Final answer to review comments on manuscript originally entitled "Investigating temporal field sampling strategies for site-specific calibration of three soil moisture – neutron intensity parameterisation methods" by Iwema et al. submitted to Hydrology and Earth System Sciences.

We thank the Editor for his work and guidance.
We address the comments of each referee below. Besides, we have improved wording, tables and figures throughout the manuscript and have corrected minor (textual) errors. All textual changes have been highlighted in blue in the new version of the manuscript attached.

Reviewer 1
We thank the reviewer (R. Schwartz) for his time and his positive feedback on the manuscript. We address all comments in detail below:

The authors have done a good job in collectively evaluating and discussing CRNS calibration results from three different sites. The results of the study provide some insight into the temporal soil moisture data required for calibration and a suitable model. I have a few comments and suggestions for improvement.

[1] When discussing results in the abstract, results and conclusion sections, it would be helpful for readers to provide some indication of the approximate error in the predicted weighted soil water contents (or range in weighted soil contents) from the specific model using CRNS neutron intensity counts. This will avoid ambiguous statements (e.g. "COSMIC performed relatively good at all three sites"; "sampling more than ten days would … improve the calibration results only a little"). Without these numerical values, it is impossible to evaluate and compare calibration strategies.

Response: We thank the reviewer for this important comment. We understand the importance of providing error estimates related to weighted soil moisture. We have included a discussion on soil moisture content errors in Section 3.2. We have removed our original threshold analysis, in which we used mean values calculated with the reference strategy and original solution MAE values. Instead, we now compare soil moisture content error estimates with typical errors of the TDT in-situ sensor and the SMOS satellite remote sensing product. We address these new results also in the Conclusions section and in the abstract.

[2] Methodology: Please identify the soil moisture sensors in each of the study sites. This is important with respect to interpretation of results (some sensors are inherently better than others are and less influenced by soil characteristics, even when calibrated).

Response: This is a good point made by the reviewer. We have included the information about the sensors in the new version of the manuscript:
"At SR 18 paired in-situ sensor profiles, with sensors (ACC-SEN-TDT, Acclima Inc., Meridian,ID,USA) at 5, 10, 20, 30, 50 and 70 cm depth, were installed with the spatial distribution as described by Franz et al. (2012), with all equal horizontal weights"

"The in-situ sensor network (SoilNet, Qu et al., 2013, 2014) consisted of 83 profiles with soil moisture sensors (SPADE soil water content probes, sceme.de GmbH i.G., Horn-Bad Meinberg, Germany; H{\"u}bner et al., 2009) installed at 5, 20, and 50 cm depth"

"150 profiles with in-situ soil moisture sensors (horizontally installed ECH2O sensors (EC-5 and 5TE, Decagon Devices Inc., Pullman, USA), SoilNet, Rosenbaum et al. 2012) at 5, 20, and 50 cm depth were installed"

"The same CRNS model (CRS-1000, Hydroinnova LLC, Albuquerque, NM, USA) was used at all sites."

[3] The observation that "specifically resolving individual soil layers with COSMIC compared with depth-weighted soil moisture using the other methods led to better overall performance of the calibration" is important and should be included in the abstract if possible.

Response: We thank the reviewer for emphasizing this point. We have added a sentence in the abstract as suggested.

[4] Do the authors believe that the differing density and/or spatial (vertical & horizontal) sampling of soil moisture measurements influenced the calibration performance? Should this be discussed?

Response: This is also an important point made by the reviewer and we thank him for that. We have added a sentence to the Conclusions section of the manuscript:
"It is important to notice that varying the density and/or spatial (vertical and horizontal) sampling of soil moisture measurements may influence the calibration performance. The analysis of the actual impact on performance are beyond the scope of this study which focuses on understanding the temporal sampling using typical spatial soil sampling approaches previously published in literature (Zreda et al., 2012; Desilets and Zreda, 2013; Bogena et al., 2013)."

Reviewer 2
We thank the Referee (T. Franz) for his time and his positive feedback on the manuscript. We address all comments in detail below:

The authors present at interesting review of the three common calibration methods used for the cosmic-ray neutron sensor. The authors estimate statistically the required number of calibration datasets for the three different methods at three different study sites, providing some recommendations. The paper is well written but does contain several locations where grammar needs to be improved. Following fairly minor re-visions (updating HMF coefficients, commenting on use for mobile applications), the paper would be suitable for publication and add to the growing CRNS literature.
[1] As the main author of the HMF method, I would suggest a couple comments. First, the 4 derived coefficients were updated in the McJannet et al. 2014 WRR paper table 4. The updated coefficients should be used as they are more reflective of the 1 inch moderated tubes than a pure epithermal response (0.5 to 1000 eV). A weighted average of ~30\% thermals (0-0.5 eV) and 70% epithermals (0.5 to 1000eV) should be used by MCNPx simulations to be more representative of the actual detector response. I don't think this well have a drastic affect on the overall results but may improve on any systematic bias that may exist with the HMF or COSMIC methods.

Response: We thank the reviewer for making this important point. We have updated the coefficients as suggested by the reviewer and we observed an improvement in the performance of the HMF method, especially at the two temperate sites (Rollesbroich and Wüstebach). Figures and equations have been modified accordingly in the revised version of the manuscript.

[2] The HMF function contains only 1 free parameter as compared to 2 for COSMIC operator (2 here but 4 free parameters elsewhere) and 3 for modified N0. It seems that the performance of HMF given the less number of calibrated parameters should be taken into account for any cross comparisons of the methods. Did the authors consider any penalty factors for increased number of free parameters to calculate? Could this be considered with an AIC metric? The authors do point out the fewer number of parameters in several locations but I was just wondering if a penalty function was used.

Response: We thank the reviewer for his comment. Our original analysis did not include a penalty factor related to number of parameters as model complexity (i.e. related to total number of parameters) was subjective assessed by the authors. We do recognize the importance of also taking into account the number of parameters calibrated when evaluating the model performance. Notice that the Akaike Information Criterion (AIC) was designed to find an optimum between number of parameters and model accuracy of linear black box models, and works best when comparing model structures of different complexity within the same class (for instance, linear regression as in the original works on AIC). In our case we deal with three nonlinear models with different structures and we hence argue the AIC might not be the most appropriate choice. As far as we know, no suitable alternative method, ready to use for the concerned type of models, is available. However, we agree the issue of model complexity is worth raising in the paper and therefore in the revised manuscript we have mentioned that:
"A model with fewer parameters but similar or slightly worse performance may be preferred to a more complex model." (Results and Discussion, Section 3.2, in manuscript at p18, lines 396-397).

[3] Given the excitement for mobile CRNS surveys (Chrisman et al. 2013 and Dong et al. 2014), the authors may comment on the most appropriate method for use with mobile surveys. Perhaps in a few sentences in the discussion is all. Clearly calibrating each site 2-10 times is not a good option for mobile surveys.

Response: This is an important point raised by the reviewer in regards to calibration of mobile sensors. We will therefore add a few sentences to Section 3.2, p12-13, lines 398-403 (reproduced below).
"For applications of mobile CRNS rovers (Chrisman et al., 2013; Dong et al., 2014), multiple calibration instances are more difficult to be realised. However, in regions where stationary CRNS are available, information from mobile surveys can be better translated/constrained by such sensors, and hence multiple-day calibration becomes even more important for stationary sensors. Alternatively, one may adopt a space-for-time approach such as those proposed for satellite remote sensing soil moisture applications (e.g., Reichle and Koster, 2004)."

[4] A few examples of grammatical errors and needed sentence changes. Authors should check rest of manuscript for consistency.

Response: We thank the reviewer for this comment and have corrected grammatical errors throughout the manuscript.

[5] Pg 2353 L18: "because year long daily time series of soil moisture is usually unavailable." Equation 3. Please update coefficients using McJannet 2014 WRR Table 4.

Response: We have changed this to (p 4, lines 94-96):
"As a proxy for soil moisture samples, we used data from in-situ soil moisture sensor networks, because continuous soil moisture sampling measurements over a full year are usually not available."
We have updated the coefficients of the HMF method using Mcjannet et al. (2014) Table 4.

[6] Pg 2366 L 22. :"understood by"

Response: We changed "*understand by"* to *"understood by"*, as proposed by the reviewer

Reviewer 3
We thank the reviewer (T. Caldwell) for his time and his positive feedback on the manuscript. We address all comments in detail below:

**[1] The authors of HESSd-12-2349-2015 provide guidance to suitable calibration methods for the Cosmic-Ray Neutron Sensor (aka, COSMOS). Varying neutron intensities unrelated to soil moisture (H-content in biomass, interstial clay, snow, etc.) adversely affect it. The authors assess 3 calibration methods and data requirements (from in situ sensors) over a range of hydro-climatologies and land cover. Since CRNS integrates over a varying depth range (p2351, L3) depending on moisture content, it doesn't appear to me that the sensors were depth-weighted by any means. How does assuming a constant depth-integration weight over the 5-, 20-, and 50-cm probe impact the variable depth integration of the CRNS? Beyond the equal horizontal weights (which may also introduce some error, p2352, l12), the depth weighting of the in-situ sensors needs more clarification. In particular, these sensors are a surrogate for actual soil sampling. The authors focus on number days very well, but don't specify how deep – since some (SR) profiles exceed 50-cm. Considering that you removed the soil organic matter and interstitial H from equation 1-2, or hydrogen content in HMF, do these data need collected (over 'x' depth) as well? Also, please add a little detail on Bogena et al. 2013 which is referenced for all the sensor depth averaging. Perhaps this is all in Bogena et al. 2013, but since mean spatial and profile water content are so important, a little more elaboration is required.
The temporal resolution and 'wetness' condition is well-defined and the manuscript is well-composed and very thorough.}**

Response: We thank the reviewer for his important comment. The method of Bogena et al. (2013, WRR), which we used to compute weighted average soil moisture contents as input for the modified N0 method and the HMF method does take into account depth-weighting (i.e., weight exponentially decreases with depth). To better clarify this, we have changed this sentence in Section 2.3.1, p6, lines 170-177:
"We calculated depth-weighted profile average soil moisture contents with the methods proposed by Bogena et al. (2013)."

to:

"We calculated depth-weighted profile average soil moisture contents with the methods proposed by Bogena et al. (2013) to consider exponentially decreasing weights (wz) with depth:

$$w_z = 1 - e^{\left(-\frac{z}{\gamma}\right)}$$ *Eq. (3)*

and

$$\gamma = \frac{-5.8}{ln(0.14)\cdot(H_p + 0.0829)}$$ *Eq. (4)*

where z represents the measurement depth in cm and H_p represents the total below ground hydrogen pool in the respective soil layer in g H2O per cm^3. For a more detailed description of the derivation and computational implementation we refer to Bogena et al. (2013)."

Soil organic matter and lattice hydrogen were not depth weighted, instead we used arithmetic mean lattice water and soil organic matter contents of the top soil, following the approach of Baatz et al (2014), and provided directly by the COSMOS network.
We added the following sentence to Results and Discussion, Section 3.1, p9, lines 285-286:
"Explicitly taking into consideration the depth varying SOM and lattice water content could potentially improve neutron count estimates."

[2] Line specific comments: P2351, L20: Awkward sentence: "However, fast neutron intensity is not solely dependent on soil moisture content"

Response: We thank the reviewer for notifying us of this sentence. We have changed the sentence as proposed.

[3] P2352, L3: what is \`it'?

Response: We thank the reviewer notifying this is an unclear sentence. We have changed the sentence as follows (p3, lines 57-60):
"Typically only a single parameter (N0) of the N0-method needs to be calibrated with a single point from average soil moisture representative of the CRNS footprint (Desilets et al.,2010 and Zreda et al., 2012). A similar approach is typically used for HMF although estimates of additional hydrogen pools are also needed (Franz et al., 2013)."

[4] P2355, L7: it's \`coarse material', not course.
[5] P2360, L26: \`until' not \`till'

Response: We thank the reviewer for notifying us of these two errors; we have modified the wording in the manuscript.

Anonymous reviewer 4
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) 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 (p 4, lines 94-99):

"As a proxy for soil moisture samples, we used data from in-situ soil moisture sensor networks, because continuous soil moisture sampling measurements over a full year are usually not available. It is however important to emphasize that distributed sensor networks are not necessarily needed to be co-located with the CRNS for operational purposes, including calibration. 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 use 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. 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).

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).

The N0 equation is a shape-defining function with limited physical meaning. The values were found by fitting ground-level neutron intensities simulated by the MCNPX model (Pelowitz 2005) using a generic silica soil matrix (Desilets et al. 2010). Transferring these coefficient values to real world conditions (e.g. differing mineralogy of soil particles, differences in the modelled and measured energy spectra, etc.) clearly involves uncertainties. Adjustment of these coefficient values (Desilets et al. 2010) is a way to consider these uncertainties and it has been reported previously (Villareyes et al., 2011).

The HMF method (Franz et al., 2012) on the other hand deals with the various sources 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. However, like for the N0 method, HMF empirical coefficient values giving a better fit with observed data have been found (McJannet et al., 2014). In fact, one of the reviewers has recommended us to update HMF empirical coefficients based on these new findings. We have followed this recommendation. 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 values obtained 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 effective area 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 have corrected 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 statement "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 have replaced absolute errors (cph) with relative and percent errors.
For example, at line 246 in the original manuscript:
"overestimated (by 20 to 160 cph)"
was changed to:
"overestimated (by 1 to 8%)"

Citations

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