Editor
Comments to the Author:
Prof. Hoerdt has reviewed your manuscript a second time. Although he appreciates your efforts to shorten the manuscript and move some material to appendices, he does not feel that some of his principal comments have been adequately addressed. These need to be addressed before we can consider your manuscript for publications. If Prof. Hoerdt is confused about what you have actually done with regard to the background, then you obviously need to modify your explanations so that others are not similarly confused. If he is correct about use of a homogeneous backgrouund, then introduction and abstract need to be rewritten to state at the outset that you make this simplification -- if the theory would support some more complex conductivity model, you could point that out, but you should not mislead readers into thinking that you have actually done this. Please provide an explanation of how you address these comments with your revision.

Sorry for the delay in the review process -- someone who agreed to review this, did not, and never answered emails.

Response:

We appreciate the reviewer's constructive comments on our article. We have clarified our paper regarding Prof. Hordt's misconception about the use of a halfspace conductivity as sig_inf. We have not used a halfspace conductivity to decouple EM effects, but we assumed that the 3D conductivity is somehow obtained. We agree with Prof. Hordt's opinion that this step is crucial. In separate works we have addressed this more fully and shown the applicability of using early-time TEM data to generate sig_inf. For this paper we carried out a first order investigation to provide insight about the effects of having an incorrect conductivity. We hope that the paper is now acceptable for publication and we thank you and Prof. Hordt for helping this process!

Reviewer: 1

Comments to the Author(s)
The manuscript has improved compared to the previous version. The authors have removed a few figures, and moving some sections to the appendix has also helped to make the paper more concise. However, I can not honestly say that I am totally satisfied with the way my comments have been considered, and I find that some revision is still necessary.

First, the issue of recovering a 3D model from TEM inversion still remains. I was expecting that the authors actually could do this, i.e. generate synthetic data for a (true) 3D background model and invert the data to recover the model. This is claimed to be the first step of the procedure both in the summary "1) Invert TEM data to recover a 3D distribution of conductivity" and the introduction "1) estimating the 3D background conductivity and carrying out an EM-decoupling…". Now, it appears that there is no 3D TEM inversion code available, so that they cannot actually carry out the first step of the procedure, and have to assume that the background is a homogeneous halfspace.

Therefore, I suggest that the paper is clarified in this respect from the beginning by honestly describing what is actually being done: "assume the background model can be described by a homogeneous halfspace and substract the inductive halfspace response from the signal". The misunderstanding that a 3D inversion of TEM data is being carried out should be avoided. This option might be mentioned in the outlook at the very end, but should not be formulated as a promise that is not kept.

The term "decoupling" should be avoided as well, because it is misleading, as subtracting a homogeneous halfspace response is not the same as decoupling. Following Wait and Gruszka (1986): "…electrochemical effects, causing the resistivity to be complex and frequency-dependent, are inextricably interwined with the electromagnetic effcts. Thus we conclude that any sweeping claims to correct induced polarization in the field by various electromagnetic coupling "removal" schemes, should be viewed with concern". I believe this is still correct, and the impression that decoupling is carried out in this paper must avoided by any means.

Concerning shortening and focussing, the authors have done some work, which is appreciated, but the paper is still long, complicated and difficult to read. There is still potential for further focussing. I made some suggestions on the original version, which were ignored, but I still believe these are useful.

Response:

Thank you very much for your review. Two main issues are raised by Prof Hordt: a) estimating conductivity by inverting TEM signals, and b) the use of the term "decoupling". For the first issue, I believe there was misunderstanding between us and the reviewer. Our writing about estimating 3D conductivity was somewhat misleading, because we have not performed any 3D TEM inversion to recover 3D conductivity. We have clarified this issue in the paper (See Page 1 lines 9-11, Page 18 lines 15-20, Page 21 lines 13-14). In addition, σ_∞ is not a single value, but a 3D distribution. The reviewer's comment that we are using a halfspace conductivity for EM-decoupling arises from misunderstanding of term. The 3D σ_∞ model we used in the IP inversion example includes a conductive block in halfspace conductivity hence, sig_inf is a 3D function. For EM-decoupling and forming the sensitivity function, we assumed the 3D distribution of σ_∞ is known (See Page 17 lines 24-26, Page 18 lines 15-20). We clarified our paper, and emphasized that σ_∞ refers to the 3D distribution of conductivity at infinite frequency. The second issue is also related to a similar misunderstanding about σ_∞. For EM decoupling we have not used a halfspace conductivity, but a 3D distribution of σ_∞ (a conductive block embedded in halfspace). Based upon our definition of the IP response (Eq. 16) with a 3D distribution of σ_∞, we believe using the term "decoupling" is reasonable.

It is important to note that we have changed the wording in the first item of our workflow. Previously the first item said: " invert early-time TEM data to a 3D distribution of conductivity". We did not illustrate that here. As a consequence we have altered the verbage to say "obtain" a 3D conductivity to be used as an estimate of σ_∞. That estimate could come from a 3D inversion of early-time TEM data that are believed not to be contaminated with IP, or it could come from another survey or a guess.

I am not delivering an annotated manuscript this time, and list a few details below instead:

1) It is still not clear to me what a “galvanic source without induction effect” (p.7) should be. A galvanic source (electric dipole, for example) also produces induction currents, as soon as the current in the source is switched, the situation dB/dt=0 cannot be achieved. Therefore, induction currents also exist in conventional EIP. As written now, the impression is created that a galvanic source is a source with dB/dt=0, which is not true. In this respect, figure 2 is also misleading: The electric fields of a galvanic source are not instantaneous and also have a transient. The difference is that induction currents may be negligible when using a galvanic source rather than when using an inductive source.

Agree:

We have modified “galvanic source without induction effect” to “DC source with grounded electrodes” (See Page 6 Line 16 - Page 7 line 2).

2) Page 13, line 7, units of tau missing.

Fixed:

See Page 13 Line 7.

3) The new section 8 should be obsolete after considering my comment above, i.e. it should be clarified from the beginning that the method as presented relies on describing the background as a homogenous halfspace that can be described by one single conductivity value.

Clarified:

We have clarified that we are not assuming a constant halfspace conductivity, but we are using a known 3D distribution of σ_∞. Although not performed in this paper, we denoted that inverting early TEM signals can be an effective option for recovering 3D conductivity to proceed with EM-decoupling and 3D IP inversion.

Sincerely,

A. Hördt

Reference:

Wait, J.R. and Gruszka, T.P., 1986. On electromagnetic coupling “removal” from induced polarization surveys, Geoexploration 24, 21-27.