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Discussions

Interactive comment on "Use of cosmic ray neutron sensors for soil moisture monitoring in forests" *by* I. Heidbüchel et al.

Anonymous Referee #2

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The authors present a straight forward paper looking at mutli-calibration estimates and methods for a study site in Germany. While the methods in the paper have been previously tested in a variety of ecosystems around the globe the application to this particularly ecosystem is insightful and help further advance the CRNP method. In particular, this site contains a relatively large amount of the total hydrogen in the forest canopy given the sandy and dry soils. The paper is well written and appropriate for the HESS community. My comments and assessment largely follow Heye Bogena so I will only add new comments here or reiterate key points.

Major:

The site is interesting in that a potentially a large part of the hydrogen is contained

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in the biomass instead of the pore water content. A figure like Figure 3 in McJannet 2014 WRR for each calibration data point would be interesting to look at. In addition, perhaps some of the bias in the N0 parameter is because of how hydrogen is more distributed in the clumped biomass instead of distributed throughout the soil. Plotting the differences of N0 vs. relative biomass hydrogen to total hydrogen ratio might show this influence. Franz 2013 GRL supplemental figure S2 illustrated the influence of neutron intensity/counts due to clumped hydrogen in the tree canopy vs. more distributed hydrogen in the soil. Perhaps these detailed calibration datasets might help validate or refute these modeling results.

The conclusion that the deviation of a single calibration point is upwards of 0.12 m3/m3 is technically correct at the wet end. However, this is a bit misleading given that the neutron counts are never this low or soil moisture this high, particularly at the daily average level, because of the sandy soils. I suggest the authors use the min and max observed counts to properly assess the maximum uncertainty of the method. Looking at Figure 9 it looks like the CRNP never reads above 0.27 m3/m3. All in all, this a fairly small change. Also might be more useful to look at percent absolute error instead of just the difference.

Comment: For timescales below the daily level, and thus estimates of the peak soil moisture, clearly some cleaver smoothing filters are needed to estimate the “true peak” and separate out the signal from the noise. This estimation of the true peak will help constrain things like calculating effective infiltration flux and maybe even runoff depths for water balance studies using the CRNP data.

The method of determining lattice water by weighing the sample at 105, 400 and 1000 C has not been used by the USA COSMOS community (pg9820 L 18-30). Are they any refs suggesting this is a defensible method compared to the more rigorous approach used by Actlabs? I suspect this difference will be small here as you account for the burn off of carbon. However, for certain soil groups (volcanics?) I imagine this might be problematic. Please add any supporting refs or comment on the pragmatic approach

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taken here vs. the more rigorous laboratory approaches taken in previous COSMOS work.

Minor:

P9816 L25. Already is awkward transition. Maybe something like “As early as 1966. . .”

P9837 L8. Franz 2013 WRR investigated the impact of horizontal heterogeneity on the signal.

P9839 L21-25. Again, is this method for lattice water supported by refs? If not then should be noted that this is a pragmatic procedure with expected minimal error for most soil groups other than volcanics, . . . etc. (?). Unfortunately I don’t know all the soil groups this might be affected by so hopefully a pedologist can set us straight.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9813, 2015.