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Spectral induced polarization measurements for environmental purposes and predicting the hydraulic conductivity in sandy aquifers
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Comments

The paper compares different models of predicting hydraulic conductivity of a sandy aquifer. The prediction is based on data of spectral induced polarization. The topic is of high relevance for hydrogeophysics. But the reliability of the data, the presentation of the state of the art and the derived conclusions should be discussed properly. The manuscript needs a careful and moderate revision to get a publishable paper.
A variety of helpful suggestions is given by André Revil in his discussion. I support most of his remarks and add some more hints that should help to improve the quality of the paper:

1) Permeability prediction from IP data has got a considerable tradition (e.g. Börner et al., 1996). It is not only recently (see page 5318, line 13) that a relation between K and Spor has been postulated as can be seen in Pape et al. (1987).
2) It is not justified to state in the introduction (page 5318, lines 20ff) that a direct link between imaginary conductivity and an "effective hydraulic length scale" is missing. I would accept this statement only after a careful discussion in the conclusions.
3) The statement on page 5321 line 12 is not correct. Neither Börner et al. (1996) nor the other papers state that there exists a power law dependence between **real part of resistivity** and S_{por}.
4) Börner at al. (1996) state in their equation 17 a proportionality. It is a misinterpretation to assume $a = 1$. You find the exact formulation for permeability in their equation 15.
5) There is a contradiction between equation 11 and the describing text. The equation does not include the imaginary conductivity but the DC resistivity. This equation is quite similar to the so-called "new approach" in equation 12 that considers both resistivity and relaxation time.
6) I cannot see how the sample preparation for laboratory SIP experiment guarantees in situ compaction, water conductivity, and water saturation. We know from own experiments that slight changes in compaction of the same material causes significant changes in the resulting complex conductivity spectra. This is one reason that makes a comparison between lab and field data complicated.
7) Please discuss the accuracy of K determination by grain size analysis. All these formulas do not consider the state of compaction.
8) Changes in water conductivity and water chemistry also cause changes in the IP spectra (see Weller et al., 2011; Weller and Slater, 2012). The used equations are based on water conductivity of about 100 mS/m.
9) Please indicate the errors in your phase data. We know that sands are characterized by low phases (see your figure 11 with a phase of ~ 1.5 mrad at 1 Hz). Slight errors in the phase measurement cause considerable changes in the resulting hydraulic conductivity.
10) The discrepancy between the 1D and 2D inversion results should be discussed in more detail. I assume that the 1D inversion is done considering the borehole information. It would have been better to consider the 1D result as starting model for the 2D inversion (e.g. Olayinka and Weller, 1997).

11) Please indicate how tau_max and tau_lw have been determined? I guess that tau_lw corresponds to the mean relaxation time as proposed by Nordsiek and Weller (2008) in their equation 8 with logarithmic weighting.
12) The real part of resistivity does not indicate grain size distribution as stated at page 5331, line 19f.
13) Regarding the remark 15 of A. Revil, it can be stated that even buried dead wood exhibits measureble polarization effects (see Weller et al. 2006).
14) I cannot get the message of the statement at page 5334, line16f. We have shown that there exists a reliable correlation between S_{por} and imaginary conductivity.
15) Looking at Figure 8, I recognize a variation in real part of resistivity between 200 and 500 Ohm*m (variation of less than a factor of 2.5!), in imaginary part of resistivity between 5*10^-6 and 3*10^-5 Ohm*m (variation of less than a factor 6), and in hydraulic conductivity between $2*10^{-4}$ and $8*10^{-4}$ m/s (variation of less than a factor 4). This limited range of variation, which is close to the accuracy of the measured values, does not enable reliable correlations. This is the main weak point of the manuscript. Reliable correlations require a wide variation of the investigated parameters (especially of hydraulic conductivity). I fear that a variation of the hydraulic conductivity between 10^{-4} and 10^{-3} m/s is hardly resolvable by both hydraulic experiments and SIP.
A variation of K over two decades as stated at page 5334, line 20 is not supported by the presented data.
16) The last statement in the discussion chapter is not correct. Weller et al. (2010a) recommended the mean relaxation time that seems to be equivalent to the so-called "logarithmic weighted average relaxation time". Zisser et al. (2010b) recommended the median relaxation time.

Minor errors:
P. 5325, line 2: topics -> optic
P. 5332: reference to equation 13 should be replaced by a reference to equation 12
Several referenced papers are missing in the list of references:
P. 5321: Slater et al. (2006)
P. 5321: Weller and Börner (1996)
P. 5336: Börner et al. (2002)
Fig. 12: labels at vertical axis are missing

Further references:
Olayinka, A.I.; Weller, A.: The inversion of geoelectrical data for hydrogeological applications in crystalline basement areas of Nigeria. Journal of Applied Geophysics 37 (1997), 103-115.
Weller, A.; Slater, L.: Salinity dependence of complex conductivity of unconsolidated and consolidated materials: Comparisons with electrical double layer models. Geophysics 77 (2012), No. 5, D185-D198.
Weller, A.; Breede, K.; Slater, L.; Nordsiek, S.: Effect of changing water salinity on complex conductivity spectra. Geophysics 76 (2011), No. 6, F315-F327.
Weller, A.; Bauerochse, A.; Nordsiek, S.: Spectral induced polarisation – a geophysical method for archaeological prospection in peatlands. Journal of Wetland Archaeology 6 (2006), 105 – 125.