Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-93-SC1, 2016

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Repeated electromagnetic induction measurements for mapping soil moisture at the field scale: validation with data from a wireless soil moisture monitoring network" *by* E. Martini et al.

B. Hjaldrgegnir

Hjaldrgegnir@sol.dk

Received and published: 28 April 2016

Review: Repeated electromagnetic induction measurements for mapping soil moisture at the field scale: validation with data from a wireless soil moisture monitoring network

General remarks

The study deals with time lapse electromagnetic induction (EMI) measurements for mapping soil moisture at the field scale and aims to investigate the potential of repeated EMI measurements to map soil water content (θ) with particular focus on the temporal variability of the spatial patterns of 20 apparent electric conductivity (ECa)

and θ. Although the provided data amount seem adequate for investigation of ECa - θ relationship from the first view, a more detailed review revealed profound knowledge lacks and missing requirements to judge a publication of the presented results in a scientific journal.

Prominent weakness is the absence of calibrated ECa values which are fundamental for any further proceedings. It is common knowledge in the community that EMI might be affected by several undesired outside influences (e.g. operator, weather conditions) and thus ECa values demands an instrument- and site-specific calibration procedure, e.g. by using inverted electrical resistivity tomography (ERT) data and ECa forward modeling as successful applied by several studies (Lavoué et al., 2010, Mester et al., 2011, Minsley et al. 2012, von Hebel et al., 2014). The authors mention this fact in page 8 around line 17; however instead of applying an established calibration procedure to provide absolute ECa, needed e.g. for comparing the temporal ECa - θ trends, they continue the investigation with 'the analysis of differences in spatial patterns of ECa on the individual measurement dates'. As long as the used ECa only represents 'relative' values potentially affected by outside condition, a serious comparing with spatial and temporal theta variability is unfeasible. Hence every following results and discussion are challengeable and missed a certain amount of reliability. In this context it appears questionable to interpret the presented ECa variation by variation in soil water conductivity (ECw) – even an explanation attempt by theta is unwarranted in this case.

Next outstanding weakness is the missing ECw data. Since no ECw data were presented by the authors conclusion about ECw as main ECa trigger are indefensible - the mentioned reference for ECw (Pütz, et al, 2011 page 6 line 5) is not an ISI listed publication nor available in the internet which limits the stated value (same for Reinstorf, 2010a and b). It seems that the authors refer to one single lysometer which would provide in any case no sufficient data for the whole test field and the aimed purpose (see comments below). In addition to that, it is peculiar from the authors to introduce the variable ECw without any further reference to Archie (1942) which indicates a less

profound knowledge of established ECa – θ literature. Concerning these facts, the proposed conclusion: 'the electrical conductivity of the soil solution rather than θ is likely to be the dynamic factor controlling temporal variations of ECa' seems picked up somewhere rather than it based on true consideration or measured data.

Regarding the inadequate ECa data processing (ECa calibration is state of the art and the least what the authors could have done in 2016 if aiming a qualitative ECa interpretation), the lack of proper ECw data and the avoiding of established ECa – θ Archie equation (mandatory for dealing with ECw), I am afraid the scientific value of the study in its current form is void. The manuscript contains further numerous inaccuracies and mistakes which extra limit its quality and certified a quite inattentive conduction (see specific remarks). Format of the reference list is not consistent.

Specific remarks

Page 1 line 23 (following mentions as 1/23) 'θ varied from very dry to almost saturation' – this assumption based on measured porosity data of four single points from Martini et al, (2015) and does not justified statements for the whole test site.

1/24 'stable soil properties are the major control on ECa measured with EMI' this is just a further assumption and not proofed by any data presented.

2/13 'The theory and basic principle of EMI-based measurements of ECa refers to the mechanistic soil ECa model proposed by Rhoades et al. (1989), which is based on the soil equivalent resistance model (Sauer et al., 1955).' This is not entirely correct, EMI technique based on induced eddy currents (Keller and Frischknecht, 1966) and not on the soil equivalent resistance model stated by Sauer et al., (1955).

2/20 'To a higher clay content and/or higher organic matter content usually correspond a higher content of adsorbed water (i.e., higher θ)' this statement is also is not correct since both variables can occurred total independent from soil water content. Further, there is absolutely no evidence that soil organic carbon influenced the bulk electric

conductivity. Nevertheless I don't doubt that a pseudo- correlation might occur. If the authors like to refer to such a case study, a convincing amount of references is needed instead of an unsubstantiated generalization.

3/27 the first two mentioned references Martinez et al. (2009) and Werban et al. (2009) do not fit here, again soil organic carbon (SOC) does not affect directly ECa. Both studies don't show a positive relation between SOC and ECa. The study of Martinez et al. (2009) explicit demonstrated that ECa and SOC are not directly related since plotting ECa vs. SOC resulted in a point cloud. A more precise literature review could be expected here.

3/28 what means soil quality?

4/6 'For most of the studies discussed in Calamita et al. (2015), ECa was measured with EM38 (Geonics Ltd., Canada), which is in fact the most widely used EMI sensor.' Whole sentence is not needed unless you run a Geonics endorsement (I guess the authors mentioned this in order to justify the use of the outdated EM38 device. . .), omit it.

5/10 what means 'Findings of the studies summarized above show clearly the need for a more in-depth examination of the ECa-θ relationship for soils under field conditions'? Introduction is too long and too fuzzy. What have other studies achieved and where did they struggle? Explain the special need of your study and its contribution to the scientific community!

6/6 'Data from TERENO SOILCan lysimeters (Pütz et al., 2011) located within the southexposed slope of the study area indicate ECw < 0.10 mS/cm throughout the monitoring period considered for the present work.' Does it mean one lysimeter reads approximately the same value over the whole time period? Single ECw information for the whole test site and it produced stable ECw values, does I understand it right? Regardless of the insufficient spatial and temporal allocation, if ECw remains stable you conclusion is inconsistent and indicated internal errors.

8/17 as mentioned above just 'nulling' is not enough for assure the ECa data quality in 2016!

9/9 how did you calculated the integral values?

10/1 – 3 which variation do you mean, temporal, spatial? Needs a small additional phrase.

10/22 'for the Schäfertal hillslope site, soil moisture has little influence on the measured ECa.' This is an adventurous statement based on nothing but an assumption (or on ECa calibration errors), needs verification!

11 – item 3.3. I would recommend using the integral theta values only in order to avoid confusion. It further guarantees better comparability to the integral ECa. However, why does the correlation in April fits quite good if you later stated that snowmelt and corresponding low ECw govern the relation? Inconsistent.

16/14 'Nevertheless, such procedure is based on the sensitivity function proposed by McNeill (1980), of which the limits of applicability were discussed already' this is not correct, the study of von Hebel et al., (2014) showed impressively the ability of ECa inversion to derive distinct layers with certain electric properties using the exact EMI-FM model (full frequency based Maxwell) and the L1-norm in the Shuffled Complex Evolution (SCE) algorithm without regularization to assure sharp layer boundaries (von Hebel et al.,2014). Thus, the cited study doesn't fit here. Since the authors are obviously not comfortable with multi-coil EMI inversion I wouldn't raise this item at all.

References:

Archie, G.E. 1942. The electrical resistivity log as an aid in determining some reservoir characteristics. Transactions of the American Institute of Mining and Metallurgical Engineers, 146, 54-61.

Keller, G.V. & Frischknecht F.C. 1966. Electrical methods of geophysical prospecting, International Series of Monographs in Electromagnetic Waves, Pergamon Press, Ox-

ford, New York, 10.

Lavoué, F., van der Kruk J., Rings J., Andre F., Moghadas D., Huisman J.A., Lambot S., Weihermüller L., van der Borght J., & Vereecken H.. 2010. Electromagnetic induction Calibration using apparent electrical conductivity modelling based on electrical resistivity tomography. Near Surface Geophysics. 8, 553–561.

Mester, A., van der Kruk J., Zimmermann E., & Vereecken H. 2011. Quantitative two-layer conductivity inversion of multi-configuration electromagnetic induction measurements, Vadose Zone Journal, 10(4), 1319-1330.

Minsley, B. J., Smith B.D., Hammack R., Sams J.I. & Veloski G. 2012. Calibration and filtering 621 strategies for frequency domain electromagnetic data, Journal of Applied Geophysics, 80, 56-66.

Von Hebel C., Rudolf S., Mester A., Huisman J.A., Kumbhar P., Vereecken H. & Van der Kruk J. 2014. Three-dimensional imaging of subsurface structural patterns using quantitative large-scale multi-configuration electromagnetic induction data. Water Resources Research. doi:10.1002/2013WR014864

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-93, 2016.