Dear editor, dear reviewers,

Many thanks for your helpful, critical and constructive comments, which significantly helped to improve our manuscript. Below you will find our detailed answers to all reviewer comments.

With our best regards,

Benjamin Mewes, Christin Hilbich, Reynald Delaloye and Christian Hauck

Author response to reviewer comments

I. Anonymous Referee #1

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General Comment:

This paper aims to analyse the resolution capacity of Electrical Resistivity Tomography (ERT) and Refraction Seismic Tomography (RST) to identify hydrological and periglacial processes in rock glacier. To address this objective, the authors use ERT and RST data from the Becs-de-Bosson rock glacier in the Swiss Alps measured in 2012 and 2013, create synthetic models to test the resolution capacity of ERT and RST as well as 4PM, a combined ERT-RST-approach developed by Hauck et al. (2011). Hydrological processes of rock glaciers are seldom investigated and poorly understood. There is a lack information how water flow influences active layer thaw or permafrost degradation in rock glaciers or affect rock glacier movement. Therefore, the objectives of this study are interesting for a wider readership and addresses a relevant scientific question within the scope of journal The Cryosphere.

I.1 *However, this study provides no novel concepts, ideas or tools but partially new data. There are no substantial conclusions reached based on the presented results and assumptions of the used synthetic models have no empirical or knowledge basis and contradict partially physical laws. Consequently, the analysis of the resolution capacity completely fails. In addition, scientific methods and model assumptions are insufficiently outlined.*

Response to reviewer: We are very sorry for apparently having been unable to better explain the general and more specific aims of our study! The main aim of the study was not to explain hydrological processes within a specific rock glacier (Becs-de-Besson) or within rock glaciers in general, but rather to create a test environment to evaluate the performance of the 4-phase model on e.g. rock glaciers; specifically with respect to uncertainties stemming from inversion or geometrical issues, such as its resolution capacity. The data from Becs-de-Besson rock glacier were meant to illustrate how this approach could be applied to real-world problems, but not as real case study. Similarly, we tried to use a real example as base for generating the synthetic models – however, by reading the comments of reviewer 1 we now noticed that this induced more confusion than gain, as he/she correctly identified all physical implausibility within these models, which we did not pay enough attention to. From a theoretical point of view (the 4PM test environment), the choice of the synthetic models does not matter so much, as the goal of the study is to see how well the 4PM and the inversions were able to reproduce them – and this is mainly a matter of gradients in the geophysical properties (resistivity and P-wave velocity) and geometry. However, of course we should have paid more attention to the plausibility of our synthetic models and we would like to apologise for this neglect.

Regarding the above hopefully clarified aims we do think that our study provides new concepts, ideas and tools as to our knowledge (1) no one has ever analysed the accuracy of the 4PM in this way and in relation to the quality of both ERT and RST inversions; and (2) nobody has used the full forward/inverse cycles and appraisal of ERT and RST AND 4PM regarding the quantification of ground ice and water content in one conceptual work flow yet.

In our revised version of the manuscript we will largely improve the explanation of the study design and remove any parts where the apparent misunderstanding relates from. We also see now that our aims have to be reformulated to better communicate our intention with this study. We will also improve the description of the methodology and the model assumptions, especially regarding the choice of synthetic models. More details about this are listed below with respect to the detailed reviewer comments, also regarding the comments of reviewer 2.

I.2 The major problem of this study is, it uses data which was not derived to address this study's research question and research set up is not suited to give new insights on the important topic of hydrological processes in rock glaciers.

Response to reviewer: The reviewer is very right that the monitoring installation at Becs-de-Besson rock glacier, which used a large sensor spacing as it is common in many general monitoring studies on Alpine permafrost, were originally not started to address the above clarified research questions. It is also correct that with the large sensor spacing used for the geophysical surveys on the rock glacier most of the small-scale hydrological processes in rock glaciers cannot be monitored – but this was (a) not the original aim of the surveys, as the question of hydrological processes within rock glaciers became important only recently and (b) it is the aim of our paper to show exactly that: the resolution capacity of standard geophysical monitoring set-ups. Here, as mentioned in our response to comment I.1, we used the data primarily to illustrate the evaluation concept of the 4PM and not to prove/disprove the existence of e.g. vertical infiltration on this rock glacier. We even mentioned especially in our Discussion that: "…the low resolution and underestimated magnitude of change are due to the coarse sensor geometry, which was by intention chosen to be identical with the field data to simulate real field conditions. The sensor spacing of 4.5 m for the monitoring at Bec-de-Bosson is comparable to many other field sites in permafrost geophysics, where the profile length (ideally covering the entire landform) and the investigation depth (reaching the base of the landform) have similar high priority as spatial resolution. The results of this study therefore emphasise that interpretation of geophysical data and 4PM results could significantly be improved by using an increased spatial resolution of the sensor geometry, i.e. smaller electrode/geophone spacings". However, we agree that a more specific focus on the sensor spacing would be very beneficial for the paper, so we conducted all simulations also with a smaller spacing of 2 metre to show the effect of sensor spacing on the 4PM. As will be shown in the revised version of the manuscript, the smaller spacing increases the horizontal and vertical resolution, but does not induce major changes regarding the inversion artifacts discussed already in the original version of the paper. A respective section will be included in the results and discussion chapter to illustrate which artifacts and misfits stem from coarse resolution and which are due to specific contrasts in the geophysical properties. Finally, we clarified the aims of our paper in the introduction as follows:

"The aim of this study is to analyse the qualitative and quantitative resolution potential of repeated geophysical measurements (ERT and RST) for the monitoring of hydrological and glaciological processes with a focus on snow melt infiltration and ice melt. We focus hereby on standard geometries as primarily used in mountain permafrost monitoring applications such as in the context of permafrost degradation within rock glaciers. For this we set up four synthetic models with simplified geometries simulating idealized water flow paths and ice loss scenarios. After inverting the synthetic data sets to yield ERT and RST tomograms, we combine the results from both methods to run a petrophysical model, which estimates the quantitative distribution of ice and water contents within the rock glacier (Hauck et al., 2011). With this so-called 4-phase model (4PM) and the simplified scenarios of changes in ice and water content we investigate the performance of the 4PM regarding its spatial resolution capacity, the latter being partly a function of the individual resolution capacities of ERT and RST tomograms. Because resolution capacity depends first of all on acquisition geometry (sensor spacing) the synthetic experiments are conducted with different spacings including the rather coarse standard spacing of 4-5m used on many rock glaciers worldwide to achieve large penetration depths.

Finally, the results will be analysed in comparison with real field data from the Becs-de-Bosson rock glacier, a well-investigated rock glacier in the Swiss Alps (e.g. Tenthorey, 1993). Seasonal and inter-annual ERT and RST monitoring data during a 2-year period from 2012 to 2013 with coarse spacings are used to model the volumetric fractions of ice, water and air with the 4PM to illustrate that the resolution potential regarding important hydrological processes in the context of ice content changes can be quite small for standard coarse-scale monitoring setups."

Detailed Comments:

<u>*The data set and model generation of ERT and RST:*</u>

I.3 *The data set and model generation of ERT and RST: The authors used an ERT data set derived from 71 electrodes with 4.5 m spacing which should result in a transect length of 315 m much longer than the transect length displayed in Figure 3a and 9a. There is no information which software was used to invert the data, I assume Res2DInv, and which inversion parameters (e.g. robust constraints, number of iterations,* *smoothing) was selected. Res2DInv enables to weight vertical or horizontal layers, latter approach is often used in assumed layered structures such as in rock glaciers.*

Response to reviewer: As the 4PM needs joint ERT and RST data acquired along the same horizontal grid for the calculation of ice-, water- and air contents we focused our analysis on the profile where these two data sets are available, which is the length of the seismic profile shown in Figure 3. The ERT profiles are of course longer, as correctly mentioned by the reviewer. A corresponding sentence was added in the section "Field site and available data". The full ERT profiles will be included as supplementary material.

The software (Res2DINV for ERT and ReflexW for RST) and inversion parameter used were already mentioned at the end of section 3.3 in the original version of the manuscript ("All field and synthetic data were hereby inverted with the RES2DINV (Loke, 2015) and ReflexW (Sandmeier, 2015) software using a robust L1-Gauss-Newton-Inversion for the geoelectric data (and standard inversion parameters as described e.g. in (Hilbich et al., 2009), and the simultaneous iterative reconstruction technique for the seismic data (inversion parameters chosen according to Hilbich (2010). »). We moved this part to the beginning of this section for better readability. As we do not want to bias the final 4PM result towards the "known" distribution in the synthetic models no further regularisation was applied in the inversion. We added the following sentence at the end of this section 3.3: "No further a priori information was prescribed in the inversion as to introduce no (wanted or unwanted) additional bias into the 4PM evaluation."

I.4 *In addition, data processing of the seismic data remains unknown, there is no description how seismic traveltimes were derived and how these traveltimes were inverted into the tomography. I assume, the authors used the tomography module provided by ReflexW, however, inversion parameters and data quality (such as Root Mean Square Error) are required to follow the authors modelling approach and to evaluate the model quality.*

Response to reviewer: see answer to comment I.3 – inversion parameters were used following the approach described in Hilbich (2010). All data misfits (RMS, absolute errors) are now included in the respective Figures.

I.5 *Hilbich (2010) used a layered starting model with a gradient increase of p-wave velocities. This way of modelling as well as potential horizontal weighting of resistivity data results in a layered model often used in rock glacier seismic modelling. If the authors used layered modelling approaches, then it is very difficult to resolve vertical structures in tomographies which is the main objective of this study.*

Response to reviewer: We are not sure whether we understood the comment correctly: first you mention the gradient increase in the *p-wave velocity* starting model, then the horizontal weighting in *resistivity*, then again *seismic* modelling? In any case, for our 4PM appraisal we have chosen a standard approach used most often in mountain permafrost geophysics to show its resolution capacity. Of course we could have introduced more suitable acquisition geometries (smaller spacings, as now included in the revised version, but also other electrode configurations more suitable for detecting vertical structures, e.g. dipole-dipole) and/or inversion parameters more suitable to detect vertical infiltration. But our aim was not to invert the synthetic data as perfect as possible regarding the known synthetic models, but to apply standard inversion routines with regard to an unprejudiced interpretation of usually unknown conditions and in order to appraise the performance of the 4PM. We apologise to have not better communicated the aims of the study and consequently added a corresponding paragraph in the Discussion section. See also our response to comments I.1 and I.2.

I.6 *The authors aim to evaluate snow melt influence on active layer thawing, however, they have no information on snow cover. They determined complete snow meltout using temperature data loggers and conclude that snow melt was completed one day to one month before the measurements took place. Due to the timing of the measurements, it is impossible to measure water infiltration or increased water levels.*

Response to reviewer: This is a misunderstanding: we do not aim to evaluate the influence of snow melt on active layer thawing in one specific case. We aim to analyse the theoretical capabilities of the 4PM to detect/visualise these processes (as written in the original manuscript: "Geophysical input data and 4PM results are analysed with respect to their qualitative resolution potential regarding processes related to snow melt infiltration …")! The field data are only used to illustrate this concept and to point out ways to improve the representation of these processes in geophysical data and processing routines. Of course, for a detailed case study e.g. on snow melt infiltration processes measurements on different time scales have to be used, as has been published e.g. in Hilbich et al. 2011.

I.7 *The authors write in their result section that "due to the coarse grained material and the slope of the rock glacier, seasonally or inter-annually melting ice would rather result in quick infiltration and run-off on top of the ice-layer" (page 12 l.22ff). If you know that water will infiltrate and run-off immediately in high-porosity rock glaciers why you build two synthetic models that assume vertical saturated areas or and saturated layers exist? The timing of the measurements induces that the authors can obtain the response of the active-layer and permafrost layer due to thermal and hydrologic processes that occurred before the measurement. To distinguish the effects of these processes is impossible without any further data on snow distribution, snow height, water content measurements or sedimentological insights on the rock glacier. Anyway, the author derived a baseline model (SYN1, Fig. 3b) based on the ERT and RST measurements in July 2013 (Fig. 3a).*

Response to reviewer: The choice of the scenarios were amongst others inspired by the results of earlier comprehensive hydrological investigations at this site (tracer tests), which imply a complex network of preferential flow paths, some of them certainly also beneath the ice core (see chapter 2). In addition, there are indications from other rock glaciers that there is indeed not only quick drainage on top of the PF, but that zones with quick infiltration (=

preferential flow) through the ice may exist (e.g. Krainer & Mostler 2002). The fact that the coarse grained material and the slope of the rock glacier will primarily lead to quick infiltration and run-off on top of the ice-layer does not mean that other processes may not take place as well at certain locations, especially where flat or concave areas are present. As the detection (and monitoring) of these kind of processes is one of the current aims in rock glacier hydrological research, we consider it reasonable to address them through specific scenarios.

Synthetic model generation and assumptions:

I.8 *It is unclear where the synthetic models are located and which part of the ERT and RST us overlapping. The baseline model (SYN1) assumes the existence of a dry, coarse active layer with 8000 Ωm and p-wave velocities between 500 and 1000 m/s in the lower section of the rock glacier and a wet, fine debris layer with 1500 Ωm and 350 – 800 m/s. Unfortunately, there is no sedimentological evidence which can support the lateral differentiation of the debris.*

Response to reviewer: The location of the overlapping ERT/RST profiles has been included in the new Figure 1, see also our response to comment I.3. The choice of the synthetic models was inspired by, both, observations in the field (indeed more fine-grained material in the upper part than in the lower part) as well as the obtained resistivity/velocity results of the field data. During an iterative process we tried to achieve the best possible match between observed resistivites/velocities, visual field observations and theoretical values for the different substrates from literature. In the original manuscript we wrote accordingly: "A simplified subsurface structure was constructed from the inverted field data, and realistic values for ρ_s and v_p were assigned to all layers (see next section). These values were taken from literature (cf. Table 1) and adapted in order to fit the inverted field data as best as possible." We are of course aware, that the process of developing synthetic models is very subjective and could have been performed also differently. In any case we will further clarify the source of the synthetic models in our revised manuscript (see also response to comment I.10).

I.9 *Furthermore, the authors distinguish ice-rich and ice-poor layers. The ice-poor layer possesses resistivities in the range of the active layer and is no evidence provided by the RST or other independent data that confirm that an ice-poor layer exists. In summary, the derived baseline model is poorly underpinned by data. Additional thermal and sedimentological data from e.g. boreholes should be used to construct a baseline model.*

Response to reviewer: See our response to comment I.8 and also I.1: the synthetic baseline model was constructed using all evidence available. We would like to use the opportunity to state once again that a synthetic model does not have to be underpinned by detailed data, as it can be seen as conceptual model of a rock glacier, and its basic (geophysical) features are true for many rock glaciers. The details (adding an ice-rich or ice-poor layer) may differ of course. In our case we used the Becs-de-Bosson rock glacier as illustrative example, but it would not be critical at all for our results if the baseline model does not correspond 1:1 to the real situation. It is just an example to analyse the resolution capacity. In Hilbich et al. 2009, a similar approach was chosen for (only) ERT inversion appraisal using rock glacier Murtèl (Eastern Swiss Alps) as illustration. Here, we go one step further by including RST inversion and 4PM analysis.

I.10 *Synthetic model 2 (SYN2) assumes the existence of a 1.5 m thick saturated layer in a depth between 3.5 and 5 m. There is no evidence in the data which supports this scenario. In addition, the authors admit in the result section that the scenario is unrealistic, thus, "seasonally or inter-annually melting ice would rather result in quick infiltration and run-off on top of the ice-layer" (page 12 l.22ff). Synthetic model 3 (SYN3) assumes the existence of three 0.5 m wide water saturated vertical paths of preferential flow and a horizontal flow path below the ice core. Rock glaciers consist of debris and porosity is assumed to be around 40%. The large pores between the individual blocks act as macropores. There is no explanation what constraints the water and results in vertical infiltration paths. It remains completely unknown why the authors choose the location of this vertical preferential paths at 30 m, 55 m and 90 m transect lengths. Furthermore, it is impossible that water saturated areas of 0.5 m width exist surrounded by pores of the same porosity filled with air. Water could use these air-filled pores to run off. However, Figure 6b displays this physically impossible scenario. The remaining synthetic model (SYN4) describes active-layer thaw and is the only reasonable model that can be derived from the ERT and RST data.*

Response to reviewer: We are thankful for this critical comment regarding the realism of the synthetic scenarios SYN2 and SYN3, as it shows that we have to better explain (a) the aim of the synthetic scenarios (see also our responses to the comments above) and (b) the technical constraints regarding geometry and resolution in ERT/RST inversions and 4Pm analysis. Synthetic model 2 (SYN2) assumes the existence of a 1.5 m thick saturated layer – this might be exaggerated in thickness, but is caused by the coarse model grid which corresponds to the large sensor spacing at the surface. A saturated layer on top of the ice layer, however, is conceivable (see also Hilbich et al. 2009). As stated above, the scenario (and the paper) investigates the resolution capacity of the ERT/RST/4PM approach, i.e. whether such a layer, if present, **could be detected with these methods or not**. That does not mean that we know it is present at this specific rock glacier. This whole analysis is independent of the specific rock glacier Becs-de-Besson.

Furthermore, we do not think that our sentence: "seasonally or inter-annually melting ice would rather result in quick infiltration and run-off on top of the ice-layer" is a contradiction to SYN2, as this can be seasonally very different. During the snow melt and thawing phase, water can accumulate on top of the ice layer (in addition to refreezing) as most of the pores are still frozen and water cannot easily infiltrate. However, as soon as thawing has advanced to larger depths, the free pore space can indeed lead to very quick run-off, which can also be seen in a strong decrease of electrical resistivity the high-resolution ERT monitoring studies (cf. Hilbich et al. 2011).

Similarly, the heterogeneity of the ice-rock matrix may lead to preferential infiltration, i.e. the ice in the pores in one place blocks the melt water, but it allows macropore flow in another place, as the ice will not homogeneously block water flow along the whole rock glacier. Even within the correspondingly homogeneous rock glacier Murtèl, Eastern Swiss Alps, spatially heterogeneous behaviour regarding water flow has been observed (see e.g. Arenson et al. 2010). Correctly, the pure vertical flow in SYN3 is a strong over-simplification but was chosen to distinguish the resolution effects of horizontal (SYN2) and vertical (SYN3) layers. The position of the vertical flow paths is purely arbitrary – here, only the ration between distance and extension of the different flow paths matter for the resolution analysis. The unrealistically thick extension of the flow paths is again due to the geometry of the whole model set-up.

The section introducing and justifying the synthetic models was consequently thoroughly reworked. It now hopefully better justifies each synthetic model and discuss the presence/absence of realism as mentioned by the reviewer. Where sensor spacing/model geometry is concerned these points are also further discussed by comparing the results for small/large sensor spacings.

4PM model

I.11 *In the next step of this study, the synthetic models are applied in the 4-Phase model. The authors conclude that a suitable porosity model is of great importance for 4PM modelling, thus, porosity stays constant with time and small errors in the porosity model will impact the resulting model. However, it is not sufficiently explained what porosity model they chose and why. Figure 6a displays a two layered porosity scenario. The upper layer has a porosity of approximately 40 % and reach from the surface to 10 m depth. The lower layer range from 10 to 15 m and shows a porosity value of approximately 5 to 10 %. This porosity model contradicts the RST and ERT data in Fig. 3a as well the baseline model in Fig. 3 b. If there exist a difference between coarse debris in the lower part of the rock glacier and fine debris in the upper part, the porosity should reflect this sedimentological differences.*

Response to reviewer: We apologise for the missing information about how and why the porosity model was chosen in this way. We added a corresponding paragraph in the 4PM section (chapter: Methods) and explain it below. However, we do not agree with the reviewer regarding the point that the porosity model in old Fig. 6 contradicts the base line model and the field data: the porosity model that was chosen reflects basically the presence of coarse-grained rock glacier material with a porosity of 40% and a bedrock layer at around 10m depth. The presence and depth of this bedrock layer can be seen in the RST field data. Note, that for our synthetic modelling study simplified structures were chosen, so there is no need to introduce small-scale variations in the bedrock topography. The lateral differences in resistivity between 0-10m in the field data can be due to substrate differences (which still may have the same porosity!), ice content differences and water content differences (related to e.g. temperature). A priori, it is not clear whether these differences (which are less visible in the seismics data) should be reflected in the porosity model or would rather prescribe the result, i.e. a laterally different ice content. For this reason, we chose a laterally homogeneous porosity model.

Finally, our visual observations in the field (more fine-grained material in the upslope part of the rock glacier) relate only to the surface material, and no information about porosity at larger depth is available.

I.12 The bedrock is described of quartzite and gneiss which are usually possess lower porosities than 5 to 10 % especially if they are covered by 10 m thick debris. A borehole could be used to determine once realistic values. The sensitivity of 4 PM modelling to porosity should be quantified.

Response to reviewer: This is correct. However, the two petrophysical equations that form the base of the 4PM (Archie's Law and the time-averaged model by Wyllie) are badly defined for very low porosities close to 0%. For numerical reasons, a porosity of 10% was therefore chosen. A corresponding sentence was added to the new paragraph describing the porosity model, where we also discuss the sensitivity of the 4PM to porosity (presented already in detail in e.g. Hauck et al. 2011 and Pellet et al. 2016)

Summary:

I.13 In summary, the authors construct two unrealistic scenarios (SYN 2 and 3) and a nonconfirmed baseline model (SYN1) combine it with a 4PM model of assumed porosities and show that vertical structures and saturated layers are well resolved in their synthetical models. Unfortunately, these features cannot be detected in any of the data-derived ERT and RST models. Therefore, the authors validate their unrealistic assumptions with their synthetic models. The whole research approach is model-driven and not research-driven.

Response to reviewer: as mentioned above, this comment seems to stem from a major misunderstanding. It was not the aim of the study to find these features in the data, quite the contrary, the research question was the following: if such processes are present in a rock glacier: could they be detected with 4PM under the given numerical constraints or not? That's the question we want to answer and that's why the title of our study starts with "Resolution capacity of geophysical monitoring…" – it's not a rock glacier case study! And yes, the research is driven by the need to improve a geophysical monitoring technique and a corresponding model approach: it's about the accuracy and the applicability of the 4PM approach. We do not understand at all the remark in the last sentence of this comment.

I.14 Resolution of vertical and lateral structure was the original aim of the study. This resolution depends on the geometry of electrodes and geophones. An investigation of resolution should use different geometries and compare the effect of these geometries.

Response to reviewer: this is correct. Additional simulations with smaller spacing are added in the revised manuscript. Note however, that even though lateral/vertical resolution increases with smaller sensor spacing, the numerical (inversion) artefacts stemming from the typically large contrasts of geophysical properties are still present for the smaller sensor spacings.

I.15 *The used measurement set up, timing of measurements and the lack of additional geomorphological, sedimentological and snow cover data result that this study provides no novel information which should be published in The Cryosphere. The authors fail completely to achieve one of their aims. All conclusions are basic knowledge and published before in numerous papers.*

Response to reviewer: We do hope that we have given sufficient clarification of the aims and set-up of our study in our responses to the comments above to show that the above comment is certainly not true. We apologise for the misunderstanding regarding the aim of the paper – apparently we have explained our concept and aims not clear enough. This study goes clearly one step beyond typical geophysical monitoring paper of mountain permafrost, as it does not present one specific case study, but tries to analyse the principal resolution capacities of typical geophysical monitoring approaches. As numerically coupled ERT and RST monitoring approaches are very rare and only very few papers exist, we cannot understand the motivation for the above sentence "*All conclusions are basic knowledge and published before in numerous papers*." in this comment.

II. Anonymous Referee #2

Received and published: 6 January 2017

The paper by Mewes et al. describes synthetic studies and a field application of geophysical monitoring on alpine rock glaciers. As such, the topic is well suited for The Cryosphere. I have read the paper with great interest, and I judge that the author's approach has a considerable potential. However, I have a few critical comments concerning the structure of the paper and its contents that should be considered, before the paper can be accepted.

II. 1 The structure of the paper and the associated workflow in Figure 2 are unnecessarily complicated. Likewise, I find the convoluted discussion on "forward-inverse-cycles" (page 5) not at all helpful. What the authors essentially do is the following.

(i) Setup of four synthetic models (inspired by an actual field scenario)

(ii) Inversion of synthetic data followed by a 4PM analysis

(iii) Inversion of observed data followed by a 4PM analysis

(iv) Appraisal of the significance of the field data results using the findings of the synthetic study.

I recommend restructuring the paper following the outline above. The models SYN1 to SYN4 are a very crude approximation to the assumed Bec-de-Bosson rock glacier structure. When introducing the synthetic models, it is therefore sufficient to mention that they were inspired by the Bec-de-Bosson rock glacier. Consequently, most of the material contained in section 2 can be moved to the field data analysis part.

Response to reviewer: We are very thankful for this very helpful comment, which made us restructure our paper in a much clearer way and make it hopefully easier to follow. We also think that it might help to avoid the misunderstandings which seem to have partly caused some of the comments in review #1. We did restructure our paper accordingly: chapter 2 was shortened considerably focusing more on data acquisition and less on the hydrological processes which were partly included now in the results chapter (field data analysis). The work flow in section 3.2 was modified focusing on the 4 main points mentioned in the reviewers comment above, and Figure 2 was modified accordingly. Finally, the presentation of the results in chapter 4 was slightly restructured to match the same four analysis steps.

II.2 The inversion results of the synthetic data are very poor. This is not only due to the sparse acquisition geometry and (probably) the too short offsets of the ERT and RST layouts, but also reflects the inherent resolution limits of the two methods. The ERT image of SYN1 shows a greatly blurred high resistivity zone, and none of the other features are visible. SYN2 is dominated by a blurred version of the conductive feature introduced. It totally obscures the underlying resistive zone. Likewise, the ERT results for SYN3 and SYN4 do not really mimic the true structures. Similar conclusions can be drawn from the RST tomograms. This is not surprising, because it is well known that *low-velocity layers are difficult or even impossible to resolve with RST. For high-velocity zones only the upper boundaries can be usually resolved.*

Considering the poor tomograms, it is very surprising that the 4PM results represent the features introduced in SYN2 to SYN4 relatively well. In contrast to a joint inversion, where the complementary nature of ERT and RST data are exploited, the 4PM model is a mere combination of the (poor) tomograms. Therefore, one is kept left wondering, as to whether the results in Figure 8 can be generalized, or if they are a remarkable coincidence. This needs to be further analyzed and discussed.

Response to reviewer: This is a very good point and it is also the focus of our current research work on the applicability and usefulness of the 4PM. From our previous experience and theoretical considerations (as e.g. detailed in the original 4PM paper Hauck et al 2011) the 4PM works well for large horizontal contrasts in ice content (as e.g. for delineating the ice-rich zone in rock glaciers), less so for vertical contrasts, as the sensitivity of ERT and RST below e.g. a high-resistive (high velocity) layer is greatly reduced, and because of the poor ERT tomograms in case of highly conductive layers on top (cf. Hilbich et al. 2009) and it has difficulties in distinguishing between rock and ice (which is partly resolved by prescribing the porosity model). However, the general performance is still better than expected from these considerations, as has been shown in several rather different applications (e.g. Schneider et al. 2013, Pellet et al. 2016), and many other still unpublished case studies.

We believe that this is due to a combination of two frequent biases in ERT and RST tomograms which often compensate: the too shallow layer structure in the ERT (e.g. underestimation of active layer, due to large contrast between active layer and permafrost ice) and the too deep layer structure in the RST (overestimation of active layer, due to e.g. a gradient initial velocity model and/or too coarse geophone spacing). Both biases compensate and are thereby vertically smoothed yielding a surprisingly good result. One could argue that by the combination of three (individually not always reliable) methods (ERT, RST, 4PM) only the dominant features remain in the results, by this reducing the overall bias regarding interpretation. A similar performance as in the present study was found for another large data set (yet unpublished), where a multitude of rock glaciers was investigated with the aid of 4PM and borehole information. So we think that at least for the combination of the inversion programs used here, the good performance is not just a coincidence.

A corresponding paragraph was added in the Discussion chapter.

II.3 The 4PM results for the true synthetic models (Figure 7) are in my view not useful. I guess that the resistivity and velocity values have been chosen, such that they are compatible with the 4PM model. Therefore, the good results should be obvious.

Response to reviewer: yes, that is correct. We originally included Figure 7 to illustrate better the different steps regarding synthetic model, inversion and 4PM analysis. In our revised version we now have omitted Figure 7 and replaced it by a sentence before the discussion of (old) Figure 8 mentioning that the 4PM results for the true synthetic models would by definition give ideal results.

II.4 The authors emphasize the importance of a prescribed porosity model for the 4PM analysis. However, such a model is not available for the field data. How was the porosity model for the field data established, and how trustworthy is it?

Response to reviewer: see our answer to similar comments of reviewer 1 → I.11 & I.12

II.5 Considering (i) the very poor inversion results from the synthetic data, (ii) the simplified assumptions made in the 4PM model, and (iii) the lack of a reliable porosity model raise the question about the significance of the field data results. It seems to me that the authors have provided good evidence for NOT trusting their field tomograms and the associated 4PM models!

Response to reviewer: yes, very good point! As pointed out above in the responses to reviewer 1, our aim was amongst others to evaluate the resolution capacity of this combined ERT/RST/4PM approach for monitoring purposes – with the "standard" geometries/assumptions, which are often used in permafrost research. Now, one result of this study is indeed that these standard geometries/set-ups are often not suitable for e.g. infiltration processes, and that care has to be taken when interpreting this kind of data without checking for possible inversion artifacts or without making sure that the interpreted process can indeed be detected with the approach/geometry/assumptions used. On the other hand, the study shows as well that the combination of ERT/RST/4PM might indeed be more robust against individual errors in the inversions than a single tomogram, as detailed in our response to comment II.2! From the latter, and the experience from earlier studies we do believe that our interpretation of field tomograms and associated 4PM can be trusted – this we have/will also show(n) in several studies where ground truth data from boreholes were available. But we agree, that the more (also theoretical) research is needed to understand in which situations the 4PM gives good results and what are the limitations of its applicability. This may also include a thorough analysis of the applicability of the underlying petrophysical relationships to typical permafrost situations.

A corresponding paragraph was added to the Discussion chapter, see also response to comment II.2.

II.6 The small-scale features introduced in the models SYN2 to SYN4 are obviously beyond the resolution limits of ERT and RST (with the given experimental layout). It might be useful to check with another synthetic simulation, what size of anomaly could be actually resolved. This would be helpful for interpreting the field data results from the Bec-de-Bosson rock glacier.

Response to reviewer: thank you very much for this helpful comment – see also our response to comments I.2 and I.14 of reviewer 1. We added a corresponding analysis with smaller sensor spacing.

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