SAT is a form of MAR. Discussion on the comments provided by Prof. Xinqiang Du.

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We provide this reply in the spirit of HESSD, aiming to foster constructive scientific discussion. For this reason, the present document should not be interpreted as a conventional "response to reviewers," where each comment is accompanied by a detailed revision note (we will do so once all reviewers' reports are available). Instead, our intention is to reflect on and engage with the points raised by Prof. Xinqiang Du, whom we thank for his comments and the opportunity to further discuss the important issues of Managed Aquifer Recharge (MAR) and Soil-Aquifer Treatment (SAT).

Prior to a detailed discussion of the specific points, we wish to address two issues Prof. Xinqiang Du raises regarding (1) whether SAT can be considered a MAR technique and (2) the when scientific references are needed.

Regarding the appropriateness of referring to SAT as a MAR technique, we respectfully argue that SAT is, and should be, considered a subset of MAR. We argue this point, both from a logical perspective, and from its wide use by both the scientific literature and the water management sector. While it is true that SAT's primary focus is on water quality improvement, the process inherently involves the intentional inflow (recharge) of water into an aquifer, thereby also contributing to groundwater resource augmentation, the core purpose of MAR. This inclusion of SAT under the MAR umbrella is well established in foundational and contemporary literature (e.g., Bouwer, 1991, 2002; Dillon, 2005; Bekele et al., 2011; Alharbi and El-Rawy, 2024, among others), but also in the professional sector, perhaps best summarized in the paper prepared by the IAH group on MAR (Dillon et al., 2019).

Regarding scientific referencing, we believe that referencing plays three roles: (1) providing support to a statement (e.g., we believe that SAT is a MAR technique because Dillon (2005) said so); (2) acknowledging the author of an idea (e.g., Bouwer, 1991); and/or (3) providing the reader with a source of additional details. These principles are not universally accepted, but they are the ones we try to follow. Note that, according to these principles, we argue that references are not needed for (1) concepts presented in the paper; (2) concepts that are widely known and accepted by the scientific community of the journal (e.g., we do not provide a reference for Darcy's Law because all HSS readers are familiar with it); and (3) trivial concepts.

Below we address the specific comments in blue our replies.

CC2-1- From an academic classification perspective, Soil-Aquifer Treatment (SAT) is not universally recognized as a subset of Managed Aquifer Recharge (MAR). According to standard definitions, MAR refers to the intentional recharge of water to aquifers for subsequent use or environmental benefit. At the same time, SAT primarily focuses on improving water quality through soil infiltration. Consequently, the term "MAR-SAT" does not represent a commonly accepted implementation scenario in the literature. Given the study's core focus on unsaturated zone monitoring via ERT, the standalone term "SAT" is sufficiently precise to contextualize the research. The explicit association with MAR is unnecessary unless the authors can demonstrate direct relevance to MAR's core objectives.

RCC2-1- We agree that the present study was conducted within a specific SAT system. Our initial choice to include the term "MAR" was motivated by the fact that the methodological approach we present—ERT monitoring of water movement through the unsaturated zone—is directly transferable to a broad range of Managed Aquifer Recharge (MAR) settings. In many MAR implementations, infiltration through the vadose zone plays a central role, and understanding the dynamics of percolation and water–soil interactions is equally relevant.

However, we acknowledge that SAT constitutes a well-defined category on its own and that using "SAT" alone is sufficiently precise for describing the experimental context of this work. To avoid ambiguity in terminology, we have therefore removed the explicit reference to MAR in the title. The revised title now reads: **"Mapping Water Content Dynamics in SAT Systems Using 3D Electrical Tomography."**

As argued in the introduction to this rely, we consider SAT as a subset of MAR.

CC2-2- Line 46-50, the natural pollutant reduction capacity of the porous media can be seen as an additional guarantee to MAR. So, the mentioned regulations are not a constraint for widespread application of MAR. Although the citation supports the conclusion, it is not a reasonable opinion.

RCC2-2- We agree with Prof. Xinqiang Du that the natural attenuation capacity of the soil and sub-surface is an additional guarantee in many MAR applications. Our intention was not to claim that regulations universally hinder MAR implementation, but to highlight those regulatory frameworks fail to acknowledge water quality improvement processes during soil passage.

When the same source-water quality requirements are imposed on all MAR types, the potential of infiltration-based systems to act as effective nature-based solutions for water quality improvement becomes hindered (actually, some of us argue that they effectively favor indirect pollution of groundwater, but it would be long to argue here). Techniques such as infiltration ponds, percolation tanks, and SAT systems rely on the unsaturated zone for filtration, sorption, and biodegradation to remove pathogens, organic contaminants, and nutrients, a geo-purification capacity widely acknowledged in the literature (e.g., Dillon, 2005; Bekele et al., 2011). However, in many countries, the legal framework for aquifer recharge is so restrictive that the feasibility of implementing SAT for water renaturalization at an industrial scale is severely limited. In countries such as France, Spain, and Chile, the use of treated wastewater for recharge is often permitted only at a pilot scale and for research purposes, requiring specific exemptions (Casanova et al., 2016., Miquel, 2003; Rivera-Vidal et al., 2025). This regulatory reality effectively constrains the widespread application of SAT systems, regardless of their proven geo-purification potential (rainfall fails to meet the requirements because of its low pH and the fact that samples usually contain suspended solids).

Thus, our statement aimed to reflect that, in practice, overly stringent or non-differentiated source-water requirements may limit the adoption of infiltration-based MAR systems specifically designed to take advantage of natural attenuation in the unsaturated zone, even though such attenuation indeed represents an added level of protection.

CC2-3- Line 51, ”…… for Soil Aquifer Treatment (SAT), a MAR technique that uses treated wastewater as a source for recharge” is a misleading interpretation of SAT and MAR. The reason is the same as in the above comment.

RCC2-3- We acknowledge the perspective of Prof. Xinqiang Du on this point, which mirrors the previous comment on the SAT-MAR relationship. However, we respectfully maintain that SAT is widely considered a form of Managed Aquifer Recharge (MAR) in both the scientific literature and the water management sector, because it necessarily involves the intentional recharge of treated water into an aquifer, thereby increasing groundwater storage as part of the treatment process.

CC2-4- Line 51–55， Treated wastewater is not a typical recharge source for MAR in most regions, and SAT is not widely recommended as a water quality improvement technology for MAR systems. This is primarily because pollution prevention and contaminant attenuation are paramount considerations for MAR, and SAT may not consistently meet the stringent water quality requirements for aquifer recharge without additional validation.

RCC2-4- We agree that in many regions treated wastewater is not yet a common source for MAR implementation, largely due to stringent regulatory requirements and the need to ensure the protection of groundwater resources. We also acknowledge that SAT is not universally recommended as a water-quality improvement step within MAR schemes unless performance has been demonstrated under site-specific conditions.

However, our intention in this section was not to imply that SAT is widely deployed for MAR using treated wastewater, but rather to summarize the established evidence showing that SAT can provide substantial water-quality improvements when such applications are permitted. Numerous studies have documented the ability of SAT to remove suspended solids, pathogens, nutrients, dissolved organic carbon, and a range of organic and inorganic contaminants (e.g. Valhondo et al., 2020, Sanz et al., 2024, 2024b).

CC2-5- Line 114–115， The statement "following the recommendations of a soil scientist" does not constitute a for the described methodology. To justify the approach’s rationality, the authors must cite relevant peer-reviewed literature or standardized protocols that support the adopted procedures.

RCC2-5- Indeed, Prof. Xinqiang Du is right, it is not a rigorous scientific basis, it is just a practical recommendation. We delete “following the recommendations of a soil scientist”, which still describes the practical solution we used (we hope others can use it).

CC2-6- Line 144–145， Two critical concerns arise regarding clogging-related claims:

- Logical Inconsistency: Clogging is defined as the phenomenon of reduced hydraulic conductivity. The absence of operational shutdowns for maintenance does not equate to the absence of clogging—mild to moderate clogging can occur without necessitating downtime. Notably, the statement in Line 224 ("we have observed the development of a water layer due to the reduction of infiltration capacity") directly indicates clogging-induced reduced permeability, creating a contradiction that requires resolution.

- Insufficient Evidence: The claim regarding vegetation's role in clogging control lacks empirical support. The authors must provide verified references demonstrating vegetation's efficacy in mitigating clogging in SAT/MAR systems, or supplement with in-situ data (e.g., vegetation root distribution, clogging layer composition).

RCC2-6- With some qualifications, we agree on both accounts:

First, regarding clogging, it is true that the absence of operational shutdowns does not imply that clogging does not occur (and we will reword the statement in the revised version of the paper, if accepted). What we have observed is that, in some cases, continuous operation helps sustain recharge for longer periods. The presence of a water layer on the basin surface does not necessarily indicate a reduction in infiltration capacity if this layer remains stable over time. The issue arises when the water layer increases abruptly, which suggests a sudden accumulation of fine materials that reduce infiltration capacity, possibly due to an overload at the WWTP outlet causing the mobilization of fines.

Second, regarding the role of plants, while the statement is marginal to the goal of the paper, the issue is not well known in SAT literature. Therefore, in the revised version, we will provide references both from our own experience in SAT and from the forest literature (e.g., Valhondo et al., 2020., Wu et al., 2017., Le Coustumer et al., 2012). We have observed in pilot basins of 400 and 5,000 m^2 that vegetation contributes to stabilizing the free movement of organic particles, prevent excessive algal growth by covering the water surface by reducing sunlight exposure, and limits the mobilization of fines from the basin slopes, thereby preventing their accumulation across the entire recharge basin surface. Superficial clogging was no longer observed once the plants covered the basin surface, under the same recharge scheme. Specifically, in the 5,000 m^2 basin, the complete removal of vegetation resulted in significant clogging problems in this system.

In fact, we are preparing a manuscript presenting the results in the 400 m^2 pilot basins, in which we discuss these observations.

CC2-7- Line 165， Formatting error: "250 10^{-3}" should be corrected to "250×10^{-3}".

RCC2-7- Thanks, we have corrected it

CC2-8- Line 206， Terminology Ambiguity: The phrase "aquifer head (depth to water)" is imprecise.

RCC2-8- Thank you, indeed, we will change it to “depth to the water table” in the revised manuscript.

CC2-9- Line 393–395, The opinions in this section lack support from validated peer-reviewed references. To enhance credibility, the authors must supplement with relevant literature that corroborates the proposed mechanisms or conclusions.

RCC2-9- This interpretation is based on direct, site-specific evidence. The recorded EC peaks show a precise temporal correlation with independent meteorological data on coastal windstorms, which are the known driver of seawater overtopping events in this area. Furthermore, the WWTP operators consistently document salinity surges in the influent following such storms, confirming that seawater intrusion into the coastal sewerage network is the established cause. Thus, this is not a proposed mechanism but a documented occurrence for this specific system.

We feel shy to consider this a contribution of our paper, but rather a frequent observation. Further, this statement is an explanation of the supporting information, which is not relevant to the paper. Depending on what other reviewers say, we will either delete the statement or expand the explanation in the SI of the revised version, at our site and other coastal cities, because it may be relevant for the operation of WWTPs and SAT systems. Yet, to our knowledge, it has not been addressed in the scientific literature.

CC2-10- Line 442-448, I think the conclusion that ERT allows visualizing and monitoring variations in water retention and biofilm growth is a verified fact in the past relevant research. So, the authors should keep up with the latest developments.

RCC2-10- Definitely, numerous authors, including ourselves, have used ERT for monitoring variations in water retention of soils. We do cite six such papers (there are many others). There are also some, far less, that have used ERT for SAT. And those have centered on characterization purposes (e.g., Sendrós et al., 2020) or monitoring water flow (e.g. Haaken et al., 2016, or Arboleda-Zapata et al, 2025). If the paper is accepted, we will cite these papers

CC2-11- A critical gap in the manuscript is the lack of discussion on how the study's conclusions translate to MAR applications. Specifically: What actionable principles or technical guidelines can be derived to inform MAR system design, operation, or performance assessment? How does the ERT-based monitoring approach address key challenges in MAR (e.g., recharge rate optimization, contamination risk mitigation)? The authors should supplement this section with concrete implications for MAR practice to enhance the study's applied value.

RCC2-11- We thank Prof. Du for this comment. We will add a closing remark to emphasize the importance of monitoring for the optimal operation of the system. Note, however, that optimal operation does not necessarily address the large questions posed by Prof. Du. Regarding recharge rate, SAT systems are usually designed for a given flow rate. Thus, our comment will mention the possibility of seasonal variations in flow rate depending on ERT response. As for contaminant mitigation, ERT has helped us understand the dynamism of biofilms. While this contributes to explain the outstanding behavior of SAT in contaminant removal, we do not think our understanding is sufficiently mature to to link ERT response to contaminant removal. Still, we will mention it as a future challenge.

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