

Interactive comment on “Calibration approaches of cosmic-ray neutron sensing for soil moisture measurement in cropped fields” by C. A. Rivera Villarreyes et al.

Anonymous Referee #2

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The presented study of Rivera Villarreyes et al. (2013) is focussing on a calibration approach of cosmic ray neutron sensing for two different crop types. They suggest three calibration schemes of the Desilets equation (Desilets et al., 2010). In a second step they present an improvement to estimate the penetration depth with four different schemes. One of the important take home messages is the influence of vegetation pattern on the cosmic ray signal.

The manuscript needs improvement in different directions. The story line is not clear. It is hard to understand the intention. The different chapters are not clearly organised. Information which has to be presented in the study site description is presented in the

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discussion. The material and methods block needs more continuity. They jump from the soil moisture estimation to the penetration depth continue with soil moisture and go back to the penetration depth. Results and discussion should be separated. The presentation of the observed vegetation patterns has to be extended. Only vegetation height is shown, variability is neither mentioned, nor any other measure like biomass, coverage or LAI. The study site needs more explanation. The soil is only described as a homogeneous sandy soil without any facts of variability in any soil physics except texture. They do not present that they are able to capture the true soil variability in a radius of 600 m around the probe with the installed soil moisture profiles. Only two field campaigns were conducted to prove and then only the top soil moisture was taken into account. The presented calibration procedure is only valuable for specified vegetation periods where the changes in the vegetation patterns are low. It involves a lot of parameter sets (for each vegetation period) and therefore increases in uncertainty. Compared to the procedure of Franz et al. (2013) which is not even taken into account the presented method does not look straight forward and transferable. Most of the graphics are unclear and not helpful. Some parts of the results chapter is more a discussion than the analysis of the calibration. Scientific English has to be improved.

The terminology of the three calibration approaches in chapter 2.3.2 is misleading as they call them fully empirical, semi-empirical and N0-calibration. They are all empirical. It would be better to call them three-parameter-calibration scheme, Deidri model with factor and a one parameter calibration scheme. In none of their procedures they take vegetation into account which should have an influence. Why do they not present a sensitivity analysis of the complete Deidri model instead of finding best fit of the presented three schemes? Chapter 2.3.3 is unclear in the current form. What do they want to present? The interaction of the calibration approaches and the penetration depth procedures are not well formulated and hard to follow in the text.

Specific comments:

P 4240 L 22: Add dominating geology, meteorological forcing as mean temperature,

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ET, precipitation, is there groundwater influence or not, what is the agricultural practice on the study site, is there an effect on soil properties.

P 4241 L 519-20: Add a citation.

P 4242, L 11-12: Which data was used from the FAO?

P 4243 L 12: Delete the C in front of the theta

P 4243 L 13: Comment here the importance of lattice water for the specific study site. Is the parameter important in the dominating geology? Franz et al. (2013) include as well organic content, comment also on that.

P 4243 L 22: How is atmospheric water vapour correction taken into account?

P 4244 L 5: Change variable to parameter

L 4245 L 4: Scenario is misleading, procedure would be better.

P 4246 L 14: The homogeneity of soils has to be presented with hard data.

P 4247-2448 L 9-18: Most of that block can be moved to study site description. Texture analysis does not alone describe the homogeneity of a soil. What is the structure of bulk density? Organic content which is twice mentioned in the results part (P 4250 L 15 and P 4253 L 19) should be discussed. What is the layering structure of the geology, water repellent effects in the glacial shaped landscape where the study site is located can have an influence on the soil moisture pattern, what is the dominating soil type, etc. The histogram should be presented. With two campaigns at the surface it is a hard to judge, whether they can represent the mean soil moisture in profiles.

P 4247 L 24-26: Unclear, please give additional information how that was conducted and how the structure of the soil was effected by the agricultural processing (ploughing etc.) and to what degree the structure is comparable to the current state. P 4248 L 15: Why didn't they use the specific density of the sample instead of the mean value? In the described methodology they lose information of each unique soil sample. Which

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grain density was used?

P 4249 L 2-28: Present how much variance in the observed data can be represented with the procedure. That paragraph can be shortened. Use a table.

P4250 2-3: Last sentence has to be deleted.

P 4254 L 18-20: Present or delete! What was the result of that modelling approach?

Table 1: Bulk densities are not in the equations, delete. What is the N0 value for the first two assumptions?

Table 2: Add information of variability and additional soil physics. The crop can not only be described by the average height.

Figure 5: Are the different approaches plotted or only one?

Figure 6: The initial state (of sunflowers or both vegetation types?) soil moisture shows a low correlation to neutrons. Comment on that in the text.

Figure 7: Add variance to the height. Figure 8: Why are biomass, root density and crop water content not measured? You should not "expect" you should "know". That information should be part of the analysis and not of a caption.

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