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Abstract. Reply to Reviewer # 2, M.Sc Niels Grobbe

Seismo-electrics, electro-seismics, and seismo-magnetics for earth sciences by L.Jouniaux and F. Zyserman, Answer to Reviewer #2

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1 Introduction

Dear Niels Grobbe, we are glad you have found our manuscript worthy of publication. Below you can find your comments and our answers to them, detailing the changes introduced in our work. In the manuscript these changes are in green color.

2 Comments and Answers

C1. ...However, there are several points of improvement in my opinion. First of all, the authors should try to avoid using the term electromagnetic (EM) WAVE. They should use electromagnetic field instead, throughout the manuscript. This, since for the frequency bandwidth under consideration, the EM signals are diffusive fields. The difference is nicely explained in Low-frequency electromagnetic fields in applied geophysics: Waves or diffusion? By Lars O. Løseth et al. 2006.

R1. In the well known reference you mentioned, the authors establish in their Discussion "...We conclude that one might call low-frequency propagation of EM fields in conductive media what one prefers. But when one characterizes field propagation as diffusion, it might be clearer to add that one is not referring to the random motion usually affiliated with diffusion processes. When field propagation is characterized as wave propagation, one should remember that waves are highly dispersive and strongly attenuated."
We have changed the term "wave" to "signal" in most parts of the manuscript, but we have kept it where we considered the topic being discussed was clearer by using this word. We have added, however, a comment on the behaviour of the low-frequency electromagnetic field in conductive media in the introduction p.2.

C2. Regarding the title of the manuscript: it should contain the fact that it is a review/overview paper somewhere in the title. Furthermore, the authors should try to be consistent in the use of their seismoelectric, electroseismic and seismomagnetic terminology for the situations under consideration, otherwise there is no need to distinguish between these terms (e.g. page 2568, line 16: electroseismic instead of seismoelectric). The best way to cover all is to use seismo-electromagnetic. Now the terms are quite randomly used.

R2. We have changed the title of the manuscript, and checked the terminology we use in order to be consistent with the situation under consideration. We corrected the mistake you pointed out, among others. We think that there are no remaining inconsistencies.

C3. The authors provide a nice theoretical overview, but almost completely avoid to address the also well-established quasi-static EM approach as used by for example A. Revil. For completeness, this approach should be discussed.

R3. Note that some details are given for the quasi-static approach as eq. 2, and in the whole part 4.1: Role of key parameters on the steady-state electrokinetic coupling, where the work of Revil et al. (2007) is cited. We have added new references on the quasistatic approach, namely (Revil and Linde, 2006; Revil and Jardani, 2010; Revil and Mahardika, 2013); see subsection "2-D modelling in vertically and laterally heterogeneous media".

C4. Furthermore, the authors should indicate which definition of the Fourier Transform they use, since this has implications for the signs of the imaginary parts of their equations.

R4. We have mentioned that the time dependence is assumed to be $e^{-i\omega t}$ throughout the manuscript.

C5a. "The authors should reformulate certain parts ..."

R5a. We agree that these signals are small and difficult to detect: we pointed out this limitation in the conclusion. However, several studies showed that these signals can be measured in the field and interpreted, as described in the section 6: Thompson (1939), Martner and Sparks (1959), Thompson and Gist (1993), Butler et al. (1996), Mikhailov et al. (1997), Russell et al. (1997), Garambois and Dietrich (2001), Hornbostel and Thompson (2005), Thompson et al. (2005), Kulessa et al. (2006), Strahser et al. (2007), Dupuis et al. (2007), Haines et al. (2007), Thompson et al. (2007); Hornbostel and Thompson (2007), Dupuis et al. (2009). These studies are convincing tests where the seismo-electromagnetic signals are detected.
We have changed "can" to "can, in principle".
The absolute amplitude measurements of IR are "true" in the sense that they have been measured and quantified, both in the field and by laboratory experiments.
When writing a review it is important to make some comments on the different approaches, and what is more or less appropriate. In that sense we made some comments about the eq. 21, and detailed this comment in our reply to the comment of A. Revil.
We have made our best to write the article in a fairly good English. As it is not our native language, it is very likely that the writing style can be improved. We think, however, that the manuscript can be easily read, and no misunderstandings arise from the way in which we have written it.

C5b. “Furthermore the authors should remove redundancy....do not require additional redundant introductory information.”

R5b. We provided details on technical characterization of the sources, acquisition, electrodes, etc, useful for the field observations, and for the laboratory experimental setup. We summarized these informations at the beginning of the section 6: Field observations in one hand, and at the beginning of the section 7 : Laboratory observations. The characterizations of the sources and electrodes are different for field and laboratory experiments because the frequency and wave-length are different ; Moreover the question of the equilibrium time is important for laboratory experiments. Instead of describing these technical features for each experiment, we summarized the overall characteristics at the beginning of the section 6 and 7. But these characteristics remain different for field and laboratory observations, so that we kept these descriptions at the beginning of each of these parts.

C6. "Finally, the authors should provide more references on for example..."

R6. The references Vinogradov et al. (2010) and Jaafar et al. (2009) were already cited in section 4.1.1. We have incorporated, and commented, the suggested references: We added the reference Luong and Sprik (2014) at the end of the mentioned section, stating that "Recently Luong and Sprik (2014) also proposed that the zeta potential is constant over a large range of electrolyte concentration. ", and the reference Schoemaker et al. (2012) in section 3.1, just after Eq. 15, stating that "Moreover Schoemaker et al. (2012) showed that the theoretical amplitude values of the dynamic streaming potential coefficient are in good agreement with their normalized experimental results over a wide frequency range, without assuming a frequency dependence of the bulk conductivity".

Specific comments

C1. 2564-1, 2564-2, 2564-3, 2564-4 and 2564-5.
R1. We introduced the suggested changes.

C2. 2564-13/14 and 2564-15: use electromagnetic instead of electric...
R2. We followed your suggestions.

C3. 2565-8: interface RESPONSE or seismoelectric conversion...
R3. Corrected.

C4. 2565-11-13: Add references.
R4. We added references (Garambois and Dietrich, 2002) and (Haines, 2004) in the paragraph involving the mentioned lines.

C5. 2565-11/12: potentially it can ...
R5. Changed.

C6. 2565-17: always a polarity reversal for interface response fields???...
R6. Corrected.

C7. 2565-18-20: be careful about your seismic velocity estimation...
R7. We are giving, at this point, a description of the SEM for a simple case; we agree that complex geometries would render the procedure of estimating the interface depth from the arrival time of the EM wave rather difficult. We have added a statement in the introduction implying that the

description corresponds to a simple geometry.

C8. 2566-11: what Russian and Israeli experiments? Elaborate.
R8. Improved. Notice, however, that the Russian and Israeli experiments are detailed in a review paper from Neishtadt et al. (2006), that we are not detailing here.

C9. 2566-13: more references (see general comments).
R9. We cite here the reviews already published on the subject. To our knowledge only these references have been published: Beamish and Peart (1998), Gharibi et al. (2003), Neishtadt et al. (2006), Jouniaux and Bordes (2012), Jouniaux and Ishido (2012). The goal of the lines we wrote is not to repeat what is described in the previous reviews, but to inform the reader about them, so that she/he can have a larger knowledge of what has been already written on this subject.

C10. 2566-21: Start new sentence after Frenkel...
R10. Corrected.

C11. 2567-4: reciprocal instead of reverse.
R11. Corrected.

C12. 2567-10: actually, were ELECTROSEISMIC when electric current is the source.
R12. Changed.

C13. 2567-15-end: Nice, but rephrase and correct typos!
R13. Rephrased and corrected. Added (Pride,1994) as suggested.

C14. 2568: first lines: reformulate 2568-5,6
R14. We changed the wording. We had already cited the work of Russell et al. (1997), we consider no new reference is necessary here.

C15. 2568-18: coupled EM and poroelastics (not acoustics!).
R15. Corrected.

C16. 2568: see general comments (quasi-static approach)
R16. We have added new references.

C17. 2568-23: add Pride 1994: reference.
R17. We deem not necessary to include (Pride, 1994); we begin making an account of previous results. However, we modified the sentence to avoid misunderstandings.

C18. 2569-eqns 2 and 3: emphasize how the two equations are linked (J_e)
R18. The equation 3 is the electric current density as a function of frequency, while the equation 2 is the electric current density at low frequency.

C19. 2570- mention that boldface symbols...
R19. Corrected.

C20. 2570-14: zero instead of null
R20. Corrected.

C21. 2571: avoid re-declaring variables (such as angular frequency)
R21. Corrected.

C22. 2573 - lines 9-19: Here I have a serious problem with the description...
R22. We reformulate the subsection 3.2 as: The coseismic field measured on the surface can be associated with surface waves, including the direct (compressional) wave and the surface/Rayleigh wave, travelling along the ground surface.

The interfacial response can be induced by (i) a first mechanism occurring in the first Fresnel-zone, when a spherical P wave traverses an interface directly beneath the source: this is the IR providing instantaneous arrivals across arrays of surface dipoles. This mechanism occurs at interfaces between different streaming potential coefficients. (ii) a second mechanism related to the refracted-head wave travelling along the interface, which generates an electromagnetic field providing time-dependent arrivals at arrays of surface dipoles. This mechanism occurs at interfaces with a difference in both acoustic and electrokinetic properties. We kept the possible explanation of time-dependent arrivals at surface dipole as a consequence of the refracted head-wave, which is still a plausible effect.

C23. 2575-13-16: Add reference (Grobbe and Slob, 2014).
R23. Added.

C24. 2576-5: transverse electric MODE ...
R24. Changed.

C25. 2578-eqn 21: explain more clearly where exactly the permeability comes in.
R25. Please, see the answer of the next comment.

C26. 2579-1: should not be used: formulate less strict: are you fully 100% sure.
R26. We have thoroughly backed this statement in our response to A. Revil's comment on our manuscript.

C27. 2581-end: mention maybe that solving uniquely for the permeability...
R27. We devote a subsection to works describing attempts to solve for the permeability using SEM, and we also mention it as a research goal in the Conclusions.

C28. 2590-end: Add references (Grobbe and Slob, 2013; Grobbe et al., 2014) and related comments.

R28. We have added these references, and a description of the mentioned methodology at the end of Section 5.1

C29. 2591-4: ...denying this fact...
R29. We changed the wording, formulating the statement in a weaker way.

C30. 2594-16: ...elaborate a bit on electrokinetic feedback.
R30. We changed "electrokinetic feedback" by the more appropriate "electrofiltration feedback", and added a short explanation for this term.

C31. 2595: Yeh et al. 2006...
R31. We have moved the reference, and its comment, to the more appropriate Section 5.1.

C32. 2596: Another reference....
R32. Added.

C33. 2597: Introduction on data processing....
R33. We have added a brief introduction.

C34. 2598: Add reference.
R34. We have added the mentioned reference, along with a comment.

C35. 2599: polarity reversal IR...
R35. Corrected.

C36. 2600-11: field measurements can...
R36. Based on our response R5, we consider that "can" is appropriate.

C37. 2606: also horizontal borehole acquisition...
R37. We have added the term "horizontal".

C38. 2616-25: Be careful! Yes, the magnetic field is solely...
R38. We do not understand the necessity of coupling the EM field with P-waves through S-waves. It is clear from the literature (Pride and Haartsen, 1996; Pride and Garambois, 2002) that in homogeneous media the coseismic electric field present in the P-wave is generated by charge separation, and the coseismic magnetic field present in the S-wave is caused by streaming currents, and the time variation of this magnetic field creates the (small) induced coseismic electric field in the S-wave.
For the interface response due to the P-wave, the "effective dipole" comes from different charge accumulations in the incident and reflected wave at one side of an interface, and in the transmitted wave at the other side. The nature of the origin of the IR associated to pure S-waves is still debated; Haines and Pride (2006) considered that across an interface the latter produces an imbalance of electrokinetic current, leading to charge accumulation on one side of the interface and depletion on the other; thus, like a compressional wave, it will induce electric dipoles.
Notice that it is not correct to affirm that the quasi-static approach renders no IR, because the source term includes derivatives of physical properties. At any interface between two media with discontinuities in them, any shear waves present will act as an electrical source, as also Haines and Pride (2006) stated.
Having said this, we acknowledge that the used expression was not clear; we have improved it.

C39. 2619: permeability inversion: be careful...
R39. Given that we are mentioning results from peer-reviewed articles, we consider that you should have stated what does not convince you, and explained why; otherwise it is no possible to improve our description.

C40. 2620-first lines: redundant, or too late stated in manuscript.
R40. We have removed the mentioned lines.

C41. 2621-2: limit → challenge, small level...
R41. Corrected.

C42. 2621: discuss that still in terms of absolute amplitudes...
R42. We added your suggestion.

C43. 2621-15,16,20: Minor corrections
R43. We followed your comments.

C44. 2643, figure 6: consider removing this figure, in my opinion...
R44. We finally kept this figure, because it provides a possible explanation of time-dependent arrivals at surface dipole as a consequence of the refracted head-wave, which is still a plausible effect.

C45. 2650, figure 13, change font-size:
R45. We display now this figure as figure 13a and figure 14

3 Conclusions

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