We would first like to thank Anonymous Referee #2 (referred to hereafter as AR2) for reading, commenting and evaluating our manuscript. We highly appreciate the overall positive comments that highlight the potential impact of our research on this topic. We are grateful for his/her encouraging appreciation, which gives us additional confidence in our work. We also sincerely thank AR2 for pointing out inconsistencies and poorly formulated statements, as well as for giving us important advices to improve our paper. We agree with most of AR2's comments and have therefore proposed revisions that we feel will lead to an improvement in the quality and clarity of our manuscript in the revision phase. We have included all AR2's comments below (in red, including also all comments in the AR2's pdf annotated manuscript), followed by our responses (in black). Note that our proposed revisions are highlighted in bold text in our response to AR2's comments.

1 – General comments

General comments – Quite well written, with deep numerical and field application studies as well as practical consequences for geophysicists, this paper deals with CZ regolith thickness characterization with ERT, issue that is a very important problem for geophysicists because they have to "deliver" reliable geometries for hydrological modelling. It is of interest for hydrologists. The paper doesn't perfectly fit with HESS journal* because, basically, it is a good technical paper dedicated to geophysicists who want to improve the geometry knowledge of some regolith structures in one dimensional situations, with an original approach. This objective is important for the community of Critical Zone, because regolith structure and thickness/properties are key parameters. Taking into account the above remarks, I would suggest the authors either a) to change their introduction to draw more attention to the implication of their study for hydrology and probably, in the discussion part, develop more arguments to show how their geophysical method improvement brings a definite advantage compared to large ESI survey for getting more accurate results for hydrological modeling (i.e what is the sensitivity of hydrological model to thickness determination of the regolith) b) Submit their paper to a more specialized (geophysical) journal. An alternative way would be to present the paper as a case study explaining more about the site and its hydro(geo)logy, then presenting the complete survey (all 2m ERT lines), and the difference in regolith volume with and without shallow interpolation, and the consequence in the hydrological modelling. *"fundamental and applied research that advances the understanding of hydrological systems, their role in providing water for ecosystems and society, and the role of the water cycle in the functioning of the Earth system".

In his general comments, AR2 qualifies our study as well-written, with an original approach leading to practical applications for geophysicists and an objective that is relevant for hydrologists and the critical zone research community. However, AR2 also states that our manuscript is a good technical paper dedicated to geophysicists, but that it eventually doesn't perfectly fit to HESS. We are convinced that our research work provides results and findings that are of interest to and can be used by the wide research community targeted by HESS (not only geophysicists). In addition to many references already listed in the introduction of our article, several authors have very recently pointed out the subsurface as being the greatest knowledge gap in the understanding/modeling of hydrological processes, with a greater investment into "seeing" the subsurface needed to provide the Earth System

Modelling community with critical guidance on how to parametrize model subsurface structure depths and properties (Fan et al., 2019). This general framework strengthens our belief that the topic of our work is well-suited to HESS. **To better fit HESS' scope and audience, AR2 suggests incorporating new elements into our manuscript to draw more attention to the implication of our study on hydrology and, more specifically, to better highlight the potential added value of our novel approach for hydrological modelling purposes. We agree with and plan to follow this advice. If our work is accepted for publication in HESS, we will revise the introduction and discussion sections of our paper to make it as relevant as possible to HESS's wider hydrologist / critical zone scientist audience, according to AR2's recommendations.**

2 – Title

Specific comments – Title: "Large-scale ERT surveys for investigating shallow regolith properties and architecture": I would suggest to remove the word "shallow" because the paper deals also with deep interfaces. Also, the word "large scale" is not adapted because you deal also with small scale surveys that are entire part of your methodology. At last, "properties and architecture" is not adapted: what is it exactly "properties" and "architecture"?: consider re-formulating the title.

We agree on this statement! **This will be done in the revised version of our manuscript.**

3 – Abstract

Specific comments – Abstract Note 1: The sentence :"However, ERT measurements with a high vertical resolution remain restricted to shallow depths, essentially due to the requirement of small electrode spacing increments (ESI)" should be modified: High vertical resolution is effectively restricted to shallow depths due to the intrinsic physics of the method, not to the electrode spacing. Diminish electrode spacing is a "only" way to get more dense shallow current lines with a better lateral sampling and thus improves the capacity of the method to describe shallow electrical structures.

Specific comments – Abstract Note 2: " . . . a thin surficial layer can influence inverted ERT results ïC˘ Ÿ Please describe briefly this influence "and cause a resistivity bias, " ïC˘ Ÿ idem ". . .both at the surface and at deeper horizons. "To overcome this limitation, we propose adding interpolated levels of surficial apparent resistivity based on a limited number of ERT profiles with small ESI". ïC˘ Ÿ This sentence is quite unclear : what "interpolated" means?

Other comments in the AR2's pdf annotated manuscript – abstract: page 1 line 16: "perception" define what you mean; page 1 line 18: "resistivity bias" resistivity or apparent resistivity?

We understand and agree with the inconsistencies in our abstract that AR2 pointed out. As these lacks of rightness/clarity mirror inconsistencies pointed out by AR2 further down in the core of our manuscript, please see below for our more detailed responses to AR2's comments. **We will reformulate the abstract in the revised version of our manuscript, following AR2's recommendations.**

4 – Introduction

Specific comments – Introduction Note 3: “Conventional investigation techniques”: conventional is too vague. Geophysics could also be “conventional”, at least used by many CZ teams on watersheds

We agree! **This will be corrected in the revised version, for instance, by adding between brackets “(i.e. soil pits, drillings)” and/or using the qualifying adjective “intrusive”.**

Specific comments – Introduction Note 4: “The characterization of subsurface properties and the delineation of layer boundaries. . .” the word “layer” may not be appropriate: it refers to layered ground. ERT is more dedicated for 2D and 3D structure. You should consider change this word throughout the all text. Or explain why you are using ERT for 1D structure instead of “vertical” electrical soundings.

As highlighted by AR2, we understand that the use of the word “layer” may not be appropriate in this section of the manuscript. On the other hand, we think that it can be used after the introduction because we clearly what the layers correspond to in the context of our study in the Materials and methods section. **In the revised manuscript, we will replace “The characterization of subsurface properties and the delineation of layer boundaries” with “The characterization of subsurface properties and the delineation of structural units within it” to avoid any misinterpretation.**

Specific comments – Introduction Note 5: “In many cases, the subsurface structure to be characterized is shallow and should be measured with a precise vertical resolution, thus requiring a small electrode spacing increment (ESI) (Reynolds, 2011; Chambers et al., 2014)”. This statement, already written in the abstract, makes me quite confusing. And as it is the main motivation of the paper it requires to be explain. â˘A ´c All the subsurface structure should be ideally characterized, whatever the depth, not only at shallow depth. . . â˘A ´c “precise vertical resolution”: what is “precise” to you? â˘A ´c And “vertical”? â˘A ´c “thus”: this is probably the confusing word: the link between electrode spacing and the vertical resolution at depth does not exist. Small electrode spacing allows to describe more precisely shallow structures. â˘A ´c You should rewrite as follows, as a suggestion (?): “When shallow structure are under interest, we need to consider small electrode spacing. However, when making long (large) surveys, small electrode spacing are not considered because it is time consuming. Therefore the questions rise: are deep structures well defined (within the limits of the intrinsic limitation of the method that is a decrease of the resolution with depth) if the shallow structure is not well sampled? How can we enhance our knowledge of shallow structures when making long survey?

AR2 rightly points here to the fact that the vertical resolution of ERT images is primarily linked to the intrinsic physics of the method and not to the electrode spacing (see also AR2 note 1). We are of course aware of this fundamental decrease of resolution with depth. However, electrode spacing also plays a role in the vertical resolution of ERT images and it is this information that we were trying to convey. Indeed, as mentioned by AR2 (see note 1), by reducing electrode spacing it is possible to get denser current lines (and thus denser and more well-discretized apparent resistivity measurements), both in the shallow subsurface

and further deep. We do however agree with AR2 that this apparent resistivity measurement density is essential to improve the capacity of the ERT method to describe shallow electrical structures (see AR2 note 1) and that its impact becomes increasingly negligible with depth (see AR2 note 5). That being said, we fully recognize that we must be more precise/correct in our explanations about this point as the main motivation of our work is indeed to fill the lack of shallow apparent resistivity data if an oversized electrode spacing is used. **Consequently, note that we will reformulate our explanations, research questions and the objectives we are targeting to respond to AR2's judicious criticism and recommendations more clearly in the revised manuscript.**

Specific comments – Introduction Note 6 : here you are announcing a layered model. It is not clear why you are dealing with 1D model using a 2D-3D measurement protocol method. Again, why don't you use only a "vertical" electrical sounding? You should explain somewhere why the 1D geometry is under study.

As stated in our response to AR2's Note 4, we understand that the use of the word "layered" may not be appropriate in this section of the manuscript. **In the revised manuscript, we will replace "a set of synthetic three-layered soil–saprock/saprolite–bedrock models" with "a set of synthetic soil–saprock/saprolite–bedrock models" to avoid any misinterpretation.**

Other comments in the AR2's pdf annotated manuscript – introduction:

page 2 line 6: "Fortunately" useless

We agree! **This will be corrected in the revised version.**

page 3 line 8: "resistivity bias in depth" you should precise it is the result of inversion process, which can affected by a lack of shallow data

We agree! **This will be fixed in the revised version**.

page 3 lines 9-10: "What happens if a larger ESI is used?" what happens to what?

We agree! **This question will be re-phrased in the revised version**.

page 2 line 10: "dealt" addressed

We agree! **This will be corrected in the revised version**.

5 – Materials and Methods

Specific comments – Materials and Methods Note 7: "To cover a sufficiently wide range of subsurface structures and properties," Actually, the reader doesn't understand exactly from which situation those models are derived. When talking about "structures", it is actually only "layers". The range of resistivity has also to be explained: what could be the soil characteristics you are considering? In short: why you choose a) a 1D model, and b) such range of thicknesses and resistivity values? You should more clearly say that your model are derived from a field question. And therefore, the case you are dealing with is a specific one, and the conclusion derived will be related to your case study and similar structures

We agree with AR2 that we have to state more clearly that our set of synthetic models has been selected according to a specific field question and thus to respond to a specific problem. **To counter this criticism, we plan to rearrange the Materials and Methods section by placing the description of our field study before the description of the set of synthetic models, and clearly stating that the latter is derived from the former. We will also specify that we have opted for the use of 1D synthetic models and inversed them in 2D in order to evaluate not only the accuracy but also the precision of the delineation of interface depths (this would not have been possible using a 1D inversion).**

Specific comments – Materials and Methods Note 8: "the vertical resolution needed to properly characterize the subsurface". You need to explain what you are considering with "vertical resolution" and "properly". It is compulsory to understand what objective you are targeting.

We understand and agree with AR2's comment. **In the revised manuscript, we will replace "In order to assess the vertical resolution needed to properly characterize the subsurface, simulations of apparent resistivity for both arrays were conducted using 5 different ESIs (0.25, 0.5, 1, 2 and 4 m)." with "In order to assess the effect of the lack of shallow apparent resistivity measurements related to the choice of ESI, simulations of apparent resistivity for both arrays were conducted using 5 different ESIs (0.25, 0.5, 1, 2 and 4 m)."**

Specific comments – Materials and Methods Note 9: "ERT survey design": the design is also derived from synthetic modelling or not? If yes, say it.

Our ERT field survey design was not derived from synthetic modelling results. On the contrary, it is the lack of apparent surface resistivity measurements using an electrode spacing of 2m and the resulting biased ERT inverted images obtained for our field study that motivated us to conduct this study. **This will be more clearly specified in the revised version (see our response to AR2's specific comment Note 7).**

Specific comments – Materials and Methods Note 10: the processing of field data is very well described. However, this part should be reduced to shorten the paper, because there is no technical implication for the presented study: ie. the conclusion of the paper doesn't depend on this careful process, apparently. If I am wrong, ignore my remark.

We will follow AR2's recommendation and shorten our description of how we took care to check/ensure the "good quality" of apparent resistivity measurements taken in our case study.

Specific comments – Materials and Methods Note 11: those effects have not yet been described, and the reader is expecting such a description much earlier to be convinced that your work is important.

We agree with AR2's comment. **This will be fixed in the revised version of our manuscript (see our response to AR2's specific comment Note 8).**

Specific comments – Materials and Methods Note 12: "We use these subsets to fit four linear regressions between the apparent resistivity data for external electrodes separations of 1.5,

2.5, 3.5 and 4.5 m respectively, and those of the first acquisition level measured with an ESI of 2 m." The problem here is to understand, without figure, how you propose to "fill" the 2m spacing data sets with additional extrapolated data. The concept of "linear regression" in particular is not clear because apparent resistivity data values (see a 1D sounding curve, and see several curves with different contrasts) doesn't follow linear behaviour with increasing electrode spacing. A figure, or a scheme is necessary. Consider also the case with a conductive second layer: in this case, the resistivity of the second layer could have an influence on the regression : is it still linear? Moreover, this part regarding the filling of sparse data set could be explain later in the paper, once the reader has discovered the synthetic ERT results.

As highlighted by AR2, we recognize that our proposition to upgrade apparent resistivity datasets measured with an oversized ESI (and thus which are characterized by a lack of shallow data) is not explained clearly enough and must be improved. **To meet AR2's criticism and suggestion, we propose only defining/clarifying our novel general upgrading idea here (with the help of an additional figure), that is, to a) explore if and how surficial apparent resistivity measurements at low ESI can be derived from the most surficial apparent resistivity level using larger ESI (i.e. for its minimum array extension) and b) if such relations exist, using them to fill the lack of shallow data when using the oversized ESI and evaluating the added-value in terms of inverted ERT image accuracy. As proposed by AR2, the appropriate use of linear regressions, which is specific to our context or similar context, will be explained later in the paper, once the reader has discovered the synthetic ERT results.**

Specific comments – Materials and Methods Note 13: inversion process description. One of the main question also is the size of the triangular cells you considered. What is the influence of the meshing to the resolution of the shallow subsurface? As shallow resolution here is the main point, you should tell more about this.

We understand AR2's comment. **To repond to the criticism, we propose replacing the sentence "To facilitate the comparison between the resulting interpreted resistivity images, and because inversion results are to a certain degree mesh-dependent (Günther and Rücker, 2016), the same fine mesh (whose resolution suits the smallest ESI) was used for all inversions." (page 7 lines 27-29) with the paragraph "Finally, it is important to note that particular care has been taken in discretizing the models. Indeed, following the standard automatic meshing in the inversion code, the larger the ESI, the coarser the mesh would have been (Günther and Rücker, 2016). As shallow resolution is the main point of our study, and because inversion results are to a certain degree mesh-dependent, the same fine mesh (whose resolution suits the smallest ESI according to Günther and Rücker (2016)) was used for all inversions in order to avoid any coarse meshing issues in the comparison between the resulting interpreted resistivity images."**

Other comments in the AR2's pdf annotated manuscript – Materials and methods:

page 3 line 26: "ill-posed inverse problem effects" You have to describe those "effects" to satisfy the need of non-specialist readers.

We agree. **This issue will be fixed in the revised version. Here we propose replacing "ill-posed inverse problem effects" with "ill-posed inverse problem which in our study is related to the lack of shallow apparent resistivity data induced by the use of an oversized ESI". In the introduction of the revised manuscript, we will also explain more precisely that a) the inversion process that leads to interpreted ERT images from apparent resistivity measurements can suffer from a lack of apparent resistivity data resulting in biased inverted ERT images, and b) our study concerns the impact of the lack of shallow apparent resistivity data induced by the use of an oversized ESI on ERT image accuracy.**

page 4 lines 2-15 "2.1.1 Conceptual resistivity models" this clearly has to be placed after the site description. If not, we don't understand why you choose this set of selective models.

We agree. **This will be done in the revised version**.

page 4 line 17: "To mimic resistivity measurements" apparent?

"To mimic **apparent** resistivity measurements". **This will be corrected.**

page 5 lines 2-32"2.2.1 Study area description" Ok for what is written below. but you should add information on how the resistivity is changing with depth for solum and subsolum and rock? sharp or smooth variations?. what are the consequences for your modelling excercise?

We understand AR2's comment but we did not take borehole resistivity measurements and thus cannot deliver such precise information at this stage of the manuscript. In addition, is this really useful, since readers will discover the results of the 12 surface ERT profiles in the Results section? **However, to respond to AR2's criticism, to a certain extent, we propose informing the reader here that, given the intrusive point-scale investigation knowledge, the solum/subsolum resistivity interface is expected to be sharp and the subsolum/hard bedrock one most probably more gradational.**

page 6 lines 4-5: 2 m and 0.5m: choice derived from the synthetic modelling? why not 0.25 and 1m also?

No, these choices were not derived from synthetic modelling but on the contrary, lead to our study. **This will be specified in the revised manuscript (see our response to AR2's Note 7).**

page 6 line 7: jointly with the 2m survey also within those plot, no?

Yes and no, because only the 12 plot scale ERT profiles using an ESI of 0.5m are specifically subjects of analysis in our study. **This will be clarified in the revised version**.

pages 6-7 lines 33-2: ", we propose to take advantage of the correlation between this first acquisition level and additional surficial apparent resistivity acquisition levels (i.e. quadrupoles with smaller external electrodes separations) obtained from a reduced number of ERT profiles with a smaller ESI." Not clear. Not clear at all.

See our response to AR2's Note 12.

page 7 lines 2-4: “If the top layer has a rather constant thickness and resistivity, the correlation could then be transposed to areas where the larger ESI have been used and where data gaps prevail in the shallow subsurface.” How do you control this hypothesis along the entire ERT large ESI survey?

For our specific field study, it is our knowledge of the catchment, acquired through several intrusive point-scale investigations (more than 40 drillings and soil pits) and the judicious location of our 12 plot-scale ERT profiles using an ESI of 0.5m (chosen according to prevailing local geomorphological characteristics i.e. plateau, steep and gentle hillslope, interfluve, close to the riparian zone), which allows us to reasonably assume this hypothesis at the catchment scale. As already mentioned in the Discussion section of our manuscript, our confidence in this assumption is an important point for the application of the proposed approach.

page 7 lines 2-4: “If the top layer has a rather constant thickness and resistivity, the correlation could then be transposed to areas where the larger ESI have been used and where data gaps prevail in the shallow subsurface.” what is this correlation exactly?

See our response to AR2’s Note 12.

page 7 lines 12-13: "We use these subsets to fit four linear regressions between the apparent resistivity data for external electrodes separations of 1.5, 2.5, 3.5 and 4.5 m respectively, and those of the first acquisition level measured with an ESI of 2 m.": not clear at all.

See our response to AR2’s Note 12.

page 7 line 27: “regularization parameter λ of 20” why this value? seeking for horizontal sharp interfaces a lower lambda should be tried...

It is correct that interfaces in our synthetic models are sharp and a lower lambda might be preferred (and even an L1 model constraint instead of an L2!) but this is not exactly the case for our field study. Although the solum/subsolum boundary was expected to be relatively sharp, the subsolum/hard bedrock interface had a more gradational character. Moreover, the closing with depth of the fractures in the hard bedrock also implies potential gradational changes in resistivity. On balance, an L2 model constraint with a moderate lambda value of 20 was therefore thought to provide a good compromise. **This choice will be explained in the revised manuscript**. It is worth noting that this choice has also been strengthened by preliminary tests using varying lambda values.

page 8 line 3: “observed data” actual not observed

We agree. **This will be corrected**.

page 8 line 3: “predicted data” calculated

We agree. **This will be corrected.**

page 8 lines 3-4: “(interpreted resistivity)” calculated resistivity from inversion process

We agree. **This will be corrected.**

page 8 line 7: “We compared the true interface depths with those that can be derived from inverted ERT images” How do you derived interface from an interpreted ERT model with smooth logic?

Derivative methods have already been used successfully in other ERT studies, even when using an L2-norm (smooth) model constraint (e.g. Hsu et al., 2010; Chambers et al., 2012; Ward et al., 2014). **This information will be added in the revised manuscript.**

page 8 lines 12-13: “Derivative methods assume that interfaces are located where changes in image properties are at a maximum” Yes, but what is the influence of the smoothing factor used in inversion parameters? this can produce very different gradients...

We agree with AR2 that the smoothing factor may to some extent influence the interface delineation using derivative methods, as is the use of either an L1-norm or an L2-norm. Quantifying such influences may deserve a fully-fledged study, but this is not the objective of our work. Since we demonstrate good results when using small ESI (for the synthetic models and our field study, using the same inversion parameters in both cases), our conclusions in terms of ill-posed inverse problem effects caused by ESI parameter-related choices are not impacted by such potential influences.

page 8 lines 19-21: “Note that we sometimes manually manipulated the data to ensure the continuity and horizontality of the interfaces (i.e. merging several zero contours and removing conflicting data)”. which data? apparent res data? or calculated res?

It is the derived interfaces that have been manipulated, not the resistivity data (apparent or calculated). **We propose replacing “Note that we sometimes manually manipulated the data to ensure the continuity and horizontality of the interfaces (i.e. merging several zero contours and removing conflicting data)” with “Note that we sometimes manipulated the data manually (i.e. merging several zero contours and removing conflicting interface data) to ensure the continuity and horizontality of the interfaces” to avoid any misinterpretation.**

page 8 lines 25-26: “In this case, inverted ERT profiles, resulting from the full apparent resistivity measurements using an ESI of 0.5 m, served as reference models.” for what?

As mentioned in the previous sentence, they served as reference models to assess/compare whether or not the accuracy of the inverted ERT profiles using an ESI of 2m improved when upgraded with the four surficial interpolated levels. **In the revised manuscript, we propose replacing “to assess” with “to assess/compare” in the previous sentence.**

6 – Results

Specific comments – Results Note 14: The presentation of the results is clear. There is very little difference between the 2 arrays presented. Therefore, I was wondering if the presentation of only one array could be considered, to shorten the paper. The difference between the two arrays could be explain with one figure comparing the results of the two arrays for some situations?

We understand AR2's comment. **If requested, we can transfer tables and figures related to the dipole-dipole array to the supplementary material.**

Specific comments – Results Note 15: The fact that in case of low resistivity contrast the inversion artefacts are higher should be explained by the influence of noise. You may conduct a short study on this point: take a model that display the worst results, take the original synthetic apparent resistivity data, add noise to those data with an increasing noise level (say 0.5, 1, 3 and 6%) and invert: do the artefacts increase? If yes, the influence of noise into the artefact production should be explained: the lower resistivity contrast and higher noise, the worse artefact?

We understand AR2's comment, but we are not convinced that such a short study will allow us to draw robust conclusions on the link between artefacts, resistivity contrasts and influence of noise and bring fundamental added value to the conclusions of our work. For instance, we note an intensification of artefacts at the solum level using the Wenner-Schlumberger array with an ESI of 1m instead of an ESI of 0.25 or 0.5m for the model whose subsolum is characterized with a resistivity of 1250 ohm.m and a thickness of 4m (see Figures S6, S7 and S8). In this case, it is the decrease of apparent resistivity measurement density that induced the worst artefacts. As another example, we can observe a gradual increase of top-layer local artefacts with increasing resistivity from 2500 to 20000 ohm.m when using an ESI of 4m for synthetic models characterized with a thickness of 4 or 8m (see Figures S5 and S10). As shown with these two examples, the importance of the artefacts induced by the inversion process depends on several intertwined factors (not only on the influence of noise).

Specific comments – Results Note 16: do those overall estimation values coming from the inversion parameters? Choosing a different lambda changes those values?

See our response to AR2's pdf annotated manuscript comment – Materials and methods – page 8 line 12-13.

Specific comments – Results Note 17: In figure 5 and in the text, you use the term "external electrode separation": not so clear may be you should explain that it is the "minimum array extension" (3x0.5 = 1.5m etc) and remind to the reader the corresponding ESI

AR2's suggestion is unfortunately not suitable for the external electrode separations of 2.5, 3.5 and 4.5 m.

Specific comments – Results Note 18: Figure 5 again: the value with 6m should be close to the values with 4.5 m (in red crosses). But it is not the case. Why?

This is our mistake. We wrongly reversed the colours in the caption of Figure 5. We should have written "external electrode separations of 1.5 (red crosses), 2.5 (yellow crosses), 3.5 (green crosses) and 4.5m (blue crosses)". Note that we also made the same mistake in the caption of Figure 9. **This will be corrected in the revised manuscript.**

Specific comments – Results Note 19: You are assuming that, following your results of Figure 5, that the apparent resistivity at 6m is linearly proportional to the apparent resistivity at lower ESI. From the geophysical point of view, this may be not true. What is possible to say is that the apparent resistivity at low ESI can be derived from ESI 6m assuming a linear

interpolation for your models. And uniquely for them? I.e we could find layered models that will display nonlinear relationship especially when having a conductive second layer. So you explore if other combinations of model (resistive / conductive / resistive for example) as well as different thicknesses of the solum could change the regression logic and what could be the consequence for the study.

We agree with AR2's comment and will follow his/her suggestion. **In the revised manuscript, we will indicate clearly that a) our findings and their interpretation result from the specificity of our case study and derived synthetic models, i.e. "conductive solum / resistive subsolum / conductive bedrock", whose solum has a rather constant thickness and resistivity (meaning that a generalization of the results is limited to similar contexts), and b) similar results could be expected for other situations but must be confirmed by applying the same methodology, for instance, for the reverse case "resistive solum / conductive subsolum / resistive bedrock", which is also commonly encountered.**

Specific comments – Results Note 20: "Nonetheless, overall, the inaccuracy remains considerable, as shown by similar dispersion of resistivity ratio distributions, regardless of whether the ERT images were inverted from standard (Figure 10-a) or upgraded (Figure 10-b) apparent resistivity datasets using an ESI of 2 m. " This inaccuracy should have a considerable consequence for hydrological modelling? Finally, even if the enhanced 2m ESI ERT with additional interpolated data improve the solum /subsolum definition, is this improvement so important for hydrological modelling with regards to the overall inaccuracy of the ERT method that smooths a lot the resistivity patterns?

We understand AR2's comment and will bring new elements to discuss in more detail the potential added value of our novel approach for hydrological modelling purposes. See also our response to AR2's General comments.

Other comments in the AR2's pdf annotated manuscript – Results:

page 8 line 30: "Figures S1-S12" in figures S1>S12, you should add some horizontal lines to enhance the position of the interfaces in the synthetics initial models

This will be done in the revised manuscript.

page 9 lines 22-23: "This last finding is less obvious at deeper depths to bedrock, most probably due to the fundamental lack of resolution of ERT images at increasing depth." not clear. how the well known lack of resolution of ERT at depth could influence an overestimation of the depth to the interface?

We agree that this interpretation is not fully justified. **To meet AR2's criticism, we will remove the following from the sentence: "most probably due to the fundamental lack of resolution of ERT images at increasing depth".**

page 9 lines 28-29 "The visual examination of the inversion results indicates an increase of local artefacts induced by the resolution degradation (Figures S3 and S8)." What is a local artefacts? for all those descriptions you have to indicate , on the figure Sx (with some letters A, B etc) in which part you are talking about. "Visual examination" is not evident for the reader. you must guide him

We understand AR2''s criticism. **We will try to relate the figures in the supplementary file more precisely to our explanation in the manuscript.**

page 9 lines 29-30: “This degradation is mainly restricted to the lowest resistivity contrast and therefore does not explain the general decrease in the accuracy of the results”. not clear at all why you are linking the two parts of the sentence with "therefore". explain.

Whatever the resistivity contrast, a decrease in the accuracy of the inversion results is observed. For the lowest resistivity contrast, we observed that the presence of local artefacts is an element that significantly affects the accuracy of the results, but this is not the case for the stronger resistivity contrasts (as indicated in the following sentence): “This degradation is mainly restricted to the lowest resistivity contrast and therefore it does not explain the general decrease in the accuracy of the results”. We believe that our sentence is understandable enough, but we would be willing to rephrase it if requested.

page 9 line 30: “For the strongest resistivity contrasts” highest?

We do not think that the use of "strongest" is confusing, but we would be willing to use "highest" instead if required.

page 9 line 31: ”shifted in depth” down, up?

Down. **This will be fixed in the revised manuscript.**

page 9 line 33: ”overestimation” down, up?

Up, as it is an overestimation. (?) We are not sure that we understand what AR2 means here with “down, up?”.

page 9 line 33: ”solum depth” thickness?

The solum depth corresponds to the solum thickness. We do not think that the use of the term "solum depth" is confusing, but we would be willing to use "solum thickness" instead if required.

page 10 line 3: ”pronounced” what does it means?

We understand AR2’s comment. **To clarify the sentence, we propose replacing “the deepening effect on the obtained structure is more pronounced as the resistivity of the subsolum is higher and thicker” with “the deepening effect on the obtained structure is greater as the resistivity of the subsolum is higher and thicker”.**

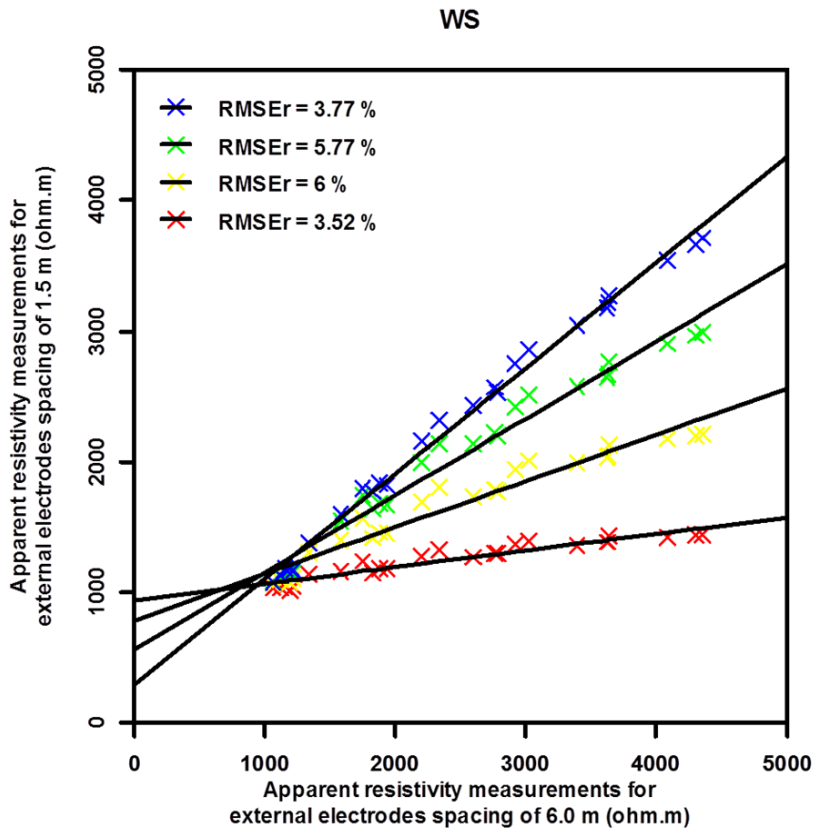
page 10 line 18: ”characteristics” resistvivity? thickness?

Resistivity and thickness! We believe that this is obvious given the second part of the sentence.

page 11 lines 10-11: “These poor results can be linked to the reliability of the linear regressions for models with the lowest resistivity contrast.” again here, noise effect study is necessary to explain (maybe) this result.

We are not sure that we understand what AR2 means here: the effect of noise on the accuracy of the inversion results or the effect of noise on the reliability of the linear

regressions? In the first case, what is certain here is that using the four surficial interpolated levels did not lead to an overall improvement of the results for the lowest subsolum resistivity contrast (some results are even worse, as indicated by lower NSE values, see Tables 1-3). Indeed, the linear regressions leading to the interpolated levels of surficial apparent resistivity crossed each other in this case and led to an unsuitable variation in the apparent resistivity (Figure 5). Ultimately, as stated in the Discussion section, this causes the formation of false resistivity layers by the inversion process (see Figures S11 and S12). Concerning the effect of noise on the reliability of the linear regressions, note that even if we do not add any noise to the apparent resistivity dataset, the linear regressions obtained are also unsatisfactory in the case of low resistivity contrast (still crossing each other), as shown in the figure below.



Scatter plots showing the first apparent resistivity level for an ESI of 2 m (external electrodes spacing of 6 m) versus the four surficial apparent resistivity levels for an ESI of 0.5 m with external electrodes separations of 1.5 (red crosses), 2.5 (yellow crosses), 3.5 (green crosses) and 4.5 m (blue crosses) using the Wenner-Schlumberger (WS) arrays for the 25 synthetic resistivity models. Here we do not add any noise to the apparent resistivity dataset. The fitted linear regressions correspond to the thick black lines and their accuracy is indicated by the root mean square relative error (RMSEr).

page 11 line 12: ”an unsuitable variation pattern of the apparent resistivity” what is a pattern of app resistivity?

We understand AR2's criticism. **To clarify the sentence, we propose replacing "an unsuitable variation pattern of the apparent resistivity" with "an unsuitable variation of the apparent resistivity".**

page 13 lines 23-26: "As clearly shown in Figures 8, Figure 11-a and Figure 12-a, the depth to bedrock of each profile is strongly overestimated in comparison with depths derived from ERT images using an ESI of 0.5 m (mean overestimation of 1.33 m). Overestimation is greater for ERT images with higher resistivity contrasts (Figure 11-a)." This is a major result: large ESI lead to overestimation to the top of the bedrock. However, this result has to be related to your own case study. Is this situation the same if the sub solum would have be less resistive than bedrock?

We agree with AR2's comment. **See our response to AR2's Note 19**.

7 – Discussion

Specific comments – Discussion Note 21: Â´n We ideally recommend using an ESI that is close to the thickness of the top subsurface layer in ERT surveys to mirror the architecture and properties of the subsurface correctly. This choice is relevant to characterize not only the shallower layer, but also the subsurface in its entirety – even when solely aiming for the characterization of deeper layers." The first reason makes this recommendation evident = "You want to define the first layer? Go for small ESI!" The second reason is definitely the good result of the study = "You want a correctly defined deep layer interface? Go for small ESI also!" However, this conclusion is, for now, restricted to your regolith logic only. The study of the reverse case (resistive / conductive / resistive, very common also in some parts of the world) should be carried out to confirmed (or not) this recommendation. Saying this, I understand that I am asking to add more modelling work. This work can be partially undertaken in the discussion part only, by modelling only few well-chosen cases, then derived the (same?) conclusions and saying that this recommendation can be extrapolated to other regolith resistivity patterns or saying that the recommendations you give are restricted to your regolith pattern only, and proposing to the geophysicist to apply the same methodology for his own case.

We agree with AR2's comment. **See our response to AR2's Note 19**.

Other comments in the AR2's pdf annotated manuscript – Discussion:

page 14 line 14: "many natural context" yes, and no. in many other context you have other logic with subsolum more conductive.

We agree with AR2's comment. **See our response to AR2's Note 19**.

8 – Conclusion

Other comments in the AR2's pdf annotated manuscript – Conclusion:

page 17 line 5: "the subsurface of many natural contexts" Yes, but restricted to some geometry logic. This is the important point: Or your conclusion should be clearly related to

your case study, or you enlarge this conclusion to more general cases for regolith patterns (see note 21)

We agree with AR2's comment. **See our response to AR2's Note 19**.

page 17 line 11: "interface" which one?

It is true for both shallow and deeper interfaces delineation. **This will be stated in the in the revised manuscript.**

page 17 line 13: "but that it also affects the characterization of deeper layers." again, this result is major and should be more detailed/explained here

We agree with AR2's comment. **This will be explained in more detail in the revised manuscript.**

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