Author Response to Referee #1

Technical Note: revisiting the general calibration of cosmic-ray neutron sensors to estimate soil water content

Maik Heistermann et al.
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RC: *Referee Comment*, AR: *Author Response*, ☐ Manuscript text

Dear referee,

thank you very much for your positive response, and for the time and effort spent to examine the manuscript.

We are grateful the various suggestions for further discussion of the existing literature, and will revise the manuscript accordingly, though keeping in mind the aspect of "brevity" as an important requirement to a technical note in HESS.

Please find a point-by-point reply to your comments below.

Thanks again for your willingness to review this manuscript.

Kind regards,
Maik Heistermann
(on behalf of the author team)

Comments and responses

RC: ***[...] Line 20: Typically we don't include dimensions for the a0, a1, and a2 as they are derived coefficients from Desilets 2010. The units make the equation balance, but I am not sure if they need to be included?***

AR: We also noticed that the units for a_0, a_1, and a_2 are mostly omitted in the literature. We do not have a strong opinion here, although omitting the dimensions would, as the referee implied, leave the equation unbalanced. Yet, for consistency with the existing literature, we will drop the units in the revised version of the manuscript.

RC: ***Line 123, Eq 5.: McJannet and Desilets 2023 recently addressed a similar topic on using different neutron monitors and correcting for spatial variability. I am not sure exactly how these approaches are the same or differ, but the citation should be added with a short discussion of that paper. Also in the original COSMOS project an fscaling factor was included. This was also tied to the appropriate reference pressure selected using the COSMOS online calculator (http://cosmos.hwr.arizona.edu/Util/calculator.php). Use of the long-term average pressure for reference pressure instead of the one from the COSMOS online calculator did cause some problems when using the rover at different locations in my experience.***

AR: We appreciate this comment, and agree that McJannet and Desilets (2023) should be mentioned in this context. The best opportunity to do this is, in our view, in the conclusions section after line 333 of the preprint. There, we already stated:

> While the PARMA model is well-established, other such models exist, and future research might explore the potential sensitivity of the neutron intensity scaling on the choice of the model.

We would prefer, though, not to comprehensively discuss the methodological differences here, as a technical note should remain focused on the main idea. However, we'd like to emphasize that using the PARMA model is a subjective choice, and that future research should strive for a more systematic comparison of the existing approaches. This should include McJannet and Desilets (2023), but also e.g. Hawdon et al. (2014) and, as the referee mentioned, the scaling approach used in the original COSMOS paper by Zreda et al. (2012). The latter was based on the method that had been presented by Desilets and Zreda (2003) which is also the basis of the COSMOS online calculator (we mentioned this approach in ll. 40-41 of the preprint). Altogether, the above paragraph could become:

> While the PARMA model is well-established, its application in this study remains a subjective and exemplary choice. Other similar models exist (e.g. Desilets and Zreda, 2003; Hawdon et al., 2014; or McJannet and Desilets, 2023), and future research should aim to explore the potential sensitivity of the neutron intensity scaling to the choice of the model and the consistency of the resulting soil moisture estimates.

With regard to the choice of the reference pressure (last sentence of the above referee comment): The long-term average pressure could address possible effects of a biased barometer. If the effect of altitude is comprised in a static scaling factor (such as in our study), the factor f_p that accounts for the *temporal* variability of barometric pressure should vary around a value of 1. However, if the barometer instrument is biased (which we found occasionally to be the case for various sensors), the long-term average will deviate from the standard atmospheric pressure, so f_p will not vary around 1 and hence propagate the bias.

RC: ***Line 140. The vegetation correction for biomass is still an active area of research so I would suggest a little more discussion here, particularly as its uncertainty is identified as being significant later in the manuscript. Hawdon et al. 2014 found a linear reduction in count rate at low biomass but it became more nonlinear for higher biomass and with sites with forest canopy. Franz 2015 found a linear reduction in count rate around 1% per kg/m^2 for croplands which is similar as reported by Baatz 2015. It seems there is a geometric effect for sites with a clumpy distribution of water, but I am not sure it is fully resolved yet (Franz et al 2013 provided some MCNPx simulations as well as Andreasen 2016, 2017). I am cautious about using the Baatz 2015 empirical equation for all vegetation types especially at high biomass sites within forests where this is likely a geometric factor reducing the impact of increasing biomass on the reduction in neutron counts (i.e. maybe a 0.3 or 0.5% in count rate per kg/m^2). I think so additional discussion of this effect some be added especially as it impacts the main conclusions of the paper.***

AR: Again, we appreciate this detailed comment and the provided references, and agree that the subject of vegetation correction should be addressed in more detail. The referee's comment provides and excellent basis for that, and we will extent the paragraph around 140 ff. accordingly. At the same time, we would like to emphasize that one of the main conclusions of our study identifies the *quantification* of biomass (in high-biomass environments, i.e. forests) as a main source of uncertainty, and we suppose that this conclusion applies even if the reduction of neutron counts should level off to some degree for the presence of very high biomass levels in the sensor footprint. Furthermore, the relationship presented by Baatz et al. (2015) is based on a substantial number of observations with high levels of above-ground biomass (up to 30 kg/m²). Still, we fully agree with the referee that the effect of biomass and its horizontal and vertical distribution are not yet sufficiently understood, and that comprehensive neutron simulation studies might provide a more robust basis for corrections function that hold across various environments.

Altogether, we suggest to revise the paragraph at ll. 140 ff. as follows:

> f_b accounts for the effect of vegetation biomass on neutron count rates. The equation is based on the empirical analysis of a wide range of biomass levels by Baatz et al. (2015), according to which epithermal neutron count rates are reduced by 0.9 % for every kg of dry above-ground biomass per m² (AGB). This rate is similar to the reduction of 1 % per kg/m^2 reported by Franz et al. (2015) for croplands. It should be noted that, based on neutron transport modelling, Andreasen et al. (2017, 2020) found some effect of forest canopy structure on the reduction of epithermal neutron intensity. Apart from this effect of canopy structure, it also remains an open issue as to which extent simple linear reduction rates may apply for very high biomass levels.

RC: ***L238. Avery 2016 also provided an uncertainty analysis and provided a CONUS map of soil properties for use with CRNS rovers and compared local sampling vs. available continuous datasets.***

AR: We thank the referee for pointing out this reference, and we will add it to the list of references which address the role of bulk density in the uncertainty of CRNS-based soil moisture estimation (in l. 238 of the preprint).

It should be kept in mind, though, that Avery et al. conceived the uncertainty of bulk density as the deviation between the bulk density taken from a global dataset (Global Soil Dataset for Earth System Modelling, Shangguan et al., 2014) and the bulk density obtained from in-situ samples. In our study, we do not define a specific source of uncertainty for bulk density, but we acknowledge that the footprint-wide average of bulk density as obtained from in-situ sampling could be quite uncertain itself, given the issue of horizontal and vertical representativeness of bulk density measurements.

RC: ***In general: Crow 2012 describes the relationship between average soil moisture and its variance at different spatial aggregations. This relationship is asymmetric but parabolic shaped. Meaning that you would expect the highest standard deviation at intermediate soil moisture and low SD at low soil moisture and intermediate SD at high soil moistures. This information could be included in the expected range of SD across average soil moisture. I am not sure how this physical constraint would affect the local vs. general calibration suggestions. The same is also true for the bulk density and LW due to textural differences within a CRNS footprint. The point is there is both measurement error due to the instrument/method and natural variation due to spatial variability with a CRNS footprint.***

AR: We thank the referee for this comment. If we understand it correctly, the referee suggests to use scaling relationships such as the one shown by Crow et al. (2012) to guess (or constrain) the uncertainty of our estimated ground truth. However, we should be aware that the relationships between standard devation and spatial mean, as shown in Fig. 3 of Crow et al. (2012), are empirical fits (not physical constraints, as the referee put it). Behind these seemingly smooth relationships is a lot of variability. This can be seen in the original publication by Famiglietti et al. (2008), e.g. in Fig. 6b where the standard deviation varies between 0.02 and 0.09 m³/m³ at the 800 m extent. In any case, we fully agree with the referee that "there is both measurement error due to the instrument/method and natural variation due to spatial variability with a CRNS footprint." We had already tried to emphasize this in the preprint (ll. 239-248) with regard to the uncertainty of θ_{cal}:

The error in the x-dimension relates to what we informally refer to as "ground truth", although the actual level of truth in θ_{cal} remains difficult to determine. All we know is that numerous errors might accumulate along the way, e.g. the measurement error of θ at a single point (possibly systematic, depending on technology), the effects of limited sample size in combination with the limited horizontal and vertical representativeness of each measurement, or the uncertainty of the horizontal and vertical weighting functions.

Overall, we think that an in-depth discussion of the scaling behaviour of soil moisture uncertainty is beyond the scope of this technical note. When we choose between local and global calibration, the magnitude of $\sigma_{\theta_{\mathrm{cal}}}$ certainly is an important aspect, yet any guess at it should consider the specific local conditions.

At the same time, we much appreciate the referee's idea to make use of the aforementioned relationships. In fact, if we acknowledge that (i) spatial variability peaks at intermediate soil moisture conditions, and (ii) that the uncertainty of θ_{cal} propagates less with increasing soil wetness (one of our conclusions), it appears that a local calibration is, if at all, most recommendable under wet soil conditions. In the revised manuscript, we suggest to add a corresponding sentence after l. 347 of the preprint:

[...] if the calibration were carried out under dry conditions, the error would grow substantially under wet application conditions. [...] Given that the spatial variability of soil moisture tends to reach a maximum under intermediate wetness conditions (see e.g. Crow at al., 2012; Famiglietti et al., 2008), it hence appears recommendable to obtain θ_{cal} under rather wet conditions [...]

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