Subject: Final author comments (ACs) – round 2

Title: Integrated approach for characterizing aquifer heterogeneity in alluvial plains

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MS No.: egusphere-2025-327

MS type: Research article

We appreciate the Reviewers' careful evaluation and suggestions, which have helped us improve our manuscript. Below, we present point-by-point responses to each comment (with the Reviewer's text underlined), along with the corresponding revisions made to the manuscript. We begin by responding to the comments from Reviewer 1 (Thomas Hermans), followed by Reviewer 2 (Anonymous Referee).

Referee #1: Hermans, Thomas

This is my second review of your paper. Although this version is largely improved, I have the feeling you misinterpreted some of my comments so that they are not entirely integrated in the new version. Through the clarifications you made, I understand this is not so crucial, as you only use the ERT data to derive estimated correlation length, but it is important that everything written is correct and that the discussion is clarified, since reviewer comments are also accessible along the published article.

I would therefore like to come back on some of my previous comments and your answers to them.

1. You are not using the ERT-data as a direct constraint to the facies. I would however classify approaches which did it as more advanced compared to what you do. I therefore think it is important this is highlighted more in the conclusion, and certainly as a perspective of the work. Such approach would make a true difference in the estimation of the facies (see 10.5194/hess-22-3351-2018, 10.5194/hess-22-5485-2018, examples with TEM data, but basically similar, and Hermans et al. (2015), cited but without a thorough discussion). This is really crucial to put your work in perspective of the existing literature.

We have revised the main text (in the Introduction and Summary and conclusions sections) to better contextualize our approach, as suggested by the Reviewer. We have clarified that our study employs the classical inversion approach or method (also referred to as deterministic or local inversion method), rather than a probabilistic approach based on stochastic regularization. Although there are some examples highlighting the advantages of stochastic regularization scheme (several modalities) in solving inversion problems, this approach remains an alternative, and not a replacement for the classical and widely preferred deterministic inversion method (e.g., Ramirez et al., 2005). The Reviewer argues that the alternative inversion method based on a stochastic regularization might yield better final model results (e.g., such approach would make a true difference in the estimation of the facies) as demonstrated in Hermans and Irving (2017). However, in our study, the ERT measurements resolve hydrofacies geometry identified by boreholes quite well (except below 20 m depth, as discussed in the text). Moreover, we correlate the real depths of different hydrofacies observed in boreholes with the iso-resistivity values that are consistent with the type of material, i.e., consistent with the electrical properties of each material. Given that the classical inversion method based on smoothness regularization constraint produces models that are geologically plausible and consistent with the lithological reality observed in the boreholes, we find it well justified for our case. That said, we agree that stochastic and deterministic methods can be complementary, as noted by Ramirez et al. (2005). Even in Hermans and Irving (2017), both approaches resolve the inversion problem similarly (their Figure 9a, 9b).

Ramirez, A. L., Nitao, J. J., Hanley, W. G., Aines, R. D., Glaser, R. E., Sengupta, S. K., Dyer, K. M., Hickling, T. L., and Daily, W. D.: Stochastic inversion of electrical resistivity changes using a Markov Chain Monte Carlo approach, J. Geophys. Res., 110, 1-18, https://doi.org/10.1029/2004JB003449, 2005.

2. You write: However, the resistivity anomalies (defining hydrofacies units up to 20 m depth in each profile) are very well defined, with clear resistivity contrasts between them that align with vertical lithological contacts observed in the wells. This result is very important to consider, because it allows us to confidently establish resistivity thresholds for each hydrofacies within the first 20 m depth, without uncertainty.

Writing “without uncertainty” is a very strong and, I am afraid, wrong statement. You should remember that any estimation you made is based on an inversion that distorts the true resistivity value. In addition, your hydrofacies are observed in boreholes that are not co-located. As a result, you don’t know where the interfaces are actually lying on your profile. It is impossible to state that it lies at a specific contourline. Even if you knew the exact location of the interface, It would likely not lie at the same contourline everywhere, because of the limitation of the inversion. This is actually confirmed as your interpretation involves different value for the transition between facies. It means that there is some subjectivity in the selection of these contourlines.

We agree that phrase “without uncertainty” may overstate it, and “with less uncertainty” would be more accurate. If we want to be rigorous, all geophysical methods are inherently subjective to some degree. In this study, and ERT investigations in general, it is impossible to know the real resistivity distribution of the subsurface. Instead, we must rely on the physics and chemistry governing electrical properties of geologic materials in order to make reasonable interpretations on the most plausible model provided by the data inversion, while ensuring consistency with geologic reality (that again, we will never know it using geophysical approaches). The concerns raised by the Reviewer are valid and were already considered in our analysis. Precisely because inversion is an imperfect process that distorts the "true resistivity," we grounded our interpretations in the physics behind the electrical properties of the materials. This approach allows us to correlate inverted resistivity values (yielded by an imperfect data inversion process) with the most appropriate geologic materials.

In our study, ERT results were interpreted through analysis of material electrical properties. The high-resistivity anomalies observed in Fig. 4 are associated gravel (G) hydrofacies. Although SPV-5 isn't co-located with the ERT profile, the thickness of high-resistivity anomalies matches the G unit observed in SPV-5, a discrepancy we attribute to lens-shaped depositional geometry. Similarly, conductive CSs-Sgcs materials at shallow depths and at about 20m depth show thicknesses consistent with those observed in SPV-5. In both cases, the resistivities of G and CSs-Sgcs materials (their boundaries) can be delineated by the contour resistivity lines in the ERT imaging as we have done, reducing the uncertainty linked to the limitations of the inversion process by correlating the contour resistivity lines with the thickness (vertical contacts) of materials observed in the borehole SPV-5. The different values for the transitions between facies (gradual resistivity changes) is what defines the intrinsic subjectivity of the resistivity method, which cannot be avoided. The interpreter's experience and scientific knowledge play a crucial role in developing realistic ERT interpretations. In this study, the reliability of our results is supported by three key factors: high-quality field measurements, proper application inversion approach (regularization criteria), and a strong consistency between vertical distribution of the subsurface resistivity (yielded by the inversion approach) and different materials observed in the boreholes. This alignment, which is consistent with the physics behind the electrical properties of materials, is what makes our interpretation reliable with less uncertainty. As a result, it was possible to estimate the lateral extension of different hydrofacies and incorporate these findings into the T-PROGS model.

3. You write. Thus, we differ with reviewer's point of view of implicitly suggesting that uncertainty analysis would show each hydrofacies having a probability of occurrence for any resistivity value, implying no distinct resistivity thresholds exist. From a mathematical perspective, this approach appears unsustainable given the physical and chemical properties of porous media under an electric field. For instance, between clays and gravel, resistivity thresholds can be clearly defined based on well-understood physical characteristics (grain size, pore geometry, grain

density, tortuosity) and chemical properties (mineral composition, CEC), along with saturation conditions and pore fluid salinity.

This statement is wrong as demonstrated by Hermans and Irving (2017), I really encourage you to read that paper in details. Indeed, you might be able to discrimante a low conductivity clay from a resistive gravel, but your geological settings also has intermediate sand facies. Your error comes from the fact that you are confusing true resistivity values with inverted resistivity values. I invite you to plot the inverted resistivity versus the true resistivity for your synthetic case (Figure 4), and you would likely observe an overlap of inverted resistivity between most facies. This all comes from the limitation of regularized inversion. Eventually, it might not make a big difference for the estimation of hydrofacies characteristic length, but it should be clear that any length you estimate from ERT can be impacted by the inversion process. Indeed, the countour lines you use certainly do not correspond to the real interfaces. And this is the case even if if the choice of your inversion parameters (smoothness constraint inversion with R2) is properly done. What is needed to understand this is present in Figure 4 and can be easily included in the text.

We respectfully note that the Reviewer's response may not fully consider the context of our approach, focusing on points that don't directly engage with our evidence-based interpretation. We used a scientific criterium to interpret the resistivity measurements based on the governing physico-chemical parameters (petrophysical and hydrogeological properties for each geologic material) that define their electrical properties (see our previous response above). After reading both Hermans and Irving (2017) and Hermans et al. (2015), we observed a very clear similarity of both deterministic (smoothness regularization) and stochastic inversion approach (Figure 9a,b in Hermans and Irving, 2017) consistent with lithology shown in Figure 3 of Hermans et al. (2015) – grain size increases gradually with depth from 0 to 10 m, so a gradual increase in resistivity with depth is expected, as it is shown by the deterministic inversion method (their Figure 9a in Hermans and Irving, 2017). Moreover, there are some puzzling aspects in the studies referenced by the Reviewer that warrant discussion. For example, the reported resistivity values, where gravel appears less resistive than sand, and clay lenses in the bottom part of the deposits tend to display values close to those of the gravel facies (Hermans and Irving, 2017). Such findings would require laboratory validation to rule out potential factors like presence of fine-grained particles in coarser facies, decreasing the “true” values of resistivity. Without this physical verification, it's difficult to assess whether their approach offers real advantages over conventional methods. In contrast, in our study we have classified hidrofacies in detail (see Table 1 for very well classified texture of geologic materials observed in the boreholes), and the ERT images of inverted resistivity are consistent with such textural classes. Even in the mixtures like the Sgcs facies (transitional between clay, CSs and gravel, G-Gsc), the dominant fraction governs the electrical signature. In our study, we are confident (as explained above in the previous response) that in the inverted data (i.e., based on the smoothness regularization constraint of inversion method) we are clearly distinguishing and delimiting all determined hidrofacies, based on realistic electrical properties of the different geologic materials observed in boreholes.

4. You write: In fact, it effectively resolves the geometric characteristics of hydrofacies with minimal residual error, meaning it fits the synthetic resistivity data very well.

You should be careful with what you do with the synthetic case. The only thing you can do is confirm that the proposed geometry in the synthetic case can lead to a similar resistivity distribution as observed after inversion. It does not mean that this interpretation is actually correct. There are plenty of other possibilities that could lead to similar results. This is the definition of a non-unique solution.

We fully agree with the Reviewer's perspective regarding the synthetic model, as it represents just one of many possible subsurface resistivity distributions. While acknowledging this limitation, the synthetic modeling results suggest that our field data inversion model is one of the most likely to occur. Therefore, we have used this criterion for validating the field data inversion process and the final inverted model from which we deduced the distribution of hydrofacies model based on resistivity values, i.e., the vertical and lateral extension of each hydrofacies as we mentioned in the text.

I have some suggestions to further improve the manuscript.

1. L64. "Unregulated" or "excessive" instead of "irresponsible". The latter implies decision despite the knowledge it could be harmful, while I assume it might not be the case.

We appreciate the insight and have implemented the suggested modification.

2. L76. I would add "as conditioning data" and cite some of the papers where it has been done like Hermans et al. (2015, aready cited) or Barfod et al. (2018, 10.5194/hess-22-3351-2018, 10.5194/hess-22-5485-2018). This is really important to properly guide the reader where more

advanced conditioning has been proposed. I would also come back on this in your summary/conclusion since there is no discussion section.

The text has been revised accordingly.

3. L127-131/ See my comments above on your response. You should highlight that the choice of the contour line bears some subjectivity as you don't know the real interface location (borehole are projected) and that you interpret the gradual transition from high to low resistivity. You cannot be certain about that and it is important you acknowledge it explicitly in the manuscript even if the impact is likely limited in your methodology as you are not using ERT as conditioning data (that can also be underlined in the manuscript).

We address this comment by modifying the text in Section 2.2. As we mentioned in previous responses and explicitly mentioned in the text, we used the two observation wells SPV-5 and SPV-8 nearby ERT profiles VIN-1, VIN-4 and VIN-10 to find the sites where each single hydrofacies matched with an iso-resistivity contour line from the overlapping between the projection of boreholes and the ERT profiles, estimating the resistivity values for each geologic material. For instance, the thickness of hydrofacies CSs-Sgcs close to the ground and the top of CSs-Sgcs lens at 20.7 m depth observed in SPV-5 (shown in Fig. 4), matched quite well with their corresponding ρ_{hf} (contour iso-resistivity value) in the projection of SPV-5 over VIN-1, whereas the thickness of G material observed in SPV-8 and the top of CSs-Sgcs lens at 25.5 m depth matched very well with their corresponding ρ_{hf} values in the projection of SPV-8 over profile VIN-4 (and also VIN-10). Based on this approach, we believe the subjectivity mentioned by the Reviewer is mitigated, given the accuracy of the final inverted model and assuming that the ρ_{hf} values are consistent with the electrical properties of geologic materials forming the hydrofacies. However, acknowledging the intrinsic subjectivity of geophysical methods (measurement errors) and inherent to the solution of inversion problem (modelling errors), we modified the text in Section 2.2 accordingly to address the Reviewer's comment.

4. L132. Space between "are" and "using".

The text has been revised accordingly.

5. L140. Space between "data" and "and".

The text has been revised accordingly.

6. L161. Refer to Table 1 when mentioning proportions.

The text has been revised accordingly.

7. L199-200. If a high resistivity is next to a low resistivity, then you will always see a transition to intermediate, even in the absence of the intermediate facies. This is why relying on a single contour line or limited number of boreholes might be misleading. In figure 4, are you therefore always sure there is Gsc between G and Sgcs? I would not be.

What we gather from this comment is that the Reviewer may have misinterpreted the lithology of our study site, and consequently, the ERT results. In Fig. 4, we present one plausible distribution of hydrofacies in the subsurface, based on resistivity distribution. This model, along with our interpretation from the original paper, clearly shows that G and Sgcs occur as lenses embedded within Gsc, which is consistent with the information from boreholes. Near the surface, particularly within the upper 6 meters, we clearly show lateral transitions between Gsc and Sgcs-CSs, or between G and Gsc. These lateral alternations are very well defined by iso-resistivity contours lines.

8. L201. Replace "strong continuity" by "good/satisfactory/acceptable consistency".

The text has been revised accordingly.

9. L212. I presume that the 0.1 is in the log scale and not the resistivity scale in ohm.m.

We corrected the text accordingly. Effectively is in log10 scale. We change 0.1 (log10) to 1.26 ohm·m to be consistent with the resistivity scale in Fig. 4.

10. L212. A kriging interpretation requires a variogram model, that should then be mentioned. I assume however a simple linear interpolation would do the same job (this is basically a downscaling approach).

In this approach, kriging is used to generate the contour lines over the grid of "true" resistivity values yielded by the inversion process. Although kriging interpolates between two consecutive values, this interpolation does not influence the results (though we have taken care to verify this). The iso-resistivity contour lines are purely for visualization, however, they prove highly useful in defining threshold resistivity values for hydrofacies.

The Reviewer's comment extends beyond the scope of our methodological approach, adding additional complexity. For example, Hermans and Irving (2017) and Hermans et al. (2015) display inversion results using contour lines, a standard method for resistivity mapping, without addressing variograms or downscaling techniques. This suggests that the comment may not fully align with the context of our study.

11. L214-216. It is still not fully clear to me. As I understand, the boreholes are projected onto the profiles, there is quite some uncertainty about these transitions. It is ok for your purpose that you define some threshold, but these should not be overinterpreted, and the amount of subjectivity involved should be acknowledged. As mentioned before, I really doubt that you have the exact same isocontour in all boreholes, even if you had co-located data. If it was the case, this would really be a coincidence that would not be validated if you had more boreholes available. Stating otherwise would be in strong contradiction with the existing literature. This is actually implicitly acknowledged in line 200-222 since you have different values of the contour line.

Important to note, our approach advances the determination of hydrofacies lens lengths, parameters typically derived from sedimentary process analogs or literature values when direct data are limited. As explained above, there is no subjectivity in the 2D ERT imaging based on contour lines or iso-resistivity contours. While it is true that the boreholes are projected onto the ERT lines (we intentionally avoided placing them directly on the lines because the metallic casing of boreholes could affect the ERT measurements), the data in Fig. 4, though not co-located, still allow for meaningful interpretation. Using physical principles of electrical properties of geologic materials, and the very clear contrasts of resistivity, corresponding to different materials or hydrofacies, we were able to match the lithology contacts in boreholes with the iso-resistivity lines or resistivity contours not exactly in the borehole projection but few meters laterally. This allowed us to assign reasonable resistivity values to each hydrofacies, in line with theoretical values of resistivity for each type of materials.

12. L234-236. It would be nice to show some of these tests, so that the reader can have a better feeling of the methodology.

There is no need to show some of the tests, because of the non-uniqueness there is an infinite number of models that can provide the same results. The most robust approach is to base the interpretation on the physics behind the electrical signatures measured in the field (resistances, apparent resistivities) and then on the distribution of resistivities provided by the inversion process ("true" resistivities = best possible resistivity values), then evaluate whether they are true or not based on the known geological and hydrogeological environment. In this study, the real vertical distribution of lithology is provided by the boreholes, and the strong correlation with the ERT images based on iso-resistivity contours lines demonstrate the accuracy of the methodology. The proposed lens-shaped geometry of CSs-Sgcs in the subsurface, initially inferred from boreholes, is clearly resolved in ERT imaging and corroborated by synthetic modeling. Both datasets show the incomplete presence of this CSs-Sgcs layer indicating its discontinuous nature across the study area, consistent with typical alluvial depositional environments.

13. L247-248. This is also linked to their thicknesses.

It might be. However, if Reviewer bases his comments on the transversal resistivity and associated problem of equivalences (Maillet, 1947), we should consider that the model parameters in 2D ERT imaging are the resistivities allocated in pixels at pseudo-depths and not thickness as in 1D (VES). Strictly speaking, the thickness as a model parameter is not considered in 2D inversion problem. Therefore, although physically it is correct to make an assumption that resistivity is affected by the thickness variation of a particular layer, we prefer to interpret that the difference in the orders of magnitudes of resistivity models is likely to be mainly caused by the different intrinsic modeling error levels between the inversion model of field data and that of synthetic models. Furthermore, this analysis is not relevant here, because the objective of the synthetic model was to corroborate the geometry of hydrofacies CSc-Sgcs as lens-shaped, and not to estimate real resistivity values for such material.

Maillet, R.: The fundamental equations of electrical prospecting, Geophysics, 12(4), 529–556, https://doi.org/10.1190/1.1437342, 1947.

14. L255-256. you don't know. You just show that your synthetic model after inversion gives something similar, it does not mean that the true model resistivity is that one. But indeed, the discrimination potential of ERT is high close to the surface (Hermans and Irving, 2017).

Revised version of the sentence excludes ambiguous elements that could confuse readers.

15. L266-267. I am pretty sure one could fine a synthetic model with a continuous lens to represent this. Maybe it would involve variation in resisitivity values like you used for the different gravel

lenses. Synthetic modelling tells you this situation is possible, not that other geometries are impossible.

While alternative interpretations are theoretically possible, the borehole data conclusively demonstrate the discontinuous nature of this layer (as explained in the text and our earlier responses). Our interpretation provides the most likely outcome, consistent with both the observed data and the known characteristics of the depositional system.

16. L290-291 + conclusion (point 1/3). It could also indicate that transition probability is not sufficient to capture the spatial patterns ? Maybe methods which better deal with multiple-point statistics are needed.

Our approach is based on the transition probability method, which has proven effective for evaluating spatial patterns in this study. While methods involving multiple-point statistics may offer additional insights, their application lies beyond the scope of the present work. Nonetheless, we acknowledge their potential and will consider integrating such techniques in future investigations to further strengthen our analysis.17. L309-310. “Two validation boreholes in R8 achieved 100 % prediction accuracy (Fig. 6b), both located in the northwestern part of the study area.” It is not relevant, realizations are equiprobable and should not be ranked based on the validation.

The text has been revised accordingly.

18. L329. What does “and any potential ambiguities from synthetic modelling at greater depths” mean? What are the ambiguities from synthetic modelling?

The text has been revised, part of the sentence left out to avoid confusing the reader.

19. L365-369. I fear you are here interpreting variations that are not statistically relevant. Have you run a statistical test?

We interpreted the resolution-dependent patterns and trends emerging from 1,200 model realizations. Statistical tests to assess the significance of differences between sets of realizations were not performed.

20. L385-393. Here, you should mention that more advanced approaches exist to constrain geostatistical models with resistivity, where resistivity values are used as a soft constraint. There

is no doubt such an approach, if resistivity values are broadly available, would result in better identification of facies.

We appreciate this perspective and have addressed this issue in our response to the first comment. In our view, stochastic inversion methods are not more advanced methods than the classical (so-called deterministic) inversion method, but rather serve as valuable complementary approaches to the latter and vice versa (Ramirez et al., 2005). We believe that combining different methodologies is one of the best practices to yield the most robust hydrofacies characterization in heterogeneous alluvial environments. In our study, ERT measurements were sufficiently accurate to provide reliable distribution of the subsurface resistivity, consistent with the lithology and their vertical extent observed in boreholes. Consequently, for our specific research objectives, the ERT imaging methodology (with its limitations), yielded results of sufficient practical value.

21. L401-402. I don't know what robust means in this sentence... ERT is just use to adapt the length of the facies, so there is not reason why TPROGS would fail taking this into account.

The text has been revised accordingly.

22. L408-409. Sentence about R8 is not relevant, see above, especially with such a low number of realizations.

We have removed the sentence, as suggested.

23. L416-417. Suggesting a coarse grid which exceeds the size of identified geometry is strange. I would not do that, especially given the small differences in obtained distributions. In addition, eventually, the size would rather be linked to the requirement of groundwater modelling.

We appreciate this insight and have revised the text to clarify that grid selection must prioritize geological accuracy and modeling needs.

Anonymous Referee #2

Thank you for responding to all of my comments from the previous submission. I think any sources of confusion have been addressed. Aims are well formed and the relationship between the electrical resistivity tomography results and the stochastic modelling has been discussed in a clearer manner.

A minor note, on line 390 the authors state that ground penetrating radar (GPR) could be used to aid in hydrofacies delineation in future studies. However the context of the paragraph discusses a highly conductive layer at ~20m depth. GPR's depth of penetration is inversely proportional to ground conductivity, so I doubt GPR would effective in such an environment; even then, GPR traces can be hard to interpret for features several meters below the ground surface (unless on a glacier) and would require a low frequency system (10s of MHz). Seismic methods might be more appropriate than GPR as one would anticipate variations in wave speed between the gravel and clay layers. A variety of EM methods could also be good candidates for studying gravel lenses.

In any case, I'm happy to recommend that the paper be accepted (pending any technical corrections).

We fully agree with the Reviewer’s comment and greatly appreciate the suggestion to incorporate seismic methods. Effectively, GPR signals (electromagnetic waves) attenuate in electrically conductive material or under high moisture conditions (water saturation). Our idea for proposing GPR (surface-deployed) was precisely to detect these attenuation patterns, particularly for identifying CSc-Sgcs lenses near the surface and at depths below 20 m. Another advantage of GPR is its significantly higher resolution compared to ERT, so we would expect more detailed delineation of features such as the geometry of G lenses. However, GPR should be very relevant if deployed in cross-borehole or well-to-well configurations, along with cross-borehole ERT and well-logging tools. Following the Reviewer's suggestion, we acknowledge seismic and EM methods would be worth implementing in the study area. In future work, we will try to test some of these methodologies, along with stochastic-based approaches for data processing and inversion.

Specific comments:

There is typo on line 83, should read "of ERT".

Thank you, we have revised the text accordingly.