Editor

Comments to the Author:

Andreas Horst and an anonymous referee have reviewed your manuscript. Prof. Hordt is fairly positive, but has two major issues that he would like to see addressed: more attention should be given to the critical issue of the decoupling of IP and EM effects, and the paper should be shortened and more focused on what is really new here. Prof. Hordt also has a number of more minor requests for clarification, or suggestions for improvements. The anonymous reviewer is more critical, especially with regard to your presentation; the need to focus on what is really new here is again raised, along with some concerns about your use of unconventional notation.

Response:

We appreciate the reviewers' constructive comments and suggestions about our manuscript. The common theme concerned focusing and shortening the paper and we have altered the manuscript accordingly. The major alterations include moving sections about a) effective pseudo-chargeability, b) its validations, and c) extracting intrinsic IP parameters to appendices. This material is very important for substantiating our techniques but it presents a hindrance to the reader when it is in the main document. Those movements requires some modification to the main text. We did those and clarified the article based upon the reviewers' comments. We also included a section regarding em decoupling as requested by Reviewer 1. We believe this version of the manuscript is much improved and more readable. We hope that the paper is now acceptable for publication and we thank you and the reviewers for your help in this process!

Reviewer: 1

Comments to the Author(s)

Kang and Oldenburg present a method for the extraction of IP parameters from inductive measurements. Their method includes a number of approximations and assumptions, which are all assessed by the authors in great detail using numerical simulations. I like the basic ideas; the approximations seem to be reasonable and serve to make this complicated problem treatable. Many interesting aspects are being discussed and illustrated that provide some insight into the physics of IP. Therefore, I believe the material should be published after some revision.

I have two main issues that should be addressed:

First, I find that one of the key problems, the decoupling of EM and IP effects, is not treated to an extent that would be required to assess the method conclusively. In the synthetic inversion examples (figures 18 and 19), the decoupling was apparently carried out based on homogeneous halfspace models. The effect of a “wrong” conductivity model was investigated by perturbing the homogeneous halfspace conductivity. However, in a real situation, the background model would be determined from a full 3-D TEM inversion, by using the early-time data. Of course, if the true model actually IS a homogenous halfspace, the inversion of early-time TEM data will always find the correct model, and the method will work.

However, if the true background model is 3-D (it would be sufficient to make the polarizable body conductive), things would be different. Inversion of only early-time TEM data might be insufficient to capture the full background structure, and the subtraction process will not provide the true IP response, but a mixture of insufficient background model and IP response.

Therefore, I would strongly appreciate if this aspect would get more attention.

The second issue relates to the analysis. Although in principle I like the fact that approximations are thoroughly being tested, and some insight into the physics is being obtained, I believe that this was exaggerated in this case. The extensive analysis of all steps makes the paper very long; some figures are discussed only with one brief sentence and do not seem all that important. I see some potential for shortening and focussing. I make some suggestions in the annotated manuscript.

Response:

Thank you for your review. We found your comments helpful and they have resulted in an improved manuscript. We think that the two main issues that you raised: a) estimating background conductivity and removing em-coupling and b) shortening the manuscript, were reasonable. The first issue you raised is a holy grail in geophysics and we make no claim about having solved it in its entirety. Clearly if only a poor estimate of the conductivity can be obtained then the IP data will have artefacts and so too will any inversion. To address this circumstance we have included an extra section (Section 8. Estimating the background conductivity). The important aspect of our paper is to provide an explicit and rigorous methodology for understanding the IP data from TEM systems and for showing how to combine data from a multi-source experiment and still work with a linearized equation for inversion. The work here can serve as a foundation for other studies, including surveys with grounded electrodes. For the second issue, we moved theoretical sections and validation of assumptions to the appendix. We feel these items are essential since, without them, it is not possible to assess whether the important assumptions that are made are reasonably valid. Overall, this rearrangement has made the manuscript much more readable.

The reviewer commented a number of important points in the article with sticky notes. We selected some of them that we think major, and answered as follows:

#1-1 Reviewer 1 comment about “pseudo-chargeability”:

Old page1 lines 11: The term and the difference to the "real" chargeabilities should be explained here

Old page4 lines 21: The use of the term "pseudo" should be explained (because it depends on time, whereas in eq. (1) it does not.

Agree:

In conventional IP we can only recover a “pseudo-chargeability”. The intrinsic, or true chargeability, can only be obtained if we know the data at zero and at infinite frequencies. We do not know that, so working with any other datum yields a “pseudo-chargeability”. This is a measure of chargeability but is not equal to the intrinsic chargeability. (See page 3 lines 16-17).

#1-2 Reviewer 1 comment about TEM and DC conductivity:

Alternatively, one might express this in terms of the DC conductivity. Please explain why this form was chosen. This is important, because it is assumed that TEM methods measure sigma_inf.

Clarified:

The DC conductivity includes the polarization response and that could be worked with. We have chosen to represent our Cole-Cole response in terms of simga_inf, because sigma_inf is our reference when the medium does not have any IP effects. This is then compatible with our goal of inverting early times (high frequencies) to recover a conductivity. Moreover, this usage has an established history in IP processing using inductive sources. It is this conductivity that is responsible for defining the fundamental fields as shown in Smith et al. (1988). We believe that Section 3 shows the rationale behind our choice.

#1-3 Reviewer 1 comment about too short discussion on several figures:

Figure 14, 15, 16 are all discussed only briefly, and the discussion of 16 is not that closely related to 14 and 15. I see potential for shortening and focussing here.

Clarified:

We added more explanation about those figures (See page 45 lines 1-4). We think Fig. 14 and 15 are important and need to be included because they show how we built up the effective pseudo-chargeability for the ATEM case. Rather than omitting those figures we moved those sections to the appendix to make our article easily readable (Figs A1 and A2).

#1-4 Reviewer 1 comment about EM induction effects on galvanic source excitation:

No, this is inaccurate. Even the electric fields of an electric dipole see the effect of induction, i.e. there is a time decay even if chargeability is zero.

Clarified:

We agree with the reviewer's comment, and our text was ambiguous. What we were referring to was a typical EIP problem that used galvanic source but in which there were no EM induction effects. We modified the corresponding text (See page 6 line 16 - page 7 line 3).

#1-5 Reviewer 1 comment:

Explain why it is usually omitted. Because e_ip is small ?

Agree:

We explained why it is omitted in Smith's work (See page 9 and lines 1-5).

#1-6 Reviewer 1 comment:

Maybe somewhere here one should wrap up what has been achieved and what the knowns and unknowns are, before going into the inversion. for example, by here it is assumed that sigma_inf is known ?

Agree:

We add a small wrap up in the article (See page 10 line 13 - page 11 line 4).

#1-7 Reviewer 1 comment:

I am not sure whether I agree with this interpretation. "Charging and discharging" would imply that the current changes sign, but it never does. Can this be resolved ?

Disagree:

The reviewer misunderstood about the sign of IP currents and the relationship between charging and discharging. The currents can be in one direction but they initially increase and then decrease. We refer to the increase in currents as a “charging” phase and a decrease in currents as a “discharging” phase. There is no sign reversal of the currents. This is illustrated by the bip-field shown in Fig. 4a (red lines).

#1-8 Reviewer 1 comment about IP currents:

I disagree. I cannot easily see the improvement.

Disagree:

Look near (0, -350) on the right panel of Fig. 11 and 12. The direction of the true and approximate polarization currents is opposite at earlier time (Fig. 11), but at late time their direction is almost same (Fig. 12).

#1-9 Reviewer 1 comment about previous works for IP inversion Yuval / Hordt:

Maybe the relationship with the Yuval and Oldenburg and the Hördt work would be explained more explicitly. Actually, in the previous work, the convolution was not considered, and it was shown that this works very well. Here, you chose to carry out the full inversion and not use the approximation, but I believe you are not doing the same thing.

As a side, one might consider to use the approximation (skipping the convolution) in your procedure as well.

Agree:

We put explanation about the previous work, and their difference from our work (See page 19 lines 16-24). We don’t dispute the fact that there can be circumstances when simpler procedures might still work.

#Reviewer 1-10 Reviewer 1 comment about

It is a bit surprising that the center curve is so much different from all others. This should be mentioned and discussed (possibly explained ?) in the text.

Clarified:

We explained the reason in the text (See page 45 lines 1-4).

#Reviewer 1-11 Reviewer 1 comment about

The number of curves should be explained somewhere. in figure 14, there are more than 100 locations; this figure shows about 20 curves, because approx. 80 are identical because of symmetry reasons ?

Clarified:

That is due to the symmetry, and we added explanation in the figure caption (See Fig. A2).

Reviewer: 2

Comments to the Author(s)

This paper deals with the splitting of EM information from IP information (and/or vice versa). Actually both are contributions in Maxwell’s equation and only differ by their relative supports (range) in the frequency domain. The proposed formalism lies on this idea, but is not really depicted in such a way.

Actually, this paper could be a good paper, but it is really difficult to read, and I find that the authors do not try to make it readable by adopting old notations and conceptions. It is very difficult to identify the novelty with respect to some papers Oldenburg teams have already published (for instance the papers by (Marchant et al.), and even what is really new with respect to the paper by Smith et al (1988) (all these papers are cited).

Response:

The difficulty that the reviewer had while reading our manuscript is understandable because there are a number of new terminologies for new concepts and validations of assumptions. His comments have motivated us to restructure the manuscript. We note his last sentence that his negative review was not written from a scientific perspective but from pedagogy.

Our article have several major contributions to the IP research. First, we linearized the time domain IP responses as a function of pseudo-chargeability for the inductive source. We meticulously tested our linearization to determine the circumstances in which it can be used. This has not been done previously in the literature. In doing this we also a provide physical understanding about time the domain inductive source IP response. Second, by suggesting effective pseudo-chargeability, we have developed a linear form of the IP functional for multiple transmitters (airborne TEM survey). Again, this is completely new. Third, by using that linear form of the IP functional, we developed a 3D IP inversion method, inverted multiple time channels of the IP data separately, and recovered the 3D distribution of the pseudo-chargeability at multiple times. Since most of the recent research for interpreting airborne time domain IP data is grounded in a 1D analysis, our 3D IP inversion method for the airborne time domain IP data is at the leading edge of this research. Fourth and last, by interpreting recovered pseudo-chargeability as a function of time, we estimated intrinsic IP parameters: time constant and chargeability. We believe the above contributions are new and we also believe that material in our manuscript can provide a firm foundation for future research about extracting IP information from an inductive time domain EM system.

Four examples showing that the main issue of this paper is of pedagogic nature:

#Reviewer 2-1

1) Eq. 1 is written in a form totally different than the one proposed by Cole-Cole (1941). First, Cole-Cole eq. is written in epsilon, and can be used for conductivity. But Eq. 1 is a Cole-Cole written for resistivity, following Pelton’s approach; second the writing it-self, using (1-eta) in factor of the frequency term, though correct, is nowadays no more used.

Disagree:

We expressed complex conductivity in terms of simga infinity, because that is our reference conductivity when there is no IP effect. This was an important construction for us to define fundamental fields, and also a necessary ingredient in our EM-decoupling method.

The reviewer prefers a certain form of the complex conductivity model without (1-eta) on the denominator which parallels to original Cole-Cole (1941). There are a number of complex conductivity models used in the literature (Dias, 2000; Tarasov and Titov, 2013), and each model has some reflection of an IP phenomenon. Each of these models is an attempt to capture the complicated nature of complex conductivity with a few parameters that may, or may not, have a physical meaning. For our research, we needed to choose one. As we commented in the manuscript, we are following Smith et al., (1988) and Marchant et al. (2014) and they used Pelton's approach. Especially, because we use Marchant et al. (2014)'s code for forward modelling, it was reasonable for us to choose Pelton's model. We clarified rationale behind our choice in the article (See page 4 lines 13-18).

#Reviewer 2-2

2) Eq. 2 and the use of Fourier. The delta distribution on figure 1b does not exist in real signal. This is linked to the fact that all signals are causal, and then Laplace's transform MUST be used instead of Fourier transform.

Agree and Disagree:

We agree using Laplace transform is a better representation, hence we changed Fourier transform to Laplace transform.

On the other hand, the use of the delta function is an important mathematical representation in eq. (1). It allows us to clearly define fundamental currents (when the chargeability is zero) using Ohm's law with time-dependent conductivity. Again, this representation is not new to those working in this field.

#Reviewer 2-3

3) In some places, we can read sentences which are nonsenses. For instance, page 6, line 11, we read: "a) galvanic source without EM induction". But expect for DC, that is at the strictly null frequency, you will always have EM induction. The whole paragraph is unclear, and finally frustrating.

Clarified:

See our answer for the first reviewer's comment #1-4

#Reviewer 2-4

4) The inversion scheme is really old-fashioned with respect to the current inversion methods and notations (see for instance A. Tarantola works, available on “Albert Tarantola Web Page”). The choice of the damping parameter alpha (eq. 42, passing by, what is W?) is not discussed, while the L-curve is known to be the best scheme for choosing it.

Clarified and Disagree:

We modified the inversion methodology section based upon the reviewer’s comment.

For choosing the trade-off parameter, we agree L-curve is a good method, but do not agree that an “old-fashioned” method is not good. A number of researchers are still using the cooling-scheme (Haber et al., 2004; Marchant et al., 2012; Yang et al., 2014), and it continues to work successfully for numerous practical applications. Further, we believe this is not our focus of the manuscript, and the cooling scheme that we used is working very well and allows us to recover reasonable 3D models.

My feeling and opinion is that despite a good scientific content, the paper should be rewritten with a special effort of clarifying the argumentation, notation, and should explain more clearly on what relies the method.

In conclusion, I would ask for a full rewriting of the paper, that is rejection with encouraging for resubmitting a clarified version. However, by reading the papers issued from the same team, I think that all this may require benefiting from some external reader to lead to a successful rewriting. Finally, this opinion is not driven by scientific argumentations, but by pedagogic ones.

References:

[1] Kenneth S. Cole and Robert H. Cole. Dispersion and absorption in dielectrics i. alternating current characteristics. The Journal of Chemical Physics, 9(4), 1941.

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[4] D. Marchant, E. Haber, and D. W. Oldenburg. Inductive source induced polarization. Geophysical Journal International, 192(2):602–612, November 2012.

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[6] Richard S. Smith, PW Walker, BD Polzer, and G. F. West. The time-domain electromagnetic response of polarizable bodies: an approximate convolution algorithm. Geophysical Prospecting, 36(April):772–785, 1988.

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[8] Dikun Yang, Douglas W Oldenburg, and Eldad Haber. 3-d inversion of airborne electromagnetic data parallelized and accelerated by local mesh and adaptive soundings. Geophysical Journal International, 196(3):1942–1507, 2014.