Reply to Reviewer #1

We thank Rev#1 for his valuable comments which allow us to improve our paper clarifying some issues, even though we partially disagree with him. We will describe our points of view in detail below. In the following, the original review is quoted in *italics*.
His main remarks concern:

1. The reliability of the TDR-based bulk electrical conductivity measurements
2. The explanation of the filtering procedure we used to make TDR and EMI measurements comparable.

1. The reliability of the TDR-based bulk electrical conductivity measurements

I have found the paper interesting and potentially worth publication. However I find it somewhat surprising that the authors seem to believe that TDR is a better method than EMI to measure electrical conductivity. This seems to be an assumption made a priori, and not supported either by the scientific literature nor by any evidence in the paper. EMI is designed specifically to measure electrical conductivity, while TDR is designed with the measurement of dielectric properties in mind. Using TDR also to measure electrical conductivity can be done, similarly to using attenuation in GPR measurements to do the same. However it is not a recommended approach. My suggestion to the author is to reverse the line of reasoning, believe more in EMI (with some caveats especially concerning the depth of investigation) and rather question TDR as a method for sigma measurement. In a nutshell, give more credibility to geophysics and question some belief in soil science. To this end, I also suggest that an eye is given to ERT as a technique that can provide ground truth much more reliable that TDR for electrical conductivity (see e.g. Cassiani et al., 2012 and Ursino et al., 2014, but many other papers deal with the EMI-ERT obvious relationship).

The reviewer states that "*EMI is designed specifically to measure electrical conductivity, while TDR is designed with the measurement of dielectric properties in mind*". This is not completely true. EMI actually measures the real (or the in-phase) and the imaginary (or the quadrature) parts of the ratio of the secondary to the primary magnetic field. Under certain constraints the imaginary part of this ratio, multiplied by an instrumental constant, gives the apparent electrical conductivity (McNeill, 1980), which, however, is not the electrical conductivity. In fact, EMI data must be inverted (and in some cases calibrated too) to get the electrical conductivity.
We agree with the reviewer that TDR is designed to measure the dielectric properties of soils. To be more specific, however, it should be said that TDR actually measures the apparent permittivity, which is defined as

$$\kappa_a = \frac{\mu_r \kappa'}{2}\left(1+\sqrt{1+\left(\frac{\kappa''}{\kappa'}\right)^2}\right)$$

where μ_r is the relative magnetic permeability while κ' and κ'' are the real and the imaginary parts of the complex permittivity, respectively. The real part κ' (also known as the dielectric constant) accounts for the energy stored in the dielectrics; the imaginary part κ'', which accounts for the energy dissipation, is defined as

$$\kappa'' = \varepsilon_{relax} + \frac{\sigma_{dc}}{\omega\varepsilon_0} = \frac{\sigma_e}{\omega\varepsilon_0}$$

where ε_{relax} represents the loss associated with molecular relaxation , σ_{dc} is the electrical conductivity at zero frequency, ω the angular frequency, ε_0 the permittivity of the free space, and $\sigma_e = \kappa'' \cdot \omega\varepsilon_0$ is the effective conductivity, which represent the TDR-measured electrical conductivity of the material (Topp et al. 2000). Therefore, measuring κ_a TDR allows to get simultaneously the dielectric constant and the effective electrical conductivity. There is plenty of literature showing how this is possible (Dalton et al. 1984; Topp et al. 1988; Weerts et al. 2001; Noborio 2001; Jones et al. 2002; Robinson et al. 2003; Lin et al. 2007; Thomsen et al. 2007; Huisman et al. 2008; Lin et al. 2008; Koestel et al. 2008; Bechtold et al. 2010; and many others). In summary, neither EMI nor TDR directly measure the electrical conductivity but both of them allow to retrieve the electrical conductivity by the apparent conductivity using inversion and/or calibration.

When the reviewer says that the use of TDR to measure the electrical conductivity is not a recommended approach he probably means that TDR doesn't allow sufficient accuracy and/or he questions how the dielectric constant and the electrical conductivity are correlated, being estimated from one measurement (the apparent permittivity). In such cases, we understand the reviewer's concerns. However, to this point too there is a lot of literature that partially disagrees with him. Huisman et al. (2008), Lin et al. (2008), Koestel et al. (2008), and Bechtold et al. (2010) are just some examples. When the bulk electrical conductivity is in the range 0.02 – 2 dS/m (which is the range of bulk electrical conductivities we measured in our case) and when TDR is properly used (for example, good installation of the probes minimizing the effect of nonparallel wires; minimization of the soil disturbance), it allows electrical conductivity measurements with errors less than 5% (Huisman et al. 2008; Bechtold et al. 2010). In such cases, TDR electrical conductivity measurements reach accuracies so high to make TDR a potential tool to rate the validity of the electrical conductivity data obtainable from EMI data inversion. Moreover, with such high accuracy TDR can act as a quantitative ground truth for ERT too, as Koestel et al. state in their conclusion: "In addition, the results suggest that TDR has high potential to act as a quantitative ground truth for ERT. Furthermore, it may be possible to use TDR data to constrain the ERT inversion process."

Given the above, we feel that the use of the TDR to calibrate EMI data is totally defendable and it is actually a technique that can provide ground truth as reliable as ERT for electrical conductivity.

Anyway, since the intent of our paper is neither setting up TDR against ERT nor proving whether TDR is more reliable than ERT or not, we will revise the manuscript with the only aim of presenting the TDR as a viable and reliable calibration tool for EMI data.

2. The explanation of the filtering procedure we used to make TDR and EMI measurements comparable.

Spending time describing Fourier transformation is probably useless. Rather, I would concentrate on describing in detail what type of filtering is applied. "Fourier filtering" is unclear. I presume it is a spatial filtering made to enhance the long wavelengths? Please be more specific and try and link the approach to established (there are far too many) filtering techniques.

Totally agreeing with the reviewer about this point, we will update the manuscript, removing all unnecessary details from the long description of the Fourier transformation and describing the filtering procedure in more detail. Since the filtering is a key point in our calibration procedure, we anticipate here a brief schematic description to explain how we filtered the TDR series to make them comparable with the EMI series.

1) For each of the four transects, we have preliminary retrieved by EMI (sharp) inversion three series (horizontal profiles) of EMI bulk conductivity ($\sigma_{,EMI}$), one for each depth interval for which TDR series are available (0-20 cm, 20-40 cm, and 40-60 cm). To this end, for each EMI sounding we have averaged the conductivities in each of the depth intervals. Then, we have estimated the mean and the standard deviation for the EMI conductivity along these horizontal profiles.

2) Firstly, a zero-padded version of the measured TDR data have been converted from the spatial domain to the wavenumber domain (Fourier spectrum) using the Fast Fourier Transform (FFT). Then, these spectra have been multiplied by the wavenumber response function of low-pass filters. Finally, the spectrum of each output has been converted back from the wavenumber domain to the spatial domain using the Inverse FFT to get the filtered TDR data.

3) Original TDR profiles have been repeatedly filtered using low-pass filters with different low-cut wavenumbers, from the Nyquist to 0 cycles/m.

4) By comparing the standard deviation of the filtered TDR profiles to the standard deviation of the corresponding EMI profiles, we were able to select the optimum filtered TDR profile to calibrate EMI data.

Just as an example, Figure 1 shows the result of the described procedure for the 50-6dS transect at 20-40 cm. In this case, the cut-off frequency to make the two standard deviations similar corresponds to 0.313 cycles/m. In other words, this means that the two series are characterized by similar patterns of variability at 3.2 m distance.

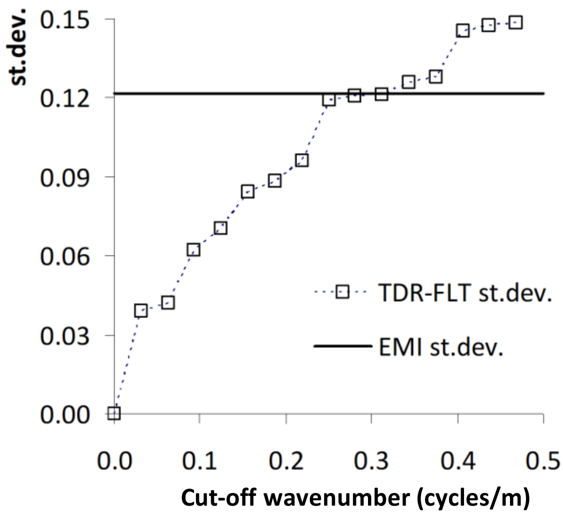

Figure 1

Other remarks

Line 26: "contributing to enhance the spatial resolution of the EMI reconstruction". I am not sure one can claim that the use of a stabilizer (how much needed would also require a specific discussion) truly *enhances spatial resolution of a geophysical method. In my opinion this statement is wrong. I suggest a reformulation here.*

The statements about the "spatial resolution" will be reformulated in the revised manuscript.

Line 125: "Then we assess the quality of these reconstructions by using TDR data as ground-truth." This is a very brave statement. I do not see TDR as any more reliable to measure sigma than EMI, indeed quite the opposite.
Line 132: "Accordingly, the paper provides a methodology to calibrate EMI results by TDR readings." This should not (cannot) be the focus of this paper. If the authors believe this is a viable strategy, I totally disagree.
Figure 6: the difference between TDR and EMI measured sigma is quite large indeed. Overall I am not sure that TDR is the best method to measure sigma. Indeed it is not. TDR is the chief approach to measure dielectric properties.

We fully discussed these issues above.

Line 727 Figure 2. "Examples of sharp and smooth inversions applied to the same dataset 100-6dS. The results are shown together with their corresponding data misfit". I see only one curve of data misfit. Does it refer to both sharp and smooth inversion?

Indeed, Figure 2 shows both sharp and smooth inversion data misfit curves, although they are barely discernible due to the poor quality of the figure. In the revised paper the quality of the figure will be improved in order to make curves clearly discernible.

Figure 8: the difference between the two images is striking. I am not sure how the authors are so confident that the correction applied to obtain the revised EMI image is correct.

Figure 8 was wrong. We will update it in the revised manuscript.

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