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Interactive comment on "Combining cross-hole geophysical and vadose zone monitoring systems for vadose zone characterization at industrial contaminated sites" *by* N. Fernández de Vera et al.

Anonymous Referee #2

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The manuscript describes a methodological concept for vadose zone subsurface characterization by combining point measurements and information from cross-hole ERT methods. This setup is tested at an industrial contaminated site in Belgium. In general I think the manuscript has an interesting topic, and promises a nice topic to combine geochemical and geophysical information, which would fit into the scope of HESS. However, in my opinion at the moment, this goal is not reached and major parts of the manuscript have to be clarified substantially before considering it for publication. Additionally, there I have major questions especially concerning the ERT data setup and the structure of the manuscript. Consequently, in its present state the manuscript does not reach substantial conclusions and requires to be resubmitted in a restructured and

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Discussion paper

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improved form.

1) The authors claim to combine geophysical and vadose zone monitoring systems, but they do not include a geophysical monitoring. They rather describe the results of classical borhole ERT survey, which is interpreted in terms of structural features. In my opinion this results are not state of the art, as they do only invert 2D section, where a 3D inversion of the six ERT borehole profiles should be possible. In addition I would expect to see a real geophysical monitoring e.g. by time-lapse ERT measurement as they authors claim to present a geophysical monitoring system. Currently, they show only a structural interpretation of the ERT data, which is not very different to the already known information from the borehole logs (Fig 3). Therefore, it is unclear from the maniscript, which advantage can be generated by using the ERT data.

2) Major methodological information of the ERT setup and inversion major information are missing or are hidden in the results section (e.g., the electrode configurations, inversion algorithm and parameters). In addition the ERT data in Fig. 3 are not consistent with the colorbar information, while the colorbar in Fig. 5 is missing completely.

3) The data ERT data show obvious artifacts close to the boreholes, which the authors discuss to be ignorable like it can be done in very high resistive environment. I consider it very questionable if this observation can be transferred to the present setting. In addition the used citation (Deiana et al., 2011) is neither from an ISI listed journal, nor openly accessible. Moreover, from my point it seems these artifacts effect large areas of the ERT data, resulting in a questionable interpretation of the ERT data. Taking into account the previous points, it is impossible for me to comment on large parts of the manuscript as substantial technical information are questionable or missing.

4) In the soil moisture data, the authors observe indications of preferential flow in a fracture network, However, such structures are known to be difficult to be observed by potential methods like ERT. Maybe an structural imaging method like GPR can provide better results in such settings. An exemplary study from a similar application is: S.

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Truss, M. Grasmueck, S. Vega, and D. a. Viggiano, “Imaging rainfall drainage within the Miami oolitic limestone using high-resolution time-lapse ground-penetrating radar,” Water Resources Research, vol. 43, no. 3, pp. 1–15, 2007.

5) Most of the chemical analysis described in chapter 4.3 and 4.4 indicate that Nickel is being transported through the vadose zone which is related with pyrite oxidation at the top, while other heavy metals are not detected. In my opinion, this part not well structured making it difficult to follow the storyline. In addition, it is unclear, why the transport of Ni from the backfilled material is important in the context of preferential flow path and spatial resolved structures. This should be pointed out in more detail in a separate discussion chapter, which clearly discusses the connection of the chemical data with the spatial information and the knowledge of preferential flow behavior.

6) The information in Fig 6 and Fig. 8 is largely identical except for the Ni concentration added to Fig 8. Here it might be possible to condense these plots. Overall I found the plots have to be improved and should be better integrated into the text. Therefore, I suggest more to add more figure references in the text and highlight the areas of interest in the plots.

7) In general I miss the combined discussion or methodological combination of the methods as promised in the title and the abstract of the manuscript. Here, I expect a combined interpretation and discussion of the applied methods and their limitations, which should be added to the manuscript.

8) References should be reworked with respect to ISI listed journals and accessibility.

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