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

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Review of the manuscript by L. Jouniaux and F. Zyserman. I have various (some of tehm being pretty serious) problems with this manuscript. These problems are listed below and should prevent the publication of this manuscript in its present form.

1. In the abstract, why limited the seismoelectric method to exploration versus monitoring. It does not make sense. Also there are very few studies of seismoelectric exploration but the method still need to be assessed as an efficient exploration tool. Finally, the depth of investigation is very limited due to the smallness of the seismoelectric conversions.

2. The term "induced signal" could appear weird in an electromagnetic context sine the

coupling has nothing to do with EM induction. Better using a more appropriate wording.

3. First sentence of the introduction: What about many other techniques: induction based, RMN, Gravimetry, mag etc.

4. Saying that the seismoelectric method can reach several kilometers in depth of investigation is clearly overselling the method. This is not acceptable. Researchers have already a hard time to identify clear seismoelectric conversions when the target is 20 m deep, so 2 km!

5. I am not sure what the authors mean by "This method works advantageously in detecting zones of high fluid mobility". How "mobility" is defined in this context.

6. The interface response is not just produce by interfaces between two homogeneous materials. Any kind of heterogeneity in the electrical and hydromechanical properties with a length > the wavelength of the seismic wave / 8 would generate a seismoelectric conversion. So the statement made by the authors is not correct. It is also unclear if the seismoelectric method is more sensitive to permeability versus water content since Pride theory, usually used to make conjunctures in this domain, is not complete.

7. It seems that the section entitled "History" is heavily borrowed from the recent book on the same topic: The Seismoelectric Method: Theory and Application by A Revil, A Jardani, P Sava, A Haas - 2015.

8. L. 2565: Electroosmosis was firt discovered by Reuss!

9. End of section 2: it is untrue that Pride and Haartsen proposed an anisotropic seismoeelctric theory. Actually the variations in the eigenvalues and eignvectors iof the vrious material properties tensors entering this coupled problem is still a matter of debate.

10. The statements below Eq. 21 is quite arrogant. Revil and co-workers have obtained the electrokientic equations by volume-averaging and the fact that the authors mentioned that QV should be related to the CEC shows that the authors have not un-

dertnd this tyope of modeling and the various approximations developed by Sava, Revil, Jardani using the acoustic approximation. The Qv entering the coupled equations is a dynamic QV (only a fraction of the diffuse layer) is dynamically advected. It has a rigorous definition and recent papers such as [Revil A. and H. Mahardika, Coupled hydromechanical and electromagnetic disturbances in unsaturated clayey materials, Water Resources Research, 49, no. 2, 744-766, doi:10.1002/wrcr.20092, 2013.] have received some awards (best paper award in Water Resources Research). It seems that the authors try to manipulate the opinion of the readers here, which is an unethical behavior.

11. The relationship between coupling coefficient and pore water conductivity is well established only through the Helmholtz Smoluchowski neglected surface conductivity and for a narrow pH range. This equation was introduced in the literature much earlier than the cited reference for instance by Revil, A., V. Naudet, J. Nouzaret, et M. Pessel, Principles of electrography applied to self-potential electrokinetic sources and hydrogeological applications, Water Resources Research, 39(5), 1114, doi: 10.1029/2001WR000916, 2003.

12. Below Eq. 25, the authors state that the surface conductivity estimation requires measurements at different salinities. This is not correct. As discussed in various papers (see discussion in Revil, A., A. Binley, L. Mejus, and P. Kessouri, Predicting permeability from the characteristic relaxation time and intrinsic formation factor of complex conductivity spectra, Water Resour. Res., 51, doi:10.1002/2015WR017074, 2015.), surface conductivity can be accurately be determined from a single measurements of the quadrature conductivity.

13. Just above Eq. 26: this is not correct since the formation factor cannot be known usually independently form the knowledge of the surface conductivity. This is a classical case of circular reasoning.

14. The effect of temperature has been first modelled by Revil et al. (1999, JGR-SE)

which is not cited.

15. Another point that is oversell is the manuscript is the potential determination of the permeability from the characteristic frequency separating the viscous laminar regime from the inertial one. In field conditions, this frequency is unreachable (> 5 kHz) because the Earth is a low-pass filter. In other words, frequency above 1 kHz are not propagated very far.

16. Very often the review is actually a boring catalogue of citations with no criticisms or pertinent remarks dealing with the advantages and true limitations of the method. This is very dangerous for the readers with no experience with the seismoelectric conversions.

17. The expression "The results we resumed in this review" does not seem right.

18. The way Figure 1 is obtained remains mysterious even after several ready of the paper by Thompson. This is not published in a peer-reviewed paper nd therefore such a result should be c considered with care.

19. Figure 3 was published much before the paper of Jouniaux and Ishido 2012. Again an unethical behavior.

20. Figure 6: is this a seismoelectric or an electroseismic coupling? I am confused here since the terminology used by the authors seems not consistent.

21. Figure 10: It is again unethical to avoid providing the names of the original contributors to this figure and datasets.

22. Figure 20 is a typical example showing that the theory of Pride is not entirely adequate because not based on the Stern layer effect in terms of frequency dependence of the conductivity. Rather than to explain this important point, the authors just tries to convey exactly the opposite statement. This is again unethical.

23. Figure 25 should be strongly improved.

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