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Interactive comment on "Investigating temporal field sampling strategies for site-specific calibration of three soil moisture – neutron intensity parameterisation methods" *by* J. Iwema et al.

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We thank the reviewer for her/his time and her/his positive feedback on the manuscript. We address all comments in detail below:

The manuscript describes an effort to assess three different calibration methodologies for cosmic-ray sensing of soil moisture. This is a worthy goal. But the authors have only attained it in part - in the conclusion that more calibrations data sets are better than fewer and that some sites require more calibration data

sets than others. These seem trivial conclusions, particularly in connection with statements of the type "we don't know why this works better than that". This makes me uncertain how useful this research is. However, giving the authors the benefit of the doubt, I think the paper may be made publishable, with some revisions as described below.

[1] Question about originality: how is this paper different from Baatz 2014? It seems that only in using more sites, but the idea is exactly the same, so the originality is questionable. Are non-original papers acceptable by HESS?

Response: We thank the reviewer for his comment. We would like to emphasize that the objective of this paper is to evaluate the impact of multiple calibration days, and not only "to assess three different calibration methodologies". We believe such objective is novel in the field of cosmic ray soil moisture sensing and inherently different from the approach used in Baatz et al. (2014), whose objective was to "assess the accuracy of soil water content determination from neutron flux measured by cosmic-ray probes under humid climate conditions" (Baatz et al., 2014, p231), and also employed a single-day calibration strategy at each site. Up to now, such single-day strategy had been assumed to be sufficient for CRNS calibration, as discussed in Zreda et al. (2012), and cited below:
"Thus, a single representative measurement of average soil moisture content in the footprint is sufficient for calibration (although measuring area-average soil moisture does involve collecting numerous soil samples within the footprint and measuring their soil moisture gravimetrically by oven-drying; see Sect. 2.5.3). Measured neutron intensity is then compared with the average soil moisture, and the calibration parameter N0 in Eq. (4) is calculated." (Zreda et al. 2012, p4086)

This was in fact one of the main motivations for our study. Please notice that our aim was not necessarily to show that "more calibrations data sets are better than fewer and that some sites require more calibration data sets than others" but instead to evaluate what could be the further improvement achieved (in terms of information gain)

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when a new calibration point with different characteristics is introduced. This is perhaps more clear when we analysed the preferred wetness conditions (Section 3.3 in original manuscript) and also shown as Cumulative Distribution Functions in Figures 9-12. Please, also notice that our work could have easily been carried out with a single chosen parameterisation (and arguably at a single site) but we have decided to include the three proposed parameterisations given their widespread application by the CRNS community. We evaluated their performances at three distinct sites given their specific characteristics. Therefore, we believe that our work is original and complements previous studies on the application of CRNS (as also highlighted by the positive comments received from other three reviewers).

[2] Comment about the value of the research: on sites with distributed point sensors net-work, where we have multiple sensors I see no reason for cosmic-ray probe because all the information is already gained from the network. Likewise, where we don't have those numerous point sensors, the proposed calibrations cannot be done. So what is the value of this research? If it is the recommendation of the number of calibration data sets needed for different sites and conditions, this is a valuable contribution. Please, make it crystal clear that you don't recommend distributed sensors network as a pre-requisite for the calibration of the cosmic-ray probe. If you do, then this is a useless contribution.

Response: We would like to clarify this issue raised by the reviewer. Typically, CRNS are calibrated with in situ gravimetric/volumetric soil samples which give a representative average soil moisture content for approximately the same footprint obtained by CRNS. We used sites with distributed sensor networks because these could serve as proxy for soil sampling campaigns, as mentioned in the original manuscript (p2353, lines 17-19) and reproduced here:

"As a proxy for soil moisture samples, we used data from in-situ soil moisture sensor networks, because no year long time series with daily soil moisture samples is usually available."

To better clarify our objective, we would like to propose to change the above sentence to the following:

"As a proxy for soil moisture samples, we used data from in-situ soil moisture sensor networks, but it is important to emphasize that distributed sensor networks are not necessarily needed to be co-located with the CRNS for operational purposes, including calibration. Such evaluation has been adopted because no year long time series with daily samples is usually available from soil samples. The combined use of CRNS and sensor networks has been essential for understanding and improving this technology (Franz et al, 2013, Rosolem et al., 2014, Bogena et al, 2013, and Baatz et al. 2014)."

We would also like to emphasize that CRNS and distributed sensor networks do not necessarily measure the exact same state. For instance, the effective measurement depth of the CRNS varies over time and it is affected by different hydrogen pools, whereas distributed sensor networks estimate soil water content only, usually with a fixed measurement depth associated with it. Furthermore, both sensors indirectly estimate soil moisture (notice that both of them have been calibrated against soil samples inside their respective footprint), hence sensor to sensor variability is also important to be recognized in this difference.

[3] Comment on non-uniqueness of solution: it seems that the introduction of fitting pa-rameters (for example b parameters in equation 2) will assure convergence. But how valid are these parameters, if we don't know whether they correspond to any parameters in cosmic-ray physics? I see a danger here in the ability to fit any data set, however good or bad, by simply adjusting (mutually compensating) these fitting parameters. I think you can arrive on a number of calibration parameter sets (in equation 2) that will provide the correct solution, but the best may be impossible to find. The correct way to calibrate the system must rely on physics, not mathematical manipulations. In this respect my opinion is that this research is questionable and should not be published.

Response: We thank the reviewer for this comment. With respect to the N0 method pointed out by the reviewer, please notice that almost all measurement devices have calibrated curves (e.g., gauging stations, radars, Time-Domain-Reflectometry soil moisture sensors) and adjusting their parameter values based on observations is general and advisable practice in environmental sciences (Robinson et al., 2008). Therefore, we believe that our approach (i.e., selecting parameter sets that give the best fit to the data) is no different from those. The coefficients from all the three parameterisations discussed in this study have been originally obtained via comparison with a neutron particle transport model (as explained in Desilets et al. 2010; Franz et al. 2013; Shuttleworth et al. 2013). Although we would always aim to have coefficients with physical meaning, a degree of empiricism is sometimes inevitable and not all coefficients have physical meaning and can be easily measured in the field (Wagener and Gupta, 2005). For example, modified parameters in the N0 method (a0, a1, and a2 in Desilets et al., 2010) for better specific applications have been reported previously (Villareyes et al., 2011). In fact, even one of the reviewers has recommended us to update HMF empirical coefficients based on new findings (McJannet et al., 2014).

The HMF method (Franz et al., 2012) on the other hand deals with the various source of hydrogen pools (notice not all pools are explicitly included in the N0 method) by employing a relation between all hydrogen pools within the CRNS footprint and neutron count. The HMF coefficients were obtained by calibrating against MCNPX (Pelowitz 2005)) and the site-specific parameter Ns (which represents the local count rate over infinite pure water body) was calibrated against soil moisture samples for each site. The obtained values in our analyses appear to be plausible and similar to other studies.

COSMIC is a physically-based parameterisation in which parameters have physical meaning (see Shuttleworth et al. 2013). These coefficients were developed by calibrating against MCNPX assuming 22 hypothetical soil moisture profiles. Baatz et al. (2014), in contrast, calibrated only one parameter (N) against only a single calibration day (i.e., profile). Notice that in our current study, two COSMIC parameters were calibrated (N and alpha) because we believe that the correlation between alpha and soil bulk density (as shown in Shuttleworth et al. 2013; Figure 6) was not particularly strong.

In summary, I am split between recommending this for publication and for rejection, leaning slightly toward publication. But please, think about the issues described herein.

[4] Specific small comments:
p. 2351, l. 9, Desilets 2001 is about high-E neutrons - remove.
p. 2351, l. 13-14, remove Kodama reference - irrelevant because his sensors were buried.

Response: Thank you for these comments, we removed both references from the manuscript.

p. 2351, l. 15, clarify that 600 m footprint is for dry soil and dry air. The footprint shrinks with added water in either reservoir.

Response: Thank you for this comment, we changed the sentence as follows:
"The sensor footprint has a horizontal diameter of about 600m at sea level for dry air but changes slightly with elevation and moisture content in the atmosphere (Desilets and Zreda 2013)"

p. 2354, l. 6-7, don't capitalize summer and winter. (Also correct all other occurrences).

Response: Thanks. We will correct this in the manuscript.

p. 2355, l. 19-21, daily averages from distributed sensors and from COSMOS probe may not be comparable. If not, this will be an unacknowledged source of error.
Section 3.1: when discussing under- and overestimation in neutron count, include not only the number of counts but also what percentage they are. (A state-

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ment "under-estimate by 80 counts" is meaningless without knowing the baseline.) This comment applies to other places throughout the manuscript. Alternatively, express the counts normalized to a reference value, or to re-normalize them, for each site separately, to the scale 0-1, like we do with saturation in soils. I am not sure if this would be beneficial, but at least the values would be easy to assess.

Response: We thank the reviewer for this important comment. We propose to normalise Mean Absolute Errors by dividing by the Mean Absolute Error of the associated Reference Solutions while discussing the results.

Citations

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