Response to reviewer comment 2

The present manuscript deals the geoelectrical monitoring of structural change induced by different treatments. It is intended for publication in SOIL. The study is of interest for the soil science community as soil structures influence many important processes. The manuscript is well written and, to the best of my knowledge, is using state-of-the-art experimental set-up and procedure. However, I do have some comments regarding the discussion and petrophysical interpretations that I develop in the detail list below.

We sincerely thank the reviewer for the positive assessment of our work and for the constructive and insightful feedback. The reviewer's comments, and particularly the suggested literature, helped us strengthen both the framing of the study and the interpretation of our geoelectrical results. We have carefully considered all the comments and will revise the manuscript accordingly. Below, we provide detailed responses to each point raised:

1. *Introduction : Given the topic of the paper, I think that the authors need to develop the literature of structural change monitoring using geophysical tools (e.g., Romero-Ruiz et al., 2018, 2022).*

We sincerely thank the reviewer for pointing us to these studies, which are highly relevant to our work and strengthen both the framing and the interpretation of our results. We will expand the Introduction to review the use of geophysical tools for monitoring changes in soil structure. Romero-Ruiz et al. (2018) showed the potential of geophysical methods for non-

invasive soil structure characterization, and Romero-Ruiz et al. (2022) demonstrated that geoelectrical monitoring can capture lasting structural changes in soil. We will also highlight that SIP, although recognized as promising for this purpose, has so far received little attention in this context, which our study aims to address. Furthermore, the finding of Romero-Ruiz et al. (2022) that increased aggregate connectivity is reflected in bulk electrical conductivity directly supports our interpretation of the in-phase conductivity results, and we will incorporate it into the Discussion accordingly.

2. *Section 2.2: Given the influence of partial saturation on SIP signals, could the author provide more detail their sample saturation degree in addition to the water content. My understanding is that it was fully water saturated but it would clearer by stating it (at the moment it is only implied in line 169).*

The reviewer is correct: all SIP and tracer columns were fully water-saturated throughout the experiments. The columns were saturated from the bottom with 5 mM $CaCl_2$ to displace air, followed by saturated flow for three pore volumes. We agree that this should be stated explicitly rather than implied, and we will revise the Methods to clearly state that all SIP measurements and tracer experiments were conducted under fully saturated conditions.

3. *Section 3.1: the physical limitations to microbial activity (i.e., soil respiration) and their relationship to aggregate (i.e., microstructural properties) through diffusive processes are discussed in the literature (e.g., Nuna, 2017, Fülöp et al., 2026).*

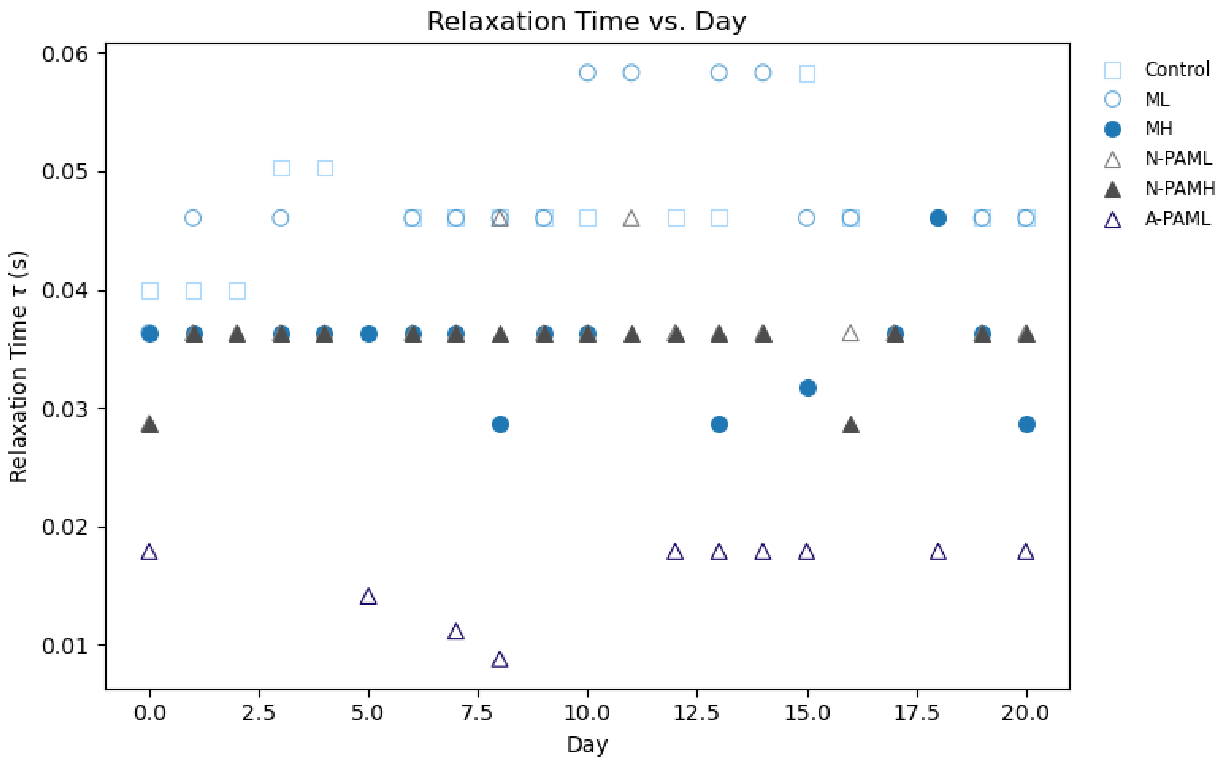
We thank the reviewer for this valuable comment and for pointing us to these studies, which provide an important mechanistic framework for interpreting our respiration results. In the manuscript, we proposed that the stable aggregates formed under A-PAM treatment alter pore architecture, reduce oxygen diffusion, and restrict microbial access to carbon substrates and electron acceptors, thereby limiting respiration. The studies suggested by the reviewer directly support this interpretation. Nunan (2017) and Fülöp et al. (2026) show that microbial decomposition largely depends on the diffusion of substrates toward decomposers, and that aggregation can physically protect organic matter from microbial access. Moreover, Fülöp et al. (2026) suggested that near saturation, respiration may be limited by oxygen diffusion, which is relevant to our study since respiration was measured at a moisture content close to saturation. We will incorporate these studies into the revised Discussion to strengthen our interpretation of the consistently lower CO_2 emissions under A-PAM treatment.

Furthermore, the framework proposed by Fülöp et al. (2026), linking electrical conductivity to diffusion-limited respiration through the tortuosity of the water phase, offers an interesting perspective on our results. The MH treatment, which showed the highest respiration rates, also showed the σ' increase that we attributed to reduced tortuosity of the conductive pathways. Although respiration and SIP were measured in separate experimental setups, and a direct quantitative link therefore cannot be established here, we will briefly note this qualitative consistency in the revised Discussion as a direction for future research.

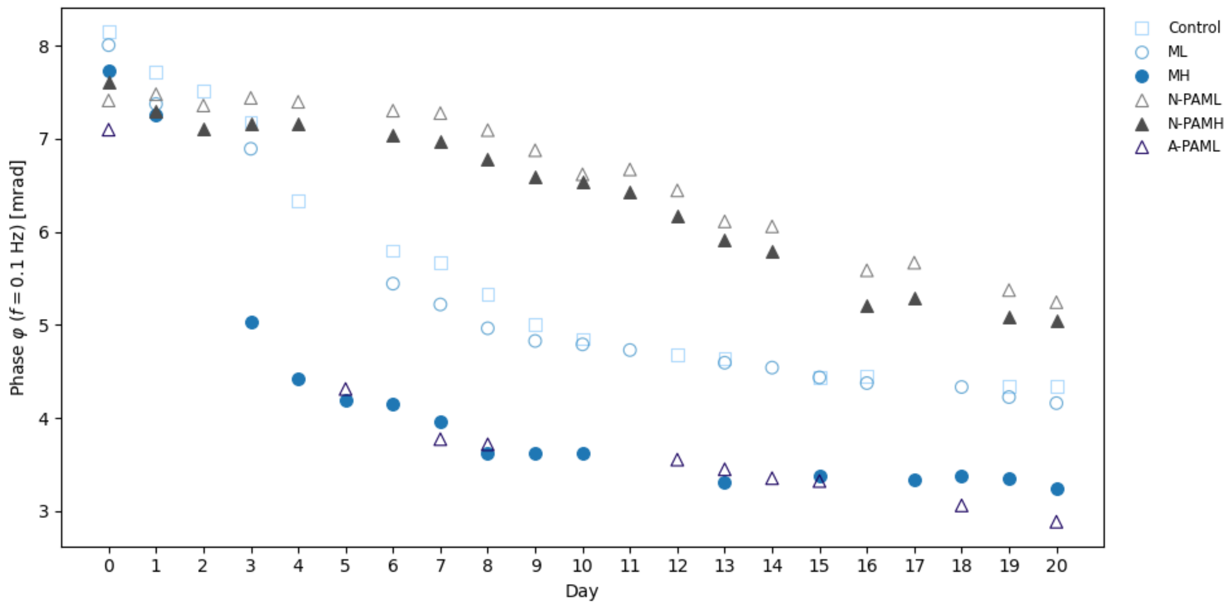
4. *Figure 5: I understand the use of fmax to plot the imaginary part of the conductivity, but I'm afraid that it hides potential information from the time series. What about the variation of f_max with time (since f_max could be related to aggregate size as discussed in lines 419-428) ? Given the heterogeneity in initial conductivities and the influence of the real part of the conductivity on the imaginary part, how is the phase evolving? Would it be possible to plot the pore water electrical conductivity as a function of time?*

We thank the reviewer for these insightful suggestions. We have analysed the temporal evolution of both the relaxation time (τ), derived from the peak frequency of σ", and the phase, but did not include these results in the original manuscript.

Throughout the entire incubation, the A-PAML treatment exhibited relaxation times two to three times shorter than all other treatments, which remained relatively stable over time. This shows that the shift toward shorter polarization length scales, presented in the manuscript only for day 0, persisted over the full 20 days, further supporting our interpretation of stable micro-aggregate formation under A-PAM treatment.



The phase decreased over time in all treatments. The decline was steepest in MH and A-PAML, intermediate in the control and ML, and smallest in N-PAM, mirroring the magnitude of the σ' increase in each treatment. Normalizing the polarization response by σ' therefore does not change the treatment differences, and the phase trends are consistent with our interpretation of the σ' and σ" results.



To allow readers to evaluate these trends directly, the full electrical spectra for all treatments and measurement days will be provided in the Supplementary Material of the revised manuscript.

Regarding pore-water electrical conductivity, a continuous time series unfortunately cannot be provided, since the columns were not equipped with internal pore-water sampling, and EC could be measured only in effluent collected under flow conditions at day 0 and day 20.

5. *Lines 313-315: would the higher mucilage explain the higher respiration rate in figure 2?*

Yes, the higher respiration rates observed in the MH treatment are consistent with its higher mucilage concentration. As mucilage is a readily available carbon source rich in labile polysaccharides, the five-fold higher application rate in MH provided substantially more substrate for microbial activity, resulting in a dose-dependent respiration response.

This observation also connects to the concentration-dependent threshold hypothesis proposed in the manuscript. Previous studies attributed the persistence of structural improvements after mucilage decomposition to microbial byproducts (Morel et al., 1991; Traoré et al., 2000; Marsico et al., 2018). The higher microbial activity in MH may therefore have promoted the production of such byproducts, including extracellular polymeric substances, which could act as secondary binding agents. This mechanism may help explain why MH retained partial aggregate stability throughout the incubation while ML did not, and it is consistent with the hypothesis presented in our conclusions regarding EPS-mediated stabilization. We will clarify this link between respiration, mucilage concentration, and aggregate stability in the revised Discussion.

6. *Equation 4: Accounting for aggregate structures in the soil samples would required more refined petrophysical models (e.g., Day-Lewis et al., 2017; Romero-Ruiz et al., 2022). But then since equation 4 is not really used in the paper, I'm wondering if it should be removed and the conductivity variations only discuss in the text citing the literature.*

We agree that Eq. 4 assumes a single conducting continuum and cannot quantitatively describe aggregated soils with distinct inter- and intra-aggregate pore domains. However, we would prefer to keep the equation, since our discussion of σ' relies on separating its electrolytic, geometric, and surface contributions. These are the reduced formation factor proposed for MH, the reduced surface conductivity proposed for N-PAM, and the pore-water conductivity driving σ' in the control and ML treatments. We believe the equation helps the *reader follow these arguments. In the revised manuscript, we will present Eq. 4 explicitly as a conceptual framework rather than a quantitative model, and clearly state its limitations for aggregated systems.*

Moreover, the studies suggested by the reviewer provide valuable support for our interpretation of the A-PAML results. Day-Lewis et al. (2017) showed that in dual-domain media, the bulk electrical response is affected by solutes in the less mobile domain, which are not captured by fluid sampling. This is consistent with our proposed 'conductive reservoir', in which ions accumulate within hydraulically isolated intra-aggregate pores, contributing to σ' but not appearing in the effluent. We will cite Day-Lewis et al. (2017) and Romero-Ruiz et al. (2022) in the revised Discussion to support this interpretation, and note that applying such dual-domain petrophysical models to aggregated soils is a promising direction for future quantitative work.