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Solid Earth
Discussions

Interactive comment on "Electric resistivity and seismic refraction tomography, a challenging joint underwater survey at Äspö Hard Rock Laboratory" *by* Mathias Ronczka et al.

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We like to thank all the reviewers and the editor for their constructive critics that will help to improve the manuscript significantly. In most cases we have additions to the text, one case (going over several points of reviewer 2) we are going to add another figure about the use of different measures (coverage and resolution radius) for appraising model uncertainty. This will, along with the other changes, help to be a guideline to use geophysics in similar settings and thus be of wide interest for the non-geophysical readership of the journal.

In the following we have answered all points by (a) a direct response and (b) by the intended changes to the manuscript at the given position related to the original

manuscript.

Reviewer 1

1) Unfortunately, it is currently not clear, what practitioners and scientists can learn from their survey. The authors present data that was acquired under what they describe as difficult conditions, but it is unclear, what this really meant for the survey and results and what others should learn from it. Should seismics and ERT be standard methods to be used for site investigation under these circumstances or not?

a) As "difficult conditions" we can mention for resistivity the extreme variation in electrode coupling, the conductive water, electrical noise from the nearby nuclear power plant and possibly the 3D-effects on the resistivity modelling. For the seismic the gas bearing sediments are a complicating factor. We recommend the combination of resistivity/refraction methods as a standard tool for site investigations under these geologic conditions. It is clear that the methods complement each other well, and we have references from land. They also complement the traditional drilling well.

b) (P02L32) The main objective was the localisation and characterisation of fracture zones under challenging conditions, which are the extreme variation in electrode coupling, possible 3D-effects on ERT data and high acoustic damping due to gas bearing sediments.

Difficult conditions regarding ERT are explained on P4L07- P4L12. Add: Large variations of the contact impedance can be handled by the used instrument. Nevertheless, this can be problematic for other instruments.

(P10L18) Therefore, a combination of geoelectric and seismic refraction should be used as a standard tool for site investigations under geologic conditions which are similar to those presented.

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2) In the introduction, the authors claim that they are testing geophysical methods to

improve planning for infrastructure projects.

a) This must be a misunderstanding. It is claimed that geophysics is gaining more attention, when it comes to the improvement of infrastructure projects. This is meant as a motivation and not as an objective.

b) (P02L03) Geophysical methods for site investigations gained more attention lately in order to integrate high resolution point information from boreholes, respectively.

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3) Also, only one of the four fracture zones that is geologically known was found using the survey and there is only speculation, why the others are not found. Also, it is unclear, which of the shear zones would be most critical for infrastructure projects.

a) Two of the four fracture zones are merging towards the profile line, thus only three fracture zones might be distinguished. It just can be a speculation, why no contrast in the physical parameter was seen. According to an internal SKB report all fracture zones are partly conductive but different in size. As all fracture zones are hydraulically conductive, they should have been detected if the model resolution would have been sufficient, but a complicating factor is the highly conductive sediments with most likely higher contrast in resistivity that may mask the fracture zones.

b) (P03L28) According to Wikbert et al. (1991) all fracture zones are at least partly water bearing. They also gave a judgment of the fracture zones according to Bäcklom et al. (1990). Based on that, the most critical fracture zones along the measured profile is NE-1, which is judged as “certain”. EW-3 is also judged “certain”, but hydraulically of minor importance. NE-3 and NE-4 are judged as “certain” as well. Both consist of several one to a few metres wide subzones, of which some are open fractures that are hydraulically highly conductive. In general, the fracture zones NE-3, NE-4 and EW-7 is judged to be “probable” in a hydraulic sense (Wikberg et al. 1991).

(P09L31) Only the NE-1 fracture zone could be identified by this survey, although the

fracture zones NE-3, NE-4 and EW-3 are as well partly water bearing according to Wikberg et al. (1991). As shown in Figure 1, NE-4 and EW-7 are close to each other at the profile line, which means that they can be most likely not separated in the inversion results. The low resistive sediments are a complicating factor with most likely higher contrast in resistivity that may mask the fracture zones by reducing the resolution such that it is not sufficient to resolve the fracture zones NE-3, NE-4 and EW-7.

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4) However, the strengths and weaknesses of the different methods in conditions of crystalline rock in combination with high-conductivity water are not clearly discussed.

a) While conducting underwater ERT parts of the current system will flow through the water, which can be included in the inversion. Thus only the loss of investigation depth is a weakness of the ERT method under these conditions. Seismic is unaffected by water as the refracted wave travels in the medium with higher velocity.

b) No change.

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5) It is also not clearly discussed why the current study is needed and what it should add, compared to the existing ones.

a) This is a representative case study for the combination of geoelectric and refraction seismic in typical Scandinavian geologic conditions at a coastal region. There are references for combining refraction/ERT on land, but we didn’t find a case study that combines refraction/resistivity in a marine environment. This survey was also a test for a joint inversion of ERT and seismic data. It shows the improvement of the results compared to a single method approach for these conditions.

b) We will rephrase the given explanation and add this to the manuscript. (Point 3-5 is mainly an improvement of the introduction)

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6) Generally, the introduction should summarize the current state of research and discuss gaps that are addressed in the manuscript. Here, it is not clear, which gaps are being addressed and how.

a) The gap that we want to address is the methodical approach: joint inversion will help to get more reliable results compared to single method approaches. That shall make interpretation easier or at least more unique.

b) This will be pointed out more clearly. (End of introduction)

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7) Furthermore, the introduction very much targets Sweden, while the study should be of more general interest to be published in an international journal.

a) Correct, we only referred to Swedish studies.

b) We will add some references for engineering projects in other countries that dealt with crystalline rock as hard-rock can be found in many countries.

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8) The site description is a very short description of all the work that has been performed at the Äspö HRL. Unfortunately, information that would be important for the current study is missing: Previous seismic surveys in the rock lab shown? Was no surface seismic data acquired in the region? If other surface seismic data J. S. Kim, Wooil M. Moon, Ganpat Lodha, Mulu Serzu, and Nash Soonawala (1994). "Imaging of reflection seismic energy for mapping shallow fracture zones in crystalline rocks." GEOPHYSICS, 59(5), 753-765. doi: 10.1190/1.1443633 Cosma, C., Olsson, O., Keskinen, J., Heikkinen, P., 2001. Seismic characterization of fracturing at the Äspö Hard Rock Laboratory, Sweden, from the kilometer scale to the meter scale. International Journal of Rock Mechanics and Mining Sciences, ApplicationÂËŸa of Geophysics to Rock Engineering 38, 859–865. doi:10.1016/S1365-1609(01)00051-X Which seismic velocities are found in the rock lab for intact rock and how does it

change in fracture zones? What is the electrical resistivity of intact rock and of fracture zones? How likely are sedimentary deposits in the region? Have other studies or evidence on land shown extensive sedimentary deposits?

a) Actually, peer-reviewed articles about Äspö being relevant for this study (type of parameters etc.) are rare. Relevant previous results can mostly be found in SKB reports.

b) (P08L10) . . . of about 5600 m/s, which agrees with the velocity for intact crystalline rock at Äspö HRL given in Wikberg et al. (1991). Brodic et al. (2016) showed recently that the velocity decreases from >5000m/s for intact rock down to 4200 – 4700 m/s. Brodic B., Malehmir A. and Juhlin C. ; Fracture System Characterization Using Wave-mode Conversions and Tunnel-surface Seismics, Ext. Abstr., , EAGE Near Surface Geophysics, 2016

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9) For the measurement techniques, some problems are described, but not really discussed how to be solved. For example: contact resistances vary by a factor of more than 1000. What does this mean for the measurements?

a) Large differences in the contact impedance (quality of the electrode grounding) can lead to oversteering of the signal, but this is handled by the used instrument which has input channels with automatic gain that can handle the full transmitter output signal on each individual channel. Since the input channels are galvanically separated one channel can have high gain and the next channel low gain without any common mode error problems. Unknown sharp boundaries between high and low resistivites are difficult to fit/image because smooth constrains of the inversion. But especially the last point can be included in the inversion by allowing sharp contrasts at known boundaries.

b) Answer already given in point 1).

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10) For the positions: electrode positions were apparently measured using differential GNNS. How did this work for the underwater survey? What was the water depth? Does an accurate bathymetry model exist? Was this bathymetry model used, or was the multi-beam echo sounder used? Was the multi-beam echo sounder used to measure 3D ocean bottom topography?

a) Positioning is a very important point. An MBES survey was performed in order to map the sea floor topography very detailed. The result is a very accurate DTM (digital terrain model) that has been used in the modelling. Positions of sensors (E/S) are coincident and measured with sufficient accuracy. Water depths are small (<10m) and due to this a GNSS position of each shot point (20m spacing) has been enough.

b) The bathymetry model used for getting the heights for sensor-positions, but not for modelling.

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11) With a difference in resistivity of more than a factor of 10000 between the water and the rock, it seems very important to know the position of the interface accurately. Were electrode positions deleted because they were in the wrong place, or simply, because the GNNS positioning did not work accurately?

a) The exact positioning is very important, but this was not a problem because of the precise Bathymetry measurements. Thus no electrode positions were deleted due to poor positioning.

b) (P04L08) A model based on accurate bathymetry measurements was used to determine the heights of the sensor positions at the seabed.

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12) For seismic refraction tomography: Crystalline rock is a perfect target for reflection seismic surveys, as shown in many studies. Why is only refraction seismics being used here? Low velocity zones – especially if they are just thin fracture zones – are very

difficult to image in refraction tomography from the surface. Reflection surveys in the area (see references above) have shown the great potential of seismic surveys under these circumstances. If the potential of geophysical methods should be addressed here, why not process the data for both reflections and refraction and compare the results?

a) The depth to bedrock is one of the two main targets. This can't be given by P-wave reflection at these small depths. According to Cosma et al. 2001 reflection seismic was rarely used for crystalline rock especially for shallow depths. The wavelengths are too long and in a high velocity environment we need some distance before we get a proper reflection. This is of course what we get from the refraction seismic. Weakness zones and fractures are the other main target. The Kim et al. (1994) paper presents material that shows that these can be mapped by reflection seismic. However, there is no information in the top ~50m of the bedrock from reflection seismic. Another arguments against reflection in our case: Our study aims at urban environments. Reflection lines need to be much longer and it is unlikely that we can count on getting space for this in most cases. If we could, then there would be a much higher cost for performing reflection seismic. It is not common to get that kind of budget in Swedish infrastructure projects.

b) No changes in the manuscript.

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13) The section on inversion is somewhat confusing and not fully consistent: For example, Equations 1a, 1b and 2 do not agree. Using eq. 2, the weighting matrix is missing in 1b. Also, according to the text, the model vector contains logarithmic resistivities. What about the slowness or velocity? Concerning the use of different norms: Most inversion schemes (and I think the one of Günther 2006b is not different) use a reweighting scheme to implement a L1 norm.

a) Equation 2 is an alternated version of Φ_{m} from equation (1a) and (1b) and thus just extended with the weighting matrix W_{c}. That is the key point of this joint inversion. But this is a little bit inconsistent.

b) We will rewrite the paragraph and the second equation to make this clear. It will be mentioned explicitly that the logarithms of the slowness is used. We will rewrite the equations and remove the L1 norm in the text as it is confusing. We will also include some more more information about the joint inversion.

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14) The discussion of the weighting matrix and the introduction of joint inversion on P5L15-22 is confusing and not well motivated. Why should a joint inversion be attempted in the first place? What could be the advantages here? What are the difficulties with single-method inversions that could be overcome by joint inversion?

a) This comment is related to point 5). This survey was designed in order to perform a joint inversion which should be tested. It was also mentioned that model ambiguities can lead to misinterpretations. These can be reduced by a joint inversion, because more data and more information (model of another method) will influence the quality of the inversion result. The presented results clearly support this statement, which is also discussed. (See also answer to 5)

b) Although it is already written in the manuscript, we will put more emphasize on this in the introduction. One sentence in section 3.1 → depth of penetration.

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15) The reference of Günther et al. 2010 is rather difficult to access. Why not give the actual equation here?

a) Mathematics and physics of a joint inversion is not the scope of this paper, that's why an extended abstract is referenced and a more demonstrative explanation for the joint inversion was written.

b) No changes in the manuscript as this would only be a repetition of the mentioned

paper.

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16) Synthetic study: It is interesting, but not surprising to learn from the synthetic study that 3D effects under these circumstances can be severe. The authors conclusion is that "ERT data gathered at the Äspö test site are contaminated" and that cautions should be used for the interpretation. However, if the conclusion is that the data is contaminated, the conclusion should be that one needs to take the 3D effects into account in the 2D inversion or perform a 3D survey. Simply saying that one needs to be careful in the interpretation is something one could say even without the synthetic study. I suspect that not only the island created artefacts, but also bathymetry changes in the area of the profile effect the results.

a) This is a quantitative measure for the 3D effects here and how they perform with parameter contrasts that we have in Äspö. It makes it more clear how strong those effects really are. Especially when non-geophysicists will read this paper, it helps to understand the difficulty by visualizing it.

b) No changes on the manuscript.

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17) A more interesting synthetic study would be to compare the current density in the rock and in the water. With a resistivity factor of more than 10000, I would suspect that only very little current flows through the rock, which directly limits the sensitivity and makes small uncertainties in the water geometry a major problem.

a) True, but ERT is also driven by driving away the current, that's why you can better see high resistivity anomalies. The positioning of the electrodes is getting more important with an increased resistivity contrast. But as we have excellent control about the sensor positions for the underwater part by bathymetry measurements, this is not assumed to be a problem

b) Currently, we perform a synthetic study, where a couple of sensor positions are moved by 30-40 cm in z-direction while using with a resistivity contrast of a factor of 1000. Thus the importance of the positioning in combination with large resistivity contrasts can be seen.

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18) Would it not be possible to use a 3D forward model that incorporates the 3D bathymetry and still invert for a 2D model along the profile? For the water, one could simply use the known water resistivity and for the rock far away from the profile, one could use the known rock resistivity. Would it not be possible to invert for one single water resistivity value? Is the measured resistivity that at the ambient temperature, or normalized to 25 degrees?

a) Using the bathymetry model could be done, but it is time consuming. It is possible, by setting the water to a single region, which allows only one resistivity for the whole region. But the resistivity of the water was measured with a minimised Wenner measurement using the Terrameter LS. Thus, no extra temperature correction is needed, as no conductivity meter was used. As the true water resistivity is known, it doesn't make sense to invert for this.

b) No changes on the manuscript for that comment as a detailed bathymetry would confuse the readers and provide no general conclusions about the strength of the 3D effects.

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19) It might be co-incidence and actually makes geological sense, but the thick sediment deposit lies at the position with the greatest water depth and thus could be influenced by the water depth.

a) The topography shows that this is indeed the deepest point (relative), but the absolute depth in comparison to the investigation depth is not so high. Especially in relation to profile length. Seismic also validates this by a low velocity zone. So we consider a

geometric error very unlikely.

b) It is not planned to make changes according to this comment.

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20) Results: How do the values compare to those expected from measurements at the HRL? How do they compare to the previous surface seismic lines? What else is known about the fracture zones?

a) Correct. We have velocities for the bedrock from a report, which agree with our bedrock velocity. There is an extended abstract regarding seismic measurements from Uppsala University, which gives some velocity information as well. We didn't found any reference data for resistivity. For the fracture zones, no velocity or resistivity information was found. Only chemical analysis of the fracture fluid and the filling material could be found, but those are not relevant for surface ERT and seismic measurements.

b) This comment is strongly related to comment 8). With the mentioned report and extended abstract as references this point can be answered as well.

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21) Here geophysical results are presented, but without incorporating ground truth from e.g. boreholes.

a) According to Wikberg et al. (1999) there should be a borehole penetrating fracture zone NE-1. Up to this point we couldn't find geophysical reference data from them, only geological reference data. The tunnel gives excellent ground truth for the positioning of the zones, but no information about ground truth on depth to bedrock was found.

b) No changes on the manuscript. (Some sentences about the type of reference data?)

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22) Are there any other indications of this sediment deposit? Are any drill holes available in the area? From other studies in the area: Are similar deposits known to exist? Are they likely, given the geological history?

a) Especially this sedimentary deposit was not known before (written in the text) and no other surface measurements were done that cover a lake. That's why no other deposits are known. A discussion with researchers from the geologic department and a SKB report supports that this scenario is geologically reasonable.

b) Dahlin et al. (2014) and Dahlin et al. (2016) also found sediments on the seabed near Stockholm. We will add these references that shows and supports the general opinion that sedimentary deposits on the seabed are typical for this region. Dahlin 2014 reference... Dahlin 2016 reference...

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