

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.

Anonymous Referee #4

Received and published: 8 April 2015

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.

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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?

Comment about the value of the research: on sites with distributed point sensors network, 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 prerequisite for the calibration of the cosmic-ray probe. If you do, then this is a useless contribution.

Comment on non-uniqueness of solution: it seems that the introduction of fitting parameters (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.

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.

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

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buried.

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.

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

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 "underestimate 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.

Recommendation: publish with modifications/improvements.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 2349, 2015.

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