We respond in the spirit of HESSD to promote discussion. Therefore, this document is not meant as a typical “reponse to comments” in which we indicate how we address comments (we will wait for the remaining reviewers). Instead, we discuss the issues raised by Alex Furman in the hope of raing discussion (and, indeed, some of the issues he brings deserve discussion by the soil aquifer treatment (SAT) community.

First of all, we thank Alex Furman for the overall positive assessment of our work and for the constructive tone of the issues he criticizes.

Regarding the, indeed pioneering, work of Haaken et al., you are absolutely right. We believe that SAT systems should be monitored continuously to help during operation and beyond, its pioneering nature, the fact that they buried their electrodes was a very good idea for large scale, long-term, operation.

Regarding Shafdan, it is indeed a paradigmatic example. We use it (and teach it in class!) as an example on what all cities in water scarce regions should do. However, it may not be so relevant for the goals of this paper, centered on the use of ERT. Depending on the comments of other reviewers, we will probably expand the introduction and discussion and make more explicit mention of Shafdan.

Below we address the specific comments in blue our replies.

1. While not the core of this study, it is about time that researchers stop claiming that adsorption is a key element in SAT. A simple mass balance would show that all the adsorption capacity is used at a relatively early stage of the SAT facility life, and additional adsorption is possible only if there exists a degradation process that ‘empties’ adsorption sites

   Actually, we do not quite agree. While it is true that sorption does not affect the flux of (inorganic) solutes after a transition time, it is fundamental for partitioning compounds (the ones that are toxic by accumulation in human tissues by processes similar to those that control absorption into biofilms). In fact, you hint it at the of your comment (“if there exists a degradation process”). The relevance of sorption lies in the fact that it increases the residence time of contaminants in the microbiologically active zones, thereby favoring biodegradation. In this sense, biofilms represent an important compartment where both sorption and degradation occur in close interaction (Jou-Claus et al., 2024).

2. L68: Biofilms are not the critical element. Microbes are. Biofilms are the environment that helps microbes perform their functions

   We appreciate your clarification regarding the distinction between microbes and biofilms. We fully agree that microbial activity is the key driver of contaminant transformation in SAT systems. However, in practice, the majority of microbes in these systems are embedded within biofilms. This structural organization creates conditions that are not present for planktonic microorganisms: biofilms provide a matrix with sorption capacity (especially relevant for compounds with high logKow), localized redox gradients, and extracellular enzymes that together enhance biodegradation (Jou-Claus et al., 2024 (https://doi.org/10.1021/acs.est.3c08465); Wang et al., 2022 (https://doi.org/10.1016/j.advwatres.2022.104286),2024 (https://doi.org/10.1029/2023WR036872)).

In addition, biofilms are not only relevant for microbial functioning but also represent the main factor responsible for biological clogging and water (and solutes) retention, which has direct implications for the hydraulic performance of SAT systems. For these reasons, we consider biofilms a critical parameter to be managed, as they integrate both the microbial activity and the hydrodynamic constraints of the system.

Apparently, the authors did not read Haaken et al. thoroughly…

R.3- We thank you for pointing this out. Indeed, Haaken et al. conducted pioneering work applying time-lapse ERT to monitor unsaturated zone processes during SAT, and we acknowledge that our current phrasing ("remains unexplored") is misleading. Our intention was to emphasize that, although Haaken et al. demonstrated the feasibility of using ERT in the USZ, the technique has rarely been applied, and its potential for systematically linking geophysical observations with hydro biogeochemical processes in SAT remains underexplored. We wonder if ERT is used to help management at Shafdan.

We will revise the sentence accordingly to properly cite Haaken et al. and clarify the novelty of our work.

3. Interestingly, the authors practically ignore the vast body of literature that comes from the Israeli SAT experience – the largest of its kind that has operated for about 30 years, and is documented quite well in the literature

   We are thoroughly familiar with the work at Shafdan, which we consider a paradigm of what all cities in water scarce countries should do. However, the main objective of our manuscript is not to provide a review of SAT systems worldwide (let us wait and see what the other reviewers say). This is why your correction about the work of Haaken et al. is so important. Our goal in this paper is to propose the application of ERT for monitoring infiltration dynamics and linking them to biofilm development in the unsaturated zone. For this reason, we have not included detailed discussion of other large-scale MAR projects. Instead, we focus on demonstrating how ERT can capture hydrological changes associated with biofilm formation, which we consider the critical element affecting system performance.

4. Pulse (it is fine to use a short version, but be consistent and use short or full versions synchronized)

   R.5 – We will correct and be consistent.

5. ~L145. Isn't the difference in environmental conditions (temperature) a concern?

   Indeed, temperature can influence microbial and geochemical processes in SAT systems and, thus, biofilm growth. Temperature also affects viscosity and oxygen solubility. In our case, both recharge episodes were conducted with relatively stable temperature conditions (Figure S.1). However, the average temperature during the Pulsed WP was slightly higher than during the Continuous WP, with differences around 7-10 °C. We therefore consider that temperature variations may have had some influence on the results. However, the impact on viscosity is moderate (some 20%) and the impact on microbial conditions is unclear (we have found that, after several years, microbial communities become quite resilient and adapt to temperature changes. This is why we believe that the operational conditions tested are more important. Nevertheless, you are right that the issue cannot be ignored and, depending on the comments by other reviewers, we will add a sentence in the discussion to acknowledge that of course temperature is an environmental factor that may modulate removal processes in SAT systems.

6. Section 2.4. Any petrophysical model to relate resistivity with water content?

   In our case, the main reasons for not implementing a petrophysical model to determine the water content from the obtained electrical resistivity was the high heterogeneity observed in the electrical resistivity 3D distribution due to preferential vertical flow paths. This heterogeneity, caused by the complex infiltration patterns and biofilm development, made it challenging to apply traditional petrophysical relationships that assume more homogeneous conditions. Instead, we prioritized understanding the spatial and temporal patterns of water infiltration and biofilm development rather than obtaining precise quantitative water content values.

7. Is a quarter of a second enough to get charge stability for the reactive layer?

   Our choice of a 250 ms current pulse proved sufficient to achieve charge stability in the reactive layer, as confirmed by our quality-control criterion. By comparing normal and reciprocal measurements and retaining only those pairs with ≤ 15% difference, we ensured that any residual polarization or transient charging had fully equilibrated within each 0.25 s injection. In practice, few measurements were discarded by this threshold.

8. 2-3. These plots are helpful, but zooming in to show a single representative day or two at different periods would be much more interesting

   We thank you for the suggestion. The purpose of Figures 2 and 3 was to illustrate the overall inflow dynamics across the two recharge episodes, and how these controlled soil moisture and water table depth. To help readers also visualize short-term variations, we have already adjusted the scale in the first and last parts of the plots to highlight daily dynamics at the beginning and end of the episodes. Following the reviewer's comment, we will include an additional zoomed-in view of one representative day from each recharge mode (Continuous and Pulsed) in the Supplementary Material.

9. Figures 5-7 are nice, but difficult to analyze. The authors are encouraged to use some integrative type of analysis that would smooth the noise

   You are right! We gave a lot of thought to the presentation and discussion of results. Actually, we hope that readers with available time (hopefully PhD students) will have the time to analyze them in detail and enjoy them as much as we did. In fact, we can conjecture some geostatistical integrative analyses (i.e., evolution of connectivity, cluster sizes, etc.). However, we do not consider that the technique is sufficiently mature for such analysis. In fact, as you correctly point out in subsequent comments, some of our analyses are somewhat speculative.

10. 5through 7.

R.11- Thanks, we will correct it.

11. L423-426. This is not much more than speculation. Any evidence?

We wrote "The fact that oxygen was virtually depleted at 35 cm during the Puls WC suggests a very high level of biological activity. Perhaps more significant is that Oxygen never reached air saturation, even though it is clear that it was being sucked (presumably across preferential air paths, in a process similar to the "aquifer breathing reported by Roumelis et al. (2025), except that in their case it was driven by aquifer head fluctuations, while ours was driven by drainage fluctuations."

Sure, there is some speculation here. We cannot directly prove the mechanism (hence "suggests"). However, the fact that observed oxygen dynamics are consistent with processes previously described as "aquifer breathing" by Roumeli et al. (2025). In our case, we hypothesize that similar preferential air pathways may exist, though driven by drainage fluctuations rather than aquifer head variations. This is consistent with ERT observation, which is the point we are trying to make. ERT only measures EC, but its time evolution yields rich food for thought.

Still, depending on the other reviewers, we will revise the text to something like: "The fact that oxygen was virtually depleted at 35 cm during the Puls WC suggests intense microbial activity. Interestingly, oxygen never reached full air saturation, despite clear evidence of air ingress. While we cannot directly demonstrate the mechanism, this pattern is consistent with the 'aquifer breathing' process described by Roumelis et al. (2025). In our case, we hypothesize that the pulsed recharge strategy enhanced oxygen entry, thereby promoting aerobic conditions in the unsaturated zone.

12. This is an interesting idea, but it requires much better support. Resistivity 'hot-spots' indicate water content. From here to biochemical hot-spots, the distance is quite far. At best, it indicates the formation of biofilm, but there are so many things that can trigger biofilm formation (some of which are related to microbial stress, not to microbial prosperity

We are not sure which text you are referring to. We believe you refer to "Beyond its potential use for detailed monitoring, ERT's ability to map water distribution heterogeneity has proven useful in gaining insight on the functioning of different recharge strategies and the use of a reactive barrier, as well as how they influence water infiltration and retention. Both approaches created localized moisture variations, which suggests the formation of large biofilm clusters".

We agree with you in that ERT detects electrical conductivity contrasts, which relate directly to water content and not biochemical activity per se. However, biofilms are composed of up to ~97% water and their extracellular polymeric substances (EPS) markedly influence local moisture distribution and hydraulic connectivity. Consequently, the presence of biofilms can enhance water retention and create localized resistivity anomalies. Our interpretation is therefore not that ERT directly images biofilms, but rather that ERT can provide indirect evidence of their dynamics.

13. L456 and on. Well, understanding that oxygen monitoring is useful is interesting, but given that this is the only thing that was monitored, it puts it in a different light. What about ORP (just an example) – won't that be equally supportive? While I do not argue, it isn't easy to conclude here

    Well, not quite true, we monitored T, water content, EPS, EC, etc. We wrote "While ERT provides reliable maps of moisture distribution, interpretation is best achieved when coupled to point measurements. In particular, we have found oxygen monitoring particularly useful to add a quantitative measure of biochemical activity to the moisture maps. The combined analysis of hydraulic monitoring, EPS quantification, and ERT imaging demonstrates that recharge strategy and barriers implementation influence biofilm development and water distribution in the USZ."- Indeed, parameters such as ORP could also provide valuable information as indicators of microbial activity, particularly if measured continuously and non-invasively. However, in this study oxygen was the parameter available for continuous monitoring, and we found it to be particularly useful for assess microbial processes. Our conclusion is therefore based on oxygen dynamics, while we recognize that integrating additional parameters (e.g., ORP, CO_2, redox-sensitive solutes) in future work would further strengthen the interpretation.

14. The use of biological materials to enhance biochemical activity is not new, but interesting in the context of ERT. Nevertheless, this practice needs to be discussed in terms of long-term activity. It is not trivial to refresh such a layer after its functionality diminishes

    Indeed, in our observations, the reactive barrier primarily serves to accelerate the establishment of a microbial community that is more resilient to environmental changes. Once this community is established, the reactive layer maintains its functionality without the need for periodic renewal, as the consumed organic matter is, at least in part, supplemented by root growth. Still, as you know from Shafdan, the long time behavior is somewhat uncertain. We have only been observing for 4 years. Still, we have given some thought on the refreshing of the layer (note that its behavior improves over time!), so certainly we do not want to build it anew.

    You mention past use of biological materials. We are not aware of this.