with homogeneous layer velocities. In the revised manuscript, we intend to rephrase to clearly differentiate between existing and new ideas.

Comment #7: The authors want to show the applicability in rough terrain (Figures 1 and 2), however, they choose a topography that challenges maybe geophysical model approaches but does not exist in nature. The author should demonstrate their approach on realistic slopes such as inclined slopes with ledges and rock steps or ridgelines.

A: The models presented in Figure 1 and Figure 2 feature an extreme topography to better demonstrate the errors related to the initial model that could be incorporated in the inversion of the data. Yet, we agree with the referee that such models can be misleading. Hence, we will move Figure 1 to the Appendix, so that users with less experience could benefit from this exaggerated yet vivid illustration of a possible source of error. In the case of Figure 2, we plan to use the same topography as for the amended synthetic models presented in Figure R1 above.

Comment #8: In addition, the authors assume an isotropic underground as most seismic models, however, a dominant vertical fracture set cause anisotropy that can impede recognition of permafrost layers (e.g. Phillips et al., 2016). The authors should discuss these effects.

A: To address the comments of the referee we propose now to use the models presented in Figure R1 (above), which include lateral heterogeneities and also vertical fractures.

Comment #9: The time-step scenarios (Fig. 5) are interesting, however, water cannot saturate the voids of debris layers on slopes (Fig.5 T1), and the water would simply run off on steep alpine slopes. Furthermore, the frozen scenarios (Fig. 5 F1 and F2) will not exist in this way. The voids of the debris layer will not be frozen to the surface, thus, saturated conditions are unrealistic. However, the pores of the individual rocks forming debris layer can be saturated and frozen and, therefore, increase p-wave velocities and enable the application of seismic methods.

A: We agree that our scenarios were oversimplified since we intended to focus on the analysis of the results obtained from different inversion approaches. Thus, we have corrected the conceptual models presented in Figure 5 to conditions that are more realistic. In the edited conceptual models shown in Figure R3 here, saturation and thermal state of subsurface materials are indicated by colored circles. Moreover, we changed the names of the conceptual models to avoid the interpretation of the conceptual models as one time step model. In the revised manuscript, affected passages of section 2.4 will be modified accordingly.

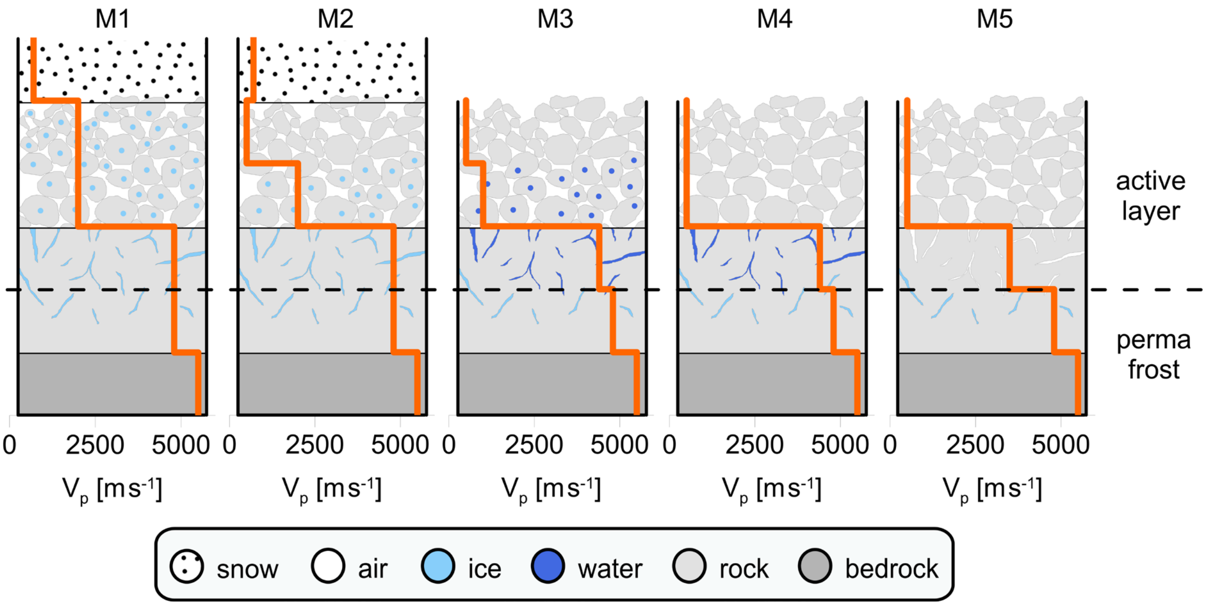

Figure R3: Corrected conceptual models illustrating subsurface conditions.

Comment #10: In general, the approach of geophysics should be a tool to provide realistic models of nature in this case alpine permafrost terrain and should be used to provide new information on permafrost dynamics interesting to the readership of The Cryosphere rather than test synthetic data of unrealistic scenarios that demonstrate the applicability of the used algorithms. The latter use is a valid approach, however, more interesting to a geophysical community and a journal focusing on geophysics, algorithm and inversion approaches. Phillips, M., et al. (2016). "Seasonally intermittent water flow through deep fractures in an Alpine rock ridge: Gemsstock, central Swiss Alps." Cold Regions Science and Technology 125: 117-127.

A: The analysis of synthetic data allows us to quantitatively investigate the actual deviations between inverted and true seismic velocities. An investigation based solely on real data would be affected by noise in the data, variable signal strength and unknown heterogeneities.
We believe that improving the inversion approach is a pre-requisite to improve the quantitative interpretation of geophysical results. This is a critical step to improve the ability of geophysical methods to resolve for real scenarios.
We fully understand the concern of the referee that the manuscript does not present new information about the Hoher Sonnblick; however, that is not the focus of this manuscript. Our aim is to resolve reliable seismic velocities in alpine investigations by enhancing the inversion of the data.
We plan to reformulate the introduction to clearly point out aim, goal and objective of the manuscript.

Comment #11: (2) The authors collected GPR and seismic data in May at a point in year when the active-layer is frozen. Therefore, the authors can identify structural differences in their GPR and seismic data that show a snow layer underlying by a frozen debris and a subsequent frozen bedrock layer. They cannot provide any information on permafrost, thus, the timing of data collection inhibit the differentiation of an unfrozen active layer above a frozen permafrost layer.

A: We believe that this remark of the referee is still based on the same misunderstanding described before. Our study is not aiming at an improved characterization of the permafrost geometry at the summit of Hoher Sonnblick. Our field example aims at demonstrating the applicability of the proposed constrained inversion of refraction seismic data to obtain reliable seismic velocities from the inversion of data collected in permafrost environments. In this case the data collected in May provide an exceptional opportunity to simplify the subsurface properties. As mentioned by the referee, we are facing three main units: (1) the snow layer, (2) a layer of frozen debris, and (3) a frozen bedrock layer. These conditions are properly resolved by means of ground penetrating radar (GPR) and RST. Moreover, our study clearly demonstrates that the incorporation of constraints in the inversion improves the resolved seismic velocities.

Comment #12: Therefore, the novel seismic model maybe improves the data inversion but the applicability to permafrost is purely based on synthetic models. These models should be validated by data. Therefore, I recommend including minimum one more time step that shows the maximum or near maximum active layer depth in late summer and beginning of autumn. This is the usual timing permafrost scientists apply geophysical techniques to get information on active layer depth and annual changes of permafrost distribution to estimate effects of climate change such as the authors introduced.

A: In line with the referee's recommendation we plan to present results obtained from the inversion of a refraction seismic data set acquired in August 2016 along the same profile as the data set presented in the submitted manuscript. For the collection of the data in August 2016, 24 geophones were deployed on the surface with a spacing of 4 m; hammer blows were performed at the geophone positions.
In the revised manuscript section 2.3 and Figure 3 will we modified accordingly.

Comment #13: Furthermore, the authors should provide some independent data to validate their models in terms of unfrozen and frozen status. By incorporating GPR data in the seismic inversion routine, there is a lack of independent data to validate the model results such as temperature boreholes or 1D to 3D temperature models could provide.

A: Initially we wanted to avoid referring to ground truth data at the Hoher Sonnblick, as we wanted to have a larger weight in our study in the detailed numerical models, and use the field case only to demonstrate the application of our observations on real data. However, to make the manuscript more attractive to the cryosphere community we have decided to present temperature data that could support our interpretation of the models obtained for the real field data sets. Nonetheless, we need to point out that the dependence of seismic velocities on temperature is controlled significantly by the saturation level. Hence, the discussion presented in our study necessarily requires the synthetic models to evaluate the resolved seismic velocities.
Please find in Figure R3, an overview of the available borehole temperature information for the summit of Hoher Sonnblick (boreholes 1 top, borehole 2 middle and borehole3 bottom).

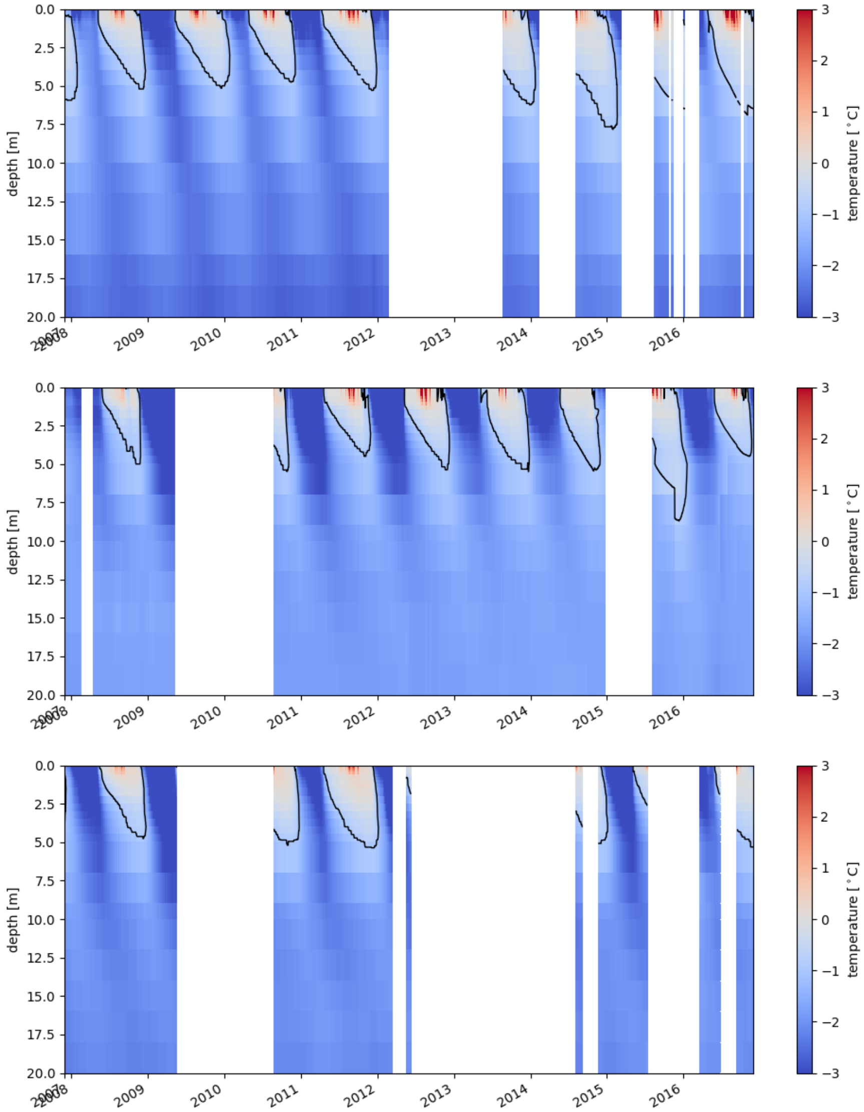

Figure R4: Availability of borehole temperature information at the summit of Hoher Sonnblick. Top: borehole next to the observatory. Middle: Borehole in the center of the slope. Bottom: Borehole in the vicinity of the adjacent glacier.

Comment #14: Schoener et al. (2012) used an interesting approach combining GPR, seismic data, borehole data and temperature models to show active layer dynamics and permafrost distribution in the Hoher Sonnblick.

A: The objective of our manuscript is not the same as in the study by Schöner et al. (2012) since our study does not aim at resolving the dynamics of the active layer at the summit of Hoher Sonnblick.

Our aim is to investigate the quality of the seismic velocities resolved by means of different inversion approaches and used the data set collected at Hoher Sonnblick to investigate the applicability of our approach for the inversion of real data. Nevertheless, we refer in the manuscript consistently to the study of Schöner et al. (2012) regarding the expected seismic velocities.
In this regard, we want to note that in the interpretation of the RST results Schöner et at. (2012) state that seismic velocities of < 3600 m/s indicate jointed bedrock while higher velocities indicate bedrock. Yet, in the combined interpretation approach mentioned by the referee, they draw the border between jointed bedrock and compacted bedrock at ~ 4200 m/s which fits well to the modeled heat transfer. This is a classical problem where the seismic velocities are rather interpreted in a qualitatively way and just the contrasts between low and high velocities are used to indicate possible geometries. However, we believe that resolved seismic velocities need to be quantitatively accurate to permit the application of geophysical methods beyond the delineation of possible geometrical information.

Comment #15: Compared to Schoener et al.'s study, this study currently provides no new insights on permafrost dynamics at Hoher Sonnblick.

A: This is not the scope of the current manuscript. The Sonnblick data solely served as an example data set to demonstrate the applicability of the presented inversion approach.

Anonymous referee #2

Comment #1: 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.

A: We appreciate the constructive comment from the referee. Following the recommendations made by the referee we a) modified the conceptual models to illustrate more realistic scenarios (see the reply to comment #9 made by referee #1 and Figure R1); b) intend to present an extended discussion based on the modified synthetic models (see the reply to comments #2 and #3 made by referee #1) and refraction seismic data collected in August 2016 (see reply to comment #12 made by referee #1) and include borehole temperature data in the interpretation and discussion of the obtained results (we refer the reader to comment #13 made by referee #1 and Figure R2); and c) discuss in detail the influence of improved RST images for the ice content and porosity estimations based on petrophysical models, e.g. the four phase model (Hauck et al., 2008; Hauck et al., 2011).

Comment #2:
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 _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.

A: We appreciate the detailed suggestions of the referee concerning the improvement of the conceptual and synthetic models used in the numerical study.
We want to state that one of the reasons of the initially proposed models (6 layers), was to investigate the resolution and accuracy in the inversion of RST, yet instead of adding lateral heterogeneities, we built them vertically. This needed to be corrected, and now we propose the models presented in Figure R1, which we hope address the comments of the two referees.

Comment #3: (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?

A: The referee is right in stating that in case of the extended constrained inversion the velocity is already prescribed. However, in case of real data it is not possible to prescribe the true velocity for a certain layer. Moreover, in most cases even the structural constraints will not resemble the true subsurface conditions, as these might come from other data such as temperature, lithological logs, GPR. We addressed these problems by conducting a numerical study in the submitted manuscript. Based upon the results of the numerical (Figure 8 in the submitted manuscript) study we could show that even in case of erroneous a priori knowledge we obtain better estimates for the seismic velocity that can be quantitatively interpreted. This implies that the extended constrained inversion approach improves significantly the retrieved seismic velocities, even if the existing information does not perfectly related to seismic velocities. Moreover, such approach permits to include different information to solve for a single model.
For the revised manuscript, we plan to discuss the influence in the inversion after providing incorrect structural information and initial velocities (for the case that known information is incorrect) based on the synthetic model presented in Figure R1a.

Comment #4: (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

A: The aim of our study is not to obtain an improved characterization of the permafrost at Hoher Sonnblick. The presented field data serves solely as an example data set demonstrating the feasibility of inversion approaches incorporating a priori knowledge to resolve reliable seismic velocities in permafrost environments.
Following the referees recommendation in the revised manuscript we plan to present results based on a refraction seismic data set collected in August 2016 (see also reply to comment #12 made by referee #1).
Regarding the added value of the (extended) constrained inversion approach the reader is kindly referred to our answers to comments #1 and #3 made by referee #1 as well as comment #3 made by referee #2.

Comment #5:
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?

A: We thank the referee for these observations. We will edit the corresponding passage in the Introduction to state that we investigate the reliability of the standard inversion and that our numerical study demonstrates that by incorporating structural constraints yields better estimates of the true values.
For further details regarding the constrained inversion, the reader is kindly referred to our replies to comment #10 made by referee #1 and comments #3 and #14 from referee #2.

Comment #6: 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.

A: This line has been removed. The reader is kindly referred to our answer to comment #7 made by referee #1.

Comment #7: P4L28: better write "...physically more plausible..."

A: We have included the suggestion from the referee.

Comment #8: 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.

A: In the revised manuscript we will present borehole temperature data (see reply to comment #13 made by referee #1).

Comment #9: 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.)?

A: We will correct this in the revised manuscript to demonstrate the applicability of our extended structural inversion on data sets collected in two different periods (August 2018 and May 2017). The referee is kindly referred to our reply to comment #11 made by referee #1.

Comment #10: P9L13: do you mean "permafrost table" instead of "ground water level"?

A: We thank the referee for pointing out this inaccurate wording. We will rephrase it in the revised manuscript.

Comment #11: P11, Fig. 5: please add a depth axis

A: The conceptual models presented in Figure 5 are mere schematic representations of assumed subsurface conditions at a debris-covered slope. We appreciate the referees concern, but are confident that the main purpose of these generic diagrams is to provide a visual comparison of subsurface conditions and the expected seismic velocities. Hence, adding a depth information would require referring these models to a specific site; yet, this is not the intention at this point in the manuscript. Moreover, we use the conceptual models presented in Figure 5 for the numerical study presented in section 3.1 to create synthetic models by adding information regarding the profile length, layer thicknesses and topography.

Comment #12: 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?

A: To address the remark of the referee we intend to rephrase the corresponding paragraph in the revised manuscript.

Comment #13: P12L19: consider writing "permits to better resolve" instead of "permits to resolve"

A: We will rephrase the sentence in the revised manuscript as suggested by the referee.

Comment #14: 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?

A: We agree with the referee that in case of clean synthetic data sets it would be obvious to resolve for the underlying synthetic model. However, in case of real data sets various factors, for example, complex layer geometries, an unfavorable station layout or ambient seismic noise, might have a negative effect on the data quality. To this end, we tried to design demanding synthetic scenarios and, as mentioned in P12L1f, subjected the synthetic data to Additive Gaussian White Noise (AGWN). Our numerical study in the submitted manuscript shows, that in case of a challenging data quality the incorporation of a priori information permits resolve seismic velocities not only suitable for a qualitative but even a quantitative interpretation.

Comment #15: 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?

A: The reader is kindly referred to our answer to comment #2 made by referee #2.

Comment #16: P13, Fig.6: Please also show the used gradient and constrained initial models for comparison here.

A: Gradient and constrained initial models will be shown in the revised manuscript.

Comment #17: 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).

A: In line with remarks from referee #1, we modified our synthetic models to more realistic and more complex scenarios (see replies to comments #2 and #3 made by referee #1 and Figure R1). Based on the modified synthetic model presented in Figure R1a we will investigate the influence of incorrect initial velocities and wrong interface depths on the obtained RST images (see our answer to comment #3 made by referee #2).

Comment #18: 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.

A: The reader is kindly referred to our answer to comment #12 made by referee #1.

Comment #19: 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).

A: The reader is kindly referred to our replies to comments #2 and #3 made by referee #1 as well as comment #1 and #3 made by referee #2.

Comment #20: 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?

A: We will edit the manuscript to correct this line. The referee is right; the similarity between the RST images obtained by incorporating GPR-based constraints and the GPR data themselves is not a validation. Our intention was to demonstrate the applicability of our extended constrained inversion approach for real data sets by incorporating GPR-based constraints in the inversion of RST dat. To validate our results we will include borehole temperature data in the revised manuscript; we kindly refer the reader to our answer to comment #13 made by referee #1.