Solid Earth Discuss., 7, C1251–C1257, 2015
www.solid-earth-discuss.net/7/C1251/2015/

Solid Earth
Discussions
Open Access

Interactive comment on "Seismo-electrics, electro-seismics, and seismo-magnetics for earth sciences" *by* L. Jouniaux and F. Zyserman

M.Sc. Grobbe (Referee)

n.grobbe@tudelft.nl

Received and published: 28 October 2015

General Comments First of all, I would like to congratulate the authors with a very nice overview/review paper on the seismo-electromagnetic geophysical method. As the authors acknowledge, there has been a wide variety of extensive research carried out over the last decades. The authors nicely present a detailed overview of landmark seismoelectric research contributions. They provide enough in-depth details of these contributions, such that the reader can understand the main conclusions of these works. I think this overview can be valuable to both established and new seismo-electromagnetic researchers.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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.

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.

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.

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.

The authors should reformulate certain parts of their manuscript, thereby especially paying attention to not make too strong claims about the method in terms of its applicability for exploration and as a geophysical tool in general. Yes, the method is very promising, however, there are still not many convincing field tests where the signals that we are after are clearly measurable/detected. This is still the major challenge that has to be overcome in order to make the method being used in industry. Having that said, for hydrology, there are some interesting proven applications. The authors should not oversell the method, and try to be a bit more balanced, without losing the enthusiasm of describing the methods potential. An example: page 2600, line 11: Field measurements CAN. . . . → reformulate, these successful measurements are difficult. Also, the authors should be careful making too strong statements about what is right

SED

7, C1251–C1257, 2015

and what is wrong. Seismo-electromagnetics is a very complex phenomenon and still by far not fully understood. We are making great progress, however, for example absolute amplitude measurements of interface response fields have not yet been validated with theory/numerical modeling.

Furthermore, the authors should remove any redundancy in order to shorten the discussion paper. Sometimes, similar information has already been provided earlier. The authors should carefully restructure their paper such that similar topics are more consistently grouped and do not require additional redundant introductory information. In addition, the English is not always of sufficient quality. Sentences should be carefully reformulated and checked on grammar.

Finally, the authors should provide more references on for example streaming potential laboratory research (e.g. Schoemaker et al, Experimental Validation of . . .... IJG, 2012, Luong and Sprik, IJG 2013, Vinogradov and coworkers (several publications) etc.). Also, the many seismoelectric works using the quasi-static approach (Revil et al, Jardani et al, Sava and Revil etc.) should be addressed more carefully. Certain parts have enough references and are well-balanced, in other parts there are only references close to the own group. . . In those cases, more references are required if available (as the suggestions for example above).

Specific Comments Note that the numbers #-# indicate page number and line number:

2564-1 The seismoelectric method can be used 2564-2 ice is not a fluid! 2564-3 and also help to better . . .. 2564-5 However, a challenge of the method (it is not really a limitation, it is a challenge that has to be overcome) 2564-13/14: use electromagnetic instead of electric 2564-15: several thousands of meters is not realistic. The only good field tests of the method go to depths of a few meters. The Exxon results have never been reproduced and there are severe doubts in the community about this. 2565-8: interface RESPONSE or seismoelectric conversion 2565-11-13: add references! 2565-11/12: potentially it can be used for permeability etc. 2565-17: always a polarity

SED
7, C1251–C1257, 2015

reversal for interface response fields??? Be careful! This is dependent on the receiver type (vertical or horizontal electric fields measured for example. And how about the effects of the source-type?). → Adjust throughout manuscript. 2565-18-20: be careful about your seismic velocity estimation. Not always soil measurements. What if the seismic path reaches great depths in the earth? You need velocity estimation procedures. Furthermore, determining the interface depths from IR fields is also tricky: consider the case of multiples, multiple interfaces, complex realistic geometries and so on. This part should be rephrased!

2566-11: what Russian and Israeli experiments? Elaborate. 2566-13: more references (see general comments). 2566-21: Start new sentence after Frenkel. Remove 'while'. 2567-4: reciprocal instead of reverse 2567-10: actually, were ELECTROSEISMIC when electric current is the source. 2567-15-end: Nice, but rephrase and correct typos! 2567-16: strange that you mention Butler here in 1996: Pride 1994 was earlier and showed these mechanisms already. 2568: first lines: reformulate 2568-5,6: references. 2568-18: coupled EM and poroelastics (not acoustics!). 2568: see general comments (quasi-static approach). 2568-23: add Pride 1994: reference. Now it seems the authors have derived this theory... 2569-eqns 2 and 3: emphasize how the two equations are linked (J_{e}). 2570- mention that boldface symbols denote vectorial quantities. 2570-14: zero instead of null 2571: avoid re-declaring variables (such as angular frequency). Also, try to reorganize this section a bit. 2571-7: Schoemaker et al., 2012 is the journal paper to refer to. 2571: specify not only the order of the Bessel functions but also the kind of Bessel function. 2573: lines 9-19: Here I have a serious problem with the description of the theory and especially the refracted wave story. There is no real evidence (both theoretically and experimentally) that a refracted wave continuously emits IR-type EM signals! As you show later, only the first Fresnel zone contributes to the IR signals. Furthermore, you should clearly distinguish IR fields from coseismic (coelectric) fields. Coseismic fields can be associated with direct, reflected, refracted and surface waves. Reformulate and correct this whole part carefully, with appropriate references. Consider removing the figure of Beamish. In contrast, the part

SED
7, C1251–C1257, 2015

of the Fresnel zones is nice, clear and insightful.

2575-13-16: thin-beds: add also the reference to the recent study by Grobbe and Slob (2014): "Seismoelectric interface response signal behaviour in thin-bed geological settings" 2575-27/2576-1: are you sure ALL shear-wave related coseismic and IR responses are purely an induction result? Any references proving this? I am not convinced, it is wrong in my opinion. In addition, this would mean that the quasi-static approach of A.Revil will not show any S-wave related SEM responses. Be very very careful with this statement, this is most likely wrong. Furthermore, I have seen numerical modeling results having a strong S-wave related electric field. The signal strengths also depend on which source-type is used: vertical force sources (mainly compressional waves), or horizontal shear sources (generating mainly shear waves) etc... 2575-5: transverse electric MODE and transverse magnetic MODE. 2578-eqn 21: explain more clearly where exactly the permeability comes in. 2579-1: should not be used: formulate less strict: are you fully 100% sure. Current seismoelectric theory still cannot explain everything that we observe in the field or in laboratory experiments.. I agree it is not the appropriate way of doing things, but be less strong in your statements (this holds for the whole manuscript).

2581-end: mention maybe that solving uniquely for the permeability using seismo-electromagnetic signals can be challenging and should be further investigated. The dependence of the signals on different SEM parameters is hard to pin-point down, since many parameters affect each other.

2590-end: The modeling section provides a very complete overview, but I am aware of only one reference clearly missing. Refer for completeness, since it is a review/overview paper, also to: Grobbe et al. (e.g. 2013 "Validation of an electro-seismic and seismoelectric modeling code, for layered earth models, by the explicit homogeneous space solutions" and 2014: "Seismoelectric wave propagation modeling for typical laboratory configurations: A numerical validation") have developed also a layered-Earth analytically-based numerical modeling code (making use of a Global

SED

7, C1251–C1257, 2015

Reflection Scheme) similar to the code of Garambois and Dietrich. Grobbe et al. have implemented all existing possible seismo-electromagnetic and electromagneto-seismic source-receiver combinations. The code makes use of alternative, more stable eigenvector sets (but can also use the Haartsen and Pride based eigenvectors), and is capable of modeling fluid/porous medium/fluid transitions, thereby enabling modeling typical seismo-electromagnetic laboratory wave propagation experiments. Another reference that is missing and should be discussed are the works by A.Revil and coauthors, Jardani et al etc. which make use of the quasi-static EM approach for modeling the seismo-electric system.

2591-4: denying this fact. Formulate less strong... See earlier explained reasons. 2594-16: elaborate a bit on electrokinetic feedback.. 2595: Yeh et al. 2006: is this a layered Earth scenario, or flexible geometries like (finite difference/element)? Reorganize to the proper location in the manuscript. 2596: Another reference: Inversion attempts: recently, Maas et al. (2015) "Electromagnetic and seismoelectric sensitivity analysis using resolution functions" have carried out a sensitivity analysis using resolution functions as structured first steps towards inversion.

2597: introduction on data processing and its importance is lacking. Please add..

2598: Grobbe et al., Geophysical Prospecting 2015 "Unified multi depth level field decomposition", discuss an effective possible way of decomposing seismo-electromagnetic data into up/downgoing waves and the different field types. This is crucial for many data processing and imaging steps in geophysics. This reference should be added in my opinion.

2599: polarity reversal IR: again: depends on which receiver components we are looking at....

2600-11: field measurements can.... NOT SO STRONG: field measurements are difficult and convincing tests, especially at depths greater than several meters, are still lacking...

SED
7, C1251–C1257, 2015

2606: also horizontal borehole acquisition scenarios might be useful for SEM.

2616-25: Be careful! Yes, the magnetic field is solely coupled to shear-waves. However, the electric field is coupled to both compressional waves and shear waves. The coupling between fast and slow Biot waves and EM occurs via the shear wave (this is also visible in one of the modes, in for example a 2D scenario: P-SV-TM: P is coupled to TM via SV. At zero-offset, if there is no P-SV conversion, there is also no EM field. . .). . . Correct this throughout manuscript.

2619: permeability inversion: be careful. How unique?? Porosity and permeability are related. I am not convinced by the work the authors refer to. 2620-first lines: redundant, or too late stated in manuscript.

2621-2: limit → challenge, small level → weak signal strength, low signal-to-noise ratio.. 2621: discuss that still in terms of absolute amplitudes of the seismo-electromagnetic signals (as well as other field/lab observations), theory/modeling and observations still often do not match, indicating that the seismo-electromagnetic theory is till not perfect and needs improvements and further studying. 2621-15: interface response related to shear-waves: already displayed in many numerical results. . . 2621-16: seismo-magnetic → seismo-electromagnetic in the field.. 2621-20: cylindrical: what do you mean? Layered-earth with cylindrical symmetry, or borehole models, or?? 2643, figure 6: consider removing this figure, in my opinion not correct (see above). 2650, figure 13: change font-size, currently way too small..

Technical Corrections See above.

Interactive comment on Solid Earth Discuss., 7, 2563, 2015.