Response to Interactive comment by Anonymous Referee #2

Comments from referee are printed in black. Authors' responses are printed in red.

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.
We thank reviewer #2 for reading and assessing our manuscript.

Major:
The site is interesting in that a potentially a large part of the hydrogen is contained 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.
Very good suggestion. We will prepare a figure (similar to the one of McJannet) for 3 distinct soil moisture conditions and add it (new Fig. 11). In order to do this we will refine our calculations of the different hydrogen pools (now also including SOM, lattice water as well as canopy and litter interception) and also add more detail to the discussion. This new analysis supports the idea that interception can have a significant influence on soil water content measurements performed with a CRS.

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.
We will modify this section stressing the fact that 0.12 m^3/m^3 is the largest deviation that we observed. Due to the sandy soils, the absolute range of soil water content is fairly small at our site. Following your suggestion we will change absolute error to percent absolute error both in the figure and in the text.

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.
This is true. We hope that such clever smoothing filters will be developed in the future to provide us with the true peaks using additional information (like time series of precipitation).

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 taken here vs. the more rigorous laboratory approaches taken in previous COSMOS work.
The method to determine SOM proposed in this paper is officially called the loss on ignition method. We chose this method since it is indeed pragmatic and does not require the acquisition and handling of toxic chemicals (like for example the Walkley-Black method does). References that describe this method are for example Ball (1964), Ben-Dor & Banin (1989), Davies (1974), Howard & Howard (1990), Schulte et al. (1991). In clay-rich soils the loss on ignition method overestimates SOM content since also some of the lattice water evaporates. This leads to an underestimation of soil hydrogen content (since lattice water contains a higher fraction of hydrogen than soil organic matter). One complication for calcareous soils is the thermal breakdown of carbonates at high temperatures. Although this thermal breakdown can be avoided at temperatures below 430°C (Davies, 1974) at temperatures above that the burned off carbonates would contribute to the lattice water account. We still think that the results obtained with this method provide good estimates of SOM and LW for our purposes with minimal error for most soil groups.

Minor:
P9816 L25. Already is awkward transition. Maybe something like “As early as 1966. . .”
Will be changed accordingly.

P9837 L8. Franz 2013 WRR investigated the impact of horizontal heterogeneity on the signal.
In that 2013 paper the lowest count rates are also above 1000 cph.

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.
The method of heating the samples to a temperature of 1000°C to determine lattice water was used in many CRS studies (e.g. Zreda et al., 2012; Bogena et al., 2013). The only complication we found occurs in carbonate-rich soils where thermal breakdown of carbonates will contribute to the lattice water account. We will add two cautions to the recommendations we give in the appendix.