Response to Reviewers

BG-2020-438

Title: High-resolution induced polarization imaging of biogeochemical carbon turnover hot spots in a peatland

Journal: Biogeosciences

Relevant changes:

- **1.** Following the suggestion of Reviewer 1, we extended our reference list to present recent developments in the mechanistic models
- 2. Addressing the concern of Reviewer 1, we extended our discussion on the polarization mechanisms and corrected some formulations.
- **3.** Following the suggestion of Reviewer 2, we rearranged the first paragraph of chapter 4.3 to chapter 2.3
- 4. Following the recommendations of Reviewer 2, we rearranged part of chapter 3.1 to chapter 2.3 to improve the structure
- 5. Addressing the concern of Reviewer 2, we reformulated the interpretation of signatures to tight the story and avoid misunderstandings
- 6. Following the suggestion of both Reviewers, we extended our manuscript with an extra paragraph and a new figure to indicate the geometry of the hot spots found in our study.

Reviewer 1

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.

We thank the first Reviewer for his evaluation and the insightful comments. We aim at incorporating all suggestions into the revised version of our manuscript. We thank the Reviewer for the references, which will be included during the interpretation of the signatures. We added the ten papers published by Revil and co-workers regarding details of the electrode polarization to the reference. Furthermore, we tried to extend our manuscript in a quantitative way.

• 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 !

A: We agree with the Reviewer and we have extended our Abstract, which reads as following: "Our results reveal large variations in the electrical response, with the highest

IP phase values (> 20 mrad) corresponding to high concentrations of phosphates (>4000 μ M), an indicator for intensive carbon turnover. Furthermore we found a strong relationship between the polarization response and the dissolved organic carbon."

• 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.

A: We agree with the Reviewer and we have edited our manuscript. In the revised version of our manuscript the line reads now: "Revil et al (2015a, 2015b, 2018b,) concluded, that to the conductivity inversely proportional relaxation time relates to the polarization mechanism inside of the metallic particles."

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.

A: We thank the Reviewer for the reference. We have included it as part of the state-ofthe-art revision, and the Discussion section.

"Garcia-Artigas et al. (2020) demonstrated that bioclogging by bacteria increases the IP response accompanying wetlands treatment."

"Additionally, Garcia-Artigas et al. (2020) concluded that bioclogging due to fine particles and biofilms increases the specific surface area and the CEC, resulting in an increase in the polarization response."

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• œŽ ") relative to the Ohmic conduction (ð• œŽ '). 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 conductivity 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.

A: This section was rewritten according to the comments of the Reviewer. What we wanted to mention here, is that the size of the pore throats influence the polarization and the water saturation of the peat influences the in-phase conductivity. We assume that the peat in our experimental plot is almost fully water saturated (the lowest measured water table depth at the two piezometer was 8 cm), and the pore size is larger in the area East-Southeast side of the experimental plot. This would explain why both the in-phase and

quadrature conductivity are high at the western part of the experimental plot and why the in-phase conductivity is high, and quadrature conductivity is low in the East-Southeast side of the experimental plot. The phase can highlight the high quadrature conductivity values relative to the in-phase conductivity.

"The phase of the complex conductivity represents the ratio of the polarization relative to the Ohmic conduction ($\phi = \sigma''/\sigma'$). Thus, it can also be used to represent the polarization response (Kemna et al., 2004; Ulrich and Slater 2004; Flores Orozco et al., 2020). "

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.

A: Following the recommendation, we suggest new figure, which shows the active biogeochemical areas. As discussed in our manuscript, we consider that the polarization is not controlled by the iron content, as this is present in form of iron oxides which have reported low values in laboratory studies (e.g., Joyce et al., 2012). We believe the high polarization response is due to the high concentrations of Carbon inferred by the high DOC values measured in water samples. As the controls of the polarization response are still open to discussion, we decided not to apply the models proposed by Revil et al. (2015) for the quantification of metal content.



• 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.

A: We agree with the Reviewer in so far as an important part of recent model development has been completely omitted in our first version of the manuscript. However, we are not as convinced as the Reviewer regarding the conclusion that there is only one model left, which is able to explain all experimental results. We consider that a detailed discussion of this topic is far beyond the scope of this manuscript. As our discussion does not rely on any of the very specific/distinct predictions of the two model families (perfect conductor vs. semi-conductor), we decided to generalize our discussion and stick to those predictions, which are the same for both model families (e.g., the proportionality between polarization magnitude and volume content of metallic particles).

Reformulated paragraph, which is now in the section 2.3 after considering the structure changes suggested by Reviewer 2:

"The strongest polarization response is observed in the presence of electrically conducting minerals (e.g., iron) (e.g., Pelton et al., 1978) in the so-called electrode polarization (Wong et al., 1979). It arises from the different charge transport mechanisms in the electrical conductor (electronic or semiconductor conductivity) and the electrolytic conductivity of the surrounding pore fluid, which make the solid-liquid interface polarizable. Diffusion-controlled charging and relaxation processes inside the grain (e.g., Revil et al., 2018; 2019; Abdulsamad et al., 2020) or outside the grain in the electrolyte (e.g., Wong, 1979; Bücker et al., 2019) are considered as possible causes of the polarization response at low frequencies irrespective of the specific modeling approach. All mechanistic models predict an increase in the polarization response with increasing volume content of the conductive minerals (Wong, 1979; Revil et al. 2015a, 2015b, 2017a, 2017b, 2018; Qi et al., 2018; Bücker et al., 2018)."

• 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.

A: We agree with the Reviewer that the extension of electrode polarization needs to be considered, as explained in the papers published by Revil and co-workers. We believe

that the new version of the discussion, which includes the recent work of the Reviewer, is general enough to resolve the issue raised here.

• 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.

A: Previous studies such as Flores Orozco et al. (2011, 2013) demonstrated that there is a correlation between the electroactive ions in the pore space and IP response, in our manuscript we argue that the electrode polarization can be explained based on these findings and the Wong model which was extended by Bücker et al. (2018). Actually, this is one of the experimental observations, which the family of semi-conductor models has not yet been able to explain and the model by Wong (1979) (and further developments) to date (and as far as we understand) provide the only mechanistic explanation for.

• 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.

A: We agree that in the family of models developed by the Reviewer and his co-workers, the conductivity of the particles play a secondary role (or even a smaller role). We included the possibility of a polarization of moderately conductive particles into the revised version of the discussion. In particular, the intra-grain polarization mechanism is more likely to cause a measureable polarization within other iron oxides than magnetite and within iron complexes with organic matter.

• 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).

A: We are using a membrane polarization model (Bücker et al., 2017) to demonstrate a relationship between the high carbon content in the electrolyte and a change in the polarization response. As observed by Revil and Skold (2011) the increase in the salinity leads to an early increase in the quadrature conductivity (and decrease at very high salinity). Nevertheless, we agree with the Reviewer that the membrane polarization can also be formulated in terms of grain size and the polarization around the grain in the electrical double layer. They should provide key valent results in these terms. We think the membrane polarization has to be added, even if there are doubts about whether the membrane polarization is strong enough to contribute to the polarization significantly.

• Line 510: the references are not properly cited.

A: We corrected the citation.

• 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).

A: We agree with the Reviewer, we corrected the line, inserted the word "specific".

• 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.

A: The ground water level was constantly 6-8 cm below the surface during the time we performed our measurements. Furthermore at the surface the peat was almost fully water saturated. Therefore we think that the weak dependence on CEC cannot be explain by the low water content.

Reviewer 2

There is increasing evidence that hot spots of biogeochemical activity exert profound effects on ecosystems that are disproportionate relative to the physical footprint of the hotspots. Geophysical techniques have a role to play in locating such hotspots. Induced polarization is an interesting method as it may be sensitive to indicators of enhanced biogeochemical activity, such as the precipitation of metallic minerals where anaerobic and aerobic waters interact.

I found this paper interesting to read and it includes some nice graphics for illustration. My main concern about the work is that the attempt to correlate IP measurements with indicators of enhanced biogeochemical activity (in this case enhanced carbon turnover) is unconvincing based on the presented dataset. The paper focuses on correlating the IP-measured phase and/or imaginary conductivity with [1] vegetation patterns, [2] chemistry of soil samples, and [3] water chemistry. I could not follow the logic of the argument that the IP measurements are indicative of hotspots and I doubt that the assumed correlations are statistically significant. Figure 8 is presented to argue that the phase is correlated with the vegetation patterning – but how does that represent a hot spot? Figure 10 appears to be presented to show that phase is uncorrelated with fluid DOC, CI- and Ftot in the pore fluid and therefore is an indicator of biogeochemical hotspots (Lines 374-381). I could not follow this logic at all. Later in the paper, the entire upper layer of the soil is identified as a 'hot spot', which seems inconsistent with the idea that there are localized zones in space of enhanced biogeochemical activity. In summary, I really struggled to understand what how the presented dataset characterizes hotspots in this paper.

Another general problem I have with the paper is that I found myself often failing to see the apparent correlations drawn between the geophysical images and the other available site information. The inferred correlations were often unconvincing at best. As an example, on lines 550-551 the significance of a correlation between polarization response and DOC is emphasized, yet the real part of the conductivity shows the strongest correlation with DOC.

I also noted some significant technical errors that suggest some misunderstanding of induced polarization. One that was particularly concerning is the statement given at Lines 297-300. This is entirely incorrect. Ulrich and Slater (2004) and Kemna et al. (2004) definitely did not say this. This statement is particularly concerning as it implies a possible misinterpretation of the IP measurements throughout the paper, which may explain why I really struggled to follow the arguments being made in most places.

A: We thank the second Reviewer for the time taken to review our manuscript and his evaluation. It helps us to find some problems in the structure of the manuscript. We made changes in our introduction and the interpretation and discussion of the imaging results to improve the message. We have also made a careful revision of the citations. We have corrected most of the suggested changes, and we hope the message in the revised manuscript now is better communicated.

We found a high polarization response (quadrature conductivity) although, our chemical analysis shows negligible sulfate reduction. The biogeochemical processes might influence vegetation. Thus, some non-geophysical studies have used vegetation changes to assess biogeochemical hotspots' presence (e.g., Kleinebecker et al., 2008). We do not believe that the vegetation controls the electrical signatures (conductivity and polarization), but we use them as an independent validation of our interpretation. As Figure 8 shows, the phase value is higher in the area where the dense vegetation is observed than in the area where the vegetation is rare. Although the phase shows a low correlation with the geochemical parameters, altogether, the phase values are high at active biogeochemical areas. Therefore, we interpret the high phase values as the hot spots. We have edited our manuscript to make this clearer.

We do not consider the entire top 10 cm as a hot spot; this was a poor formulation on our side. The manuscript will be revised to state that we observed high polarizable values within the 10 cm in well-delimited areas defined as hot spot. We propose to include the figure below at the end of our manuscript, where we indicate the hot spots' geometry at different depths.



The quadrature conductivity and phase values appear to be sensitive to variations in the peat unit's thickness (i.e., the contact to the bedrock). We agree with the Reviewer that the bedrock contact and the low polarization values do not agree to 100%, but the depth of the contact to the low polarization response clearly reacts to variations in the depth of the peat-granite contact. We corrected our statement. Additionally, we found variations

in the peat unit's electrical values, both vertical and horizontal (shown in Figure 6-8). To address these inhomogeneities, we conducted chemical analysis on soil and pore fluid samples.

It is true that, in general, the in-phase conductivity has a stronger correlation to all chemical parameters measured in the pore fluid, which is due to the increased fluid conductivity caused by the dissolved carbon. Nevertheless, we use the high DOC as a proxy to illustrate high carbon content in the solid phase, thus defining carbon turnovers (i.e., hot-spots).

The statement at lines 297-300 was indeed poorly written. The in-phase conductivity tends to be higher at higher saturation (Tartrat et al., 2019) and the quadrature conductivity depends on the pore size and saturation (Ulrich and Slater 2004). As we do not have direct measurements at the site concerned with the saturation and pore space, we can only hypothesize based on our observation that the peat in our experimental plot is highly saturated (the deepest measured water table depth was 8 cm bgs) and the pore size is larger East-Southeast side of the experimental plot. This would explain why both the in-phase and quadrature conductivity are high at the West part of the experimental plot and why the in-phase conductivity is high, and quadrature conductivity is low East-Southeast side of the experimental plot. If we use the phase as our main parameter, we see the high phase values in the middle of the experimental plot tended to the Northside, where the quadrature conductivity is high relative to the in-phase conductivity. This difference in the quadrature and in-phase conductivity might be due to the influence of the fluid conductivity, and this is why we insisted on showing the phase values. The phase values permit a better indication of the areas where the quadrature conductivity is high relative to the in-phase conductivity furthermore (Kemna et al., 2004). Moreover, the phase values are directly resolved through the inversion scheme used in our study.

Specific comments:

Line 94: Slater and Binley (2006) is an inappropriate reference – this paper has nothing to do with IP mapping of contaminant degradation

A: Slater and Binley (2006) present changes in the electrical properties following the ground water remediation technics in zero-valent iron permeable reactive barriers. We agree with the Reviewer. This reference is not directly relevant to the degradation of contaminants, and has been removed from the citations.

Line 160: Not sure what you mean here. Fig 2d shows waterlogged/saturated soils but that doesn't mean surface runoff is occurring. Looks pretty flat as expected for a peatland

A: Yes the experimental plot is flat; however, the catchment itself has a bowl shape (Fig. 1b). Furthermore the runoff water can be observed at the surface. We have corrected our manuscript accordingly to differentiate between the experimental plot and the catchment



Line 163: Why the difference in grid resolution?

A: This is related to the time required on the field to conduct the measurements and possible disturbance of biogeochemical conditions. We measured every 50 cm along with the selected IP profiles to have a high resolution; however, we collected data only every fifth of lines assuming relative gradual changes between the profiles.

Line 164: Unclear, what experiments are you referring to?

A: We referred to the IP survey. We modified the sentence to be clearer: "The local groundwater level was measured in two piezometers and was found at ~5-8 cm below the surface during the IP survey."

Lines 184-185: I cannot follow the significance of the averaging described here

A: The precision of the final spectrum is increased by increasing the number of spectrums measured per sample as it reduces noise inherent in each individual spectrum. For the instrument used here (Bruker, Vector 22 FTIR spectrometer) it is standard measurement practice to average 32 spectrums for organic rich samples (see Biester et al, 2014; Broder et al, 2012; Moore et al, 2019)

Line 210: check formatting of prime symbols on conductivity terms – currently strange

A: We modified the format in the manuscript.

Line 218: The 20 cm electrode spacing ss very hard to get right in the field conditions like those shown. What were the positioning errors and how were they incorporated into the processing?

A: We conducted careful protocol and used three measuring tapes to ensure good positioning. The electrodes were placed along with the fourth measuring tape. We estimate a maximum error of ca. 2 cm. However, we did not incorporate these errors into the processing. To address the Reviewer's concern, we present a numerical analysis to quantify deviations in the parameters using a numerical example where an electrode's position has been misplaced. We built a two-layer model reflecting our field conditions with a polarizable anomaly. Forward modelling was conducted with a regular separation between electrodes of 20 cm (mod0), followed by experiments where an electrode above the anomaly is misplaced 5cm to the left (mod1) and then 5 cm to the right (mod2) of the correct position, as seen in the attached figure. This 25% error influences the real conductivity resolved for the polarizable anomaly (deviations in the inverted parameters are 6% and 4% by misplacing the electrode left and right of the real position), and the imaginary component of the conductivity (deviation, 9% and 6%). Simultaneously, electrodes' misplacing shows much less effect on the phase values (deviation, 3% and 2%) since the phase value is less sensitive to the geometric factor. Hence, it would support our interpretation mainly based on the phase images instead of the real and imaginary conductivity. We can present this analysis in the paper; however, we are afraid it would extend the manuscript and not engage the biogeoscience community.





Line 248 and Fig. 4: Line 248 refers to a normalized resistance but caption of Figure 4 suggests Fig 4c. is plotted as absolute resistance difference. Clarify.

A: We plotted the pseudosection of the normal and reciprocal measurement and the belonging misfit. However, we understand this may be misleading and we changed the figure to present the normalized value on Fig. 4c.

Line 264: b is shown as a percentage value but previously defined as a relative error (as needed for the equation

A: We have corrected our manuscript following the suggestion of the Reviewer.

Line 265: Why choose this value for cumulative sensitivity? No justification given.

A: The smallest sensitivity value resolved after the inversion is 10^{-3.6}; with the value 10^{-2.75}, we cut the 10% of the imaging results. We believe that using a threshold value of 10⁻³ (for instance, as suggested in Flores Orozco et al., 2013) would not justify the narrow dynamic in the sensitivity data. Weigand et al. (2017) recommend the threshold value 10⁻⁴. We wanted to reach the 1.5 m as the depth of investigation, and blanking the parameters close to the boundaries, and empirically chose the value 10^{-2.75}.

Line 281: You don't have a deep unit - maybe a 'deeper' or 'relatively deep' unit

A: We modified the text to the following: "and (iii) underneath it, a third unit characterized by moderate to low σ ' (<5 mSm⁻¹) and the lowest σ " (<40 μ Sm⁻¹) values" (see below)

Line 284: Is the porosity of the granite known or simply inferred?

A: It is not known. We inferred.

Line 288: I don't see the correspondence between the electrical imaging and the contact with the bedrock that is being inferred here

A: First of all, we realized a typo error in line 287, the contact represented by the dashed line in Fig. 6 and not the Fig. 5. The imaginary conductivity has the lowest values (below 40 μ S/m) which we associated to the granite bedrock, while the peat has usually higher imaginary conductivity value (above 40 μ S/m). However, the phase values resolve the granite bedrock more clearly (in our opinion). We understand the Reviewer's concern, that the border measured with the manual method and the imaging result does not give a 100% match, especially in the in-phase conductivity. Thus we modified this section.

"Figure 6 shows the imaging results of the N-S oriented profiles By 25, By 46, and the W-E oriented profile By 68 expressed in terms of the conductivity (σ ') and polarization (σ ''). These images reveal three main electrical units: (i) a shallow peat unit with high σ ' (>5) mSm-1) and high σ " (>100 µSm-1) values in the top 10-20 cm bgs, (ii) an intermediate unit in the peat with moderate to low σ' (<5 mSm-1) and moderate σ'' (40–100 μ Sm-1) values, and (iii) underneath it, a third unit characterized by moderate to low σ ' (<5 mSm-1) and the lowest σ " (<40 µSm-1) values, corresponding to the granite bedrock. The compact structure of the granite, corresponding to low porosity, explains the observed low conductivity values ($\sigma' < 5 \text{ mSm-1}$) due to low surface charge and surface area. The shallow and intermediate electrical units are related to the relatively heterogeneous peat (Fig. 6), which is beyond the vertical change and lateral heterogeneities in the complex conductivity parameters. As shown in plots of the σ " Fig. 6, the contact between the second and third units roughly corresponds to the contact between peat and granite measured using the metal rod; thus, evidencing the ability of IP imaging to resolve the geometry of the peat unit. Although, for the survey design used in this study, σ " images are not sensitive to materials deeper than ~1.25 m. Images of the electrical conductivity reveal much more considerable variability and lack of clear contrasts between the peat and the granite materials, likely due to the weathering of the shallow granite unit (Lischeid et al., 2002, Partington et al., 2013). "

Lines 292-293: I don't really see this evidence for higher polarization values in the northern part of the profiles relative to the southern parts

A: We understand the comment from the Reviewer. The variations are more evident in Figure 8b and 8h. The Figure 6 show in general higher polarization response in the northern part than the southern part although, the difference is slightly. Nonetheless, we agree with the Reviewer and removed this statement.

Line 296: Why 'roughly'? At the small phase angles you measure it is almost exact – any differences are well below your measurement resolution.

A: We have corrected our manuscript following the suggestion of the Reviewer.

"The phase of the complex conductivity represents the ratio of the polarization relative to the Ohmic conduction ($\phi = \sigma''/\sigma'$). Thus, it can also be used to represent the polarization response (Kemna et al., 2004; Ulrich and Slater 2004; Flores Orozco et al., 2020). "

Line 305: What do you mean 'with higher resolution' here? Why would the phase images be higher resolution? This is unlikely given the relative errors in phase measurements relative to electrical conductivity measurements.

A: We agree with the Reviewer. We modified "higher resolution" it to "higher contrast".

Line 331: what is meant by 'high electrical response'?

A: We changed the wording to more accurate expression: "and is associated with high electrical parameters in the shallow maps ($\phi > 18 \text{ mrad}, \sigma' > 7 \text{ mSm}^{-1}$ and $\sigma'' > 100 \mu \text{Sm}^{-1}$)."

Line 336-337: Maybe. But could it simply be that some other common property influences both vegetation pattern and IP signal

A: Vegetation has been used in non-geophysical manuscripts to investigate biogeochemical hot-spots (e.g., Kleinebecker et al., 2008). We believe that such reference may provide independent validation to our interpretation of the IP imaging results. However, we do not imply that vegetation is related to polarization. We understand our wording is misleading and will be corrected to make this clearer. If the Reviewer recommends it, we can remove the reference to the vegetation, as this is only an independent validation but not the scope of the manuscript.

Lines 372-373: sentence is not correct as there are no phosphate results for S3

A: We agree with the Reviewer, we did not specify the phosphate to S1. We corrected our manuscript.

Lines 374-381: I don't follow the argument that what is written here means that high phase values can be interpreted as biogeochemical hotspots

A: The in-phase and quadrature conductivity show a high correlation with the DOC and other chemical parameters likely as both are sensitive (σ " is indirect) to the chemical composition of the electrolyte (e.g., Lesmes and Frye, 2001). Accordingly, the phase is less dependent on fluid conductivity and shows the lowest correlation to the geochemical parameters measured in water samples. However, in general, the hotspots found in S1 and S3 still show higher phase values; while S2 indicates an inactive biogeochemical zone corresponding to low phase values. Therefore we interpret that high phase values as highly active areas. We updated our figures and created a new figure to prove (shown above) our findings.

Line 390: previously you refer to upper 10 cm

A: Indeed. We corrected to 10 cm.

Lines 409-411: Need to modify sentence as no phosphate levels for S3

A: We corrected the manuscript, this was a typo error.

Line 434: What do you mean by IP response here? Phase? Imaginary conductivity?

A: We modified it to imaginary conductivity. However, the phase values at S2 show similar behavior.

Line 458: What about the solid phase iron?

A: We discuss the solid phase iron under the section 4.3. The iron in the solid phase seems to be forming iron oxide complexes, which is most probably not polarizing (e.g., Abdel Aal et al., 2014, Atekwana et al., 2016).

Lines 468-482: This material belongs in the Introduction/Background to IP rather than in the Discussion

A: We think this material is necessary to be introduced in the Discussion. However we understand the concern and changed our structure according to the suggestion.

Lines 527-528: Not necessarily the case for iron minerals. Point of zero charge for iron is around 8

Our original statement was about EDL polarization and not electrode polarization (or the polarization of other metallic minerals) and thus excludes the case of iron minerals. Furthermore the point of zero charge for peat material is below the pH4 (e.g., Bakatula et al., 2018).

Line 534-535: Published work supports that in silica-dominated systems pH influence is indeed small

A: Our results, similar to (Slater et al., 2005), show that the pH has an insignificant influence on the measured IP. However, the point of zero charges of the peat is below 4 (e.g., Bakatula et al., 2018), the point of zero charges of the iron (-oxide) is around 8 (Stumm and Morgan, 1996). The measured pH in our case is between 4.5 and 5.8, this means the OM is charged negatively, and the iron oxide is charged positively. The pH at the S2-non hotspot is above 5. Although the pH is ideal for the DOC at the S1 and S3 top 20 cm to bond with the solid phase's iron (Vindedahl et al., 2016).

Lines 550-551. This makes no sense to me – the DOC correlation is stronger with the real part of the conductivity.

A: The in-phase conductivity is usually high at the near-surface and decreases with depth. The real part of the conductivity indeed correlates the most to the DOC. Because the in-phase conductivity decreases with the highest rate with depth. However, as Fig. 9 shows, the in-phase conductivity curve at S1 and S2 show almost no difference although, DOC concentration is way higher at S1 than at S2. If we take a closer look, the chemistry shows a little higher activity at the surface, even at the less active areas, and the chloride content is generally higher at the surface (Fig. 9), which influences the pore water conductivity and thus, the in-phase conductivity.

Line 570-571: Is a layer really a hotspot? It rather destroys the concept

A: We understand that our wording was misleading. We rewrote the conclusion, the hot spots are located within the top layer within the first 10 cm bgs. We add a new figure (shown above), where we delineate the exact locations of hot spots.

"The map presented in Fig. 12 is based on the maps of phase and imaginary conductivity values at a depth of 10 and 20 cm. Hot spots interpreted at those areas exceeding both a phase value of 18 mrad and imaginary conductivity of $100 \,\mu\text{Sm}^{-1}$ at the same time. Besides the geometry of the hot spots, Fig. 12 indicates that the hot spot activity attenuates with the depth."

Reference list:

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