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Comment on bg-2020-438

Andre Revil (Referee)

Referee comment on "High-resolution induced polarization imaging of biogeochemical carbon turnover hotspots in a peatland" by Timea Katona et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-438-RC1, 2021

This hot spot geophysical investigation is definitively a hot subject area (no pun intended). My comments are minor. The paper is well-written, interesting to read, exciting and cite correctly the literature. Data quality is very good and the analysis is done rigorously. My only disappointment is that the authors remain very qualitative in their use of IP while recent works have shown how these tomograms can be used quantitatively.

- Perhaps the abstract is missing a physical explanation if any regarding the field and core observations. Perhaps the statement reported in Line 550 “The strong correlation between the polarization response and the DOC suggest an, as yet not 550 fully understood, causal relationship.” Should be mentioned in the abstract. For me this is an exciting avenue in research !

- Lines 71-80, perhaps (up to you) it would be worth to mention that the ONLY mechanisms that explain the relaxation time is inversely proportional to the conductivity of the material is related to a polarization mechanism inside the metallic particles. ALL the other mechanisms invoked to date in which the polarization is at the grain surface or outside the metallic grains cannot explain this fundamental observation.

- Perhaps the authors will find the recent paper related to the IP signature of bioclogging

relevant to their research Garcia-Artigas R., M. Himi, A. Revil, A. Urruel, R. Lovera, A. Sendrós, A. Casas, L. Rivero, 2020. Time-domain induced polarization as a tool to image clogging in treatment wetlands, Science of the Total Environment, 724, 138189, https://doi.org/10.1016/j.scitotenv.2020.138189.

- I fundamentally disagree with the following sentence which is NOT supported by the underlying physics of IP "The phase of the complex conductivity roughly represents the ratio of the polarization (ð□□□'') relative to the Ohmic conduction (ð□□□'). Therefore, it has been argued that the phase represents the polarization effect better than the imaginary conductivity itself as it removes effects stemming from changes in fluid conductivity, saturation, and porosity (Kemna et al., 2004; Ulrich and Slater 2004)." It is quite wrong and misleading. Only the quadrature conductiivty or normalized chargeability represents the polarization process. What is misleading is that in presence of metallic particles, the phase and chargeability are used because it can demonstrated that these properties are proportional to the content of metallic particles. Period.

- My main criticism is that IP is used qualitatively in this paper while we can do much better in terms of quantitative assessment based on field IP measurements, see for instance Abdulsamad F., A. Revil, A. Soueid Ahmed, A. Coperey, M. Karaoulis, S. Nicaise, and L. Peyras, 2019. Induced polarization tomography applied to the detection and the monitoring of leaks in embankments dams and dikes, Engineering Geology, 254, 89–101, https://doi.org/10.1016/j.enggeo.2019.04.001. Abdulsamad F., A. Revil, A. Ghorbani, V. Toy, M. Kirilova, A. Coperey, P.A. Duvillard, G. Ménard, and L. Ravanel, 2020. Complex conductivity of graphitic schists and sandstones, Journal of Geophysical Research-Solid Earth, 124, 8223–8249. https://doi.org/10.1029/2019JB017628. Revil A., A. Soueid Ahmed, A. Coperey, L. Ravanel, R. Sharma, N. Panwar, 2020. Induced polarization as a tool to characterize shallow landslides, Journal of Hydrology 589 (2020) 125369, https://doi.org/10.1016/j.jhydrol.2020.125369.

- Section 4.3 the authors wrote "The low-frequency polarization response of subsurface materials is usually attributed to either 468 electrode polarization of highly conductive metallic minerals (e.g., Pelton et al., 1978; Wong, 469 1979)". I am quite surprise by this statement since as pointed out above, these two papers cannot explain a fundamental observation that in presence of metallic particle, the relaxation time is inversely proportional to the conductivity of the material. ONLY an intra-metallic particle mechanism can explain this observation excluding ipse factor these models, see for instance Abdulsamad F., A. Revil, A. Ghorbani, V. Toy, M. Kirilova, A. Coperey, P.A. Duvillard, G. Ménard, and L. Ravanel, 2020. Complex conductivity of graphitic schists and sandstones, Journal of Geophysical Research-Solid Earth, 124, 8223–8249. https://doi.org/10.1029/2019JB017628. Similarly Line 471, the citation of Waxmna and

Smits is incorrect since it is dealing ONLY with conductivity and not polarization. The citation of Vinegar and Waxman (1984) would be adequate.

- The explanation about electrode polarization mechanism is wrong and does not make sense. Wong is about charge transfer by redox processes and unbalances in charge transfer at the pore water / metallic particle interface. None of the models invoked in this paragraph can explain the observations in presence of metallic particles except for very small metallic particles as discussed in the series of ten papers published by Revil and co-workers in Geophysics. Lines 481-482 : All the models produces this features, not just the one by Wong.

- Line 489: this explanation is again wrong since an intra particle mechanisms has nothing to care about the concentration of Fe in the pore water. There is a lack of understanding here of the mechanisms of polarization in presence of metallic particles and how they agree or disagree with the observations.

- The conductivity of the metallic particle is TOTALLY irrelevant of the problem as discussed in depth by Revil and co-workers in this series of ten papers mentioned above. Actually a sediment with metallic particles is more resistive than the sediment alone at low frequency, a well-established observations that is corroborated by the underlying physics. There are therefore some flaws in your explanations of electrode polarization here.

- Lines 504-509, this explanation does not explain any observations. Membrane polarization does not explain the basic observations shown by experimentalists regarding the IP effect of metallic-free porous media. Sooner or later, this will be obvious to the whole community working in this are the day people will be more quantitative in their used of IP data in the field (which is again my main criticism here).

- Line 510: the references are not properly cited.

- Line 515, no the CEC is NOT “The product of both surface charge density and surface area”. The surface charge density is the ratio of the CEC by the specific surface area. The word “specific” is missing and is very important (because of the normalization by the mass of grains).

- Lines 521-522, perhaps because it depends also on the water content as shown in many models. That said, yes it is not that the CEC of zeolite and organic matter is not activated for surface conduction and polarization. This has been shown several times in the literature.