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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Geophysically-based analysis of BTCs and ion exchange processes in soil" *by* Shany Ben Moshe et al.

Anonymous Referee #3

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General comments: The manuscript titled, Geophysically-based analysis of BTCs and ion exchange processes in soil, by Ben Moshe et al., details the results of continuous-injection flow-through experiments in (1) a column packed with homogenous calcareous loamy sand and (2) a loamy sand-pure sand layered column. Via a combined geo-electrical (using spectral induced polarization, SIP) and solute transport modeling approach the authors show the applicability of SIP for capturing conservative tracer (Cl-) breakthrough curves. The manuscript is generally well written and clearly organized, and provides a meaningful contribution to research focused on the joint interpretation of geophysical and geochemical datasets, specifically by coupling the two approaches with transport simulations. The quality of the experiments and data are good. The manuscript convincingly shows the added value of geo-electrical signals in capturing

solute breakthrough curves (via the real conductivity) and their potential to also capture ion-exchange processes. However, certain aspects of the joint inverse simulation considering both geo-electrical and geochemical data, such as the seemingly arbitrary consideration of only the partial real conductivity dataset need to be clearly addressed. The interpretation of the imaginary conductivity time-series is rather brief and vague. The authors should combine their time-series analysis with an interpretation of imaginary conductivity spectra to improve the mechanistic description of the ion-exchange processes driving polarization signals. In addition, the introduction and discussion would both benefit from a comprehensive review and critical consideration of the current literature. These, along with other specific comments outlined below, should be addressed before the manuscript can be considered for publication.

Specific comments: Lines 35 – 54: The introduction should clearly define what "hydrogeophysics" is. The description of polarization mechanisms does not properly distinguish between EDL-polarization and membrane polarization and this should be clarified. Several of the statements should be properly and fairly referenced. In general the introduction would benefit from a more comprehensive review of the literature. Currently, the manuscript has a very short list of references. Lines 56 – 60: Here and elsewhere, the manuscript should acknowledge previous studies that have combined breakthrough curve analysis in flow through systems with the monitoring of geoelectrical properties (e.g. Davis et al., 2006 and Deng et al., 2020) and combined these with transport models (e.g. Slater et al. 2009 and Mellage et al. 2018). How does the current work build on what has already been done? Similarly, the results of the current work should be compared to those in previous studies in the discussion. Figure 3, Results: It seems as if only a subset of the real conductivity time series data were considered in the Hydrus-1D fitting scheme, and the cutoff was different at different channels (SIP measurement locations). The authors should clearly justify what data were used for model fitting. The authors should also report fitted parameter values. I would argue that a comparably good fit could have been achieved by considering only the Cl- breakthrough. However, the authors claim that real conductivity information could replace BTC analysis. Considering the full time-series of real conductivity would likely yield a different parameter set than using only a subset of the data because of the ion exchange processes and associated signal changes later on. Thus, if only relying on SIP data for breakthrough curve analysis, most likely, a biased set of parameters would be estimated. This is contradictory to one of the main conclusions of the manuscript. Lines 196 – 200: The initial increase in the imaginary conductivity of the loam is not observed in the sand layer (in Figure 3b), however the drop in imaginary conductivity occurs in both the loam and sand layers. The authors should provide an explanation for this difference. Lines 200 – 204: Why would sodium-calcium exchange enhance ion mobility? This contradicts the interpretation of the decrease in imaginary conductivity. How would the inflowing solution and exchange process affect the interfacial/surface conductivity of the loam? SIP spectra: Currently, the manuscript presents select spectra in the supporting information. The loam exhibits a clear sharp frequency peak, indicating a dominant polarization length-scale. Fitting, for example, a Cole-Cole model to the spectra could shed light on changes in polarization length scales and or ion mobilities that would help to improve the current conceptual model provided. Figure 5c: The sand layer seems to be thin in relation to the loam layers, this is also mentioned in the text, line 210. Did the authors consider to what extent the signal in the sand-channel is influenced by the adjacent loam layers? Is the resolution of the effective electrode response volume fine enough to resolve a "sand-only" contribution? It would be good to provide a spectral comparison between the loam and the sand.

Technical comments: Line 4: change soil profiles to soil profile Line 46: What is the low frequency range? Line 65: insert "a" in – and alternating current in "a" wide range of... How was the real conductivity normalized? What frequency is plotted in the time series plots? (This could be justified by presenting spectra)

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