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Interactive comment on "Can canopy interception and biomass be inferred from cosmic-ray neutron intensity? Results from neutron transport modeling" *by* M. Andreasen et al.

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Summary

The Authors present the results of a neutron model used to explore the effect of different hydrogen pools on the signal of the Cosmic-Ray neutron sensors (CRNS). The neutron model was set-up to mimic a specific forest site in Denmark. Based on that, a sensitivity analysis (SA) to several environmental conditions (7 factors) was provided. The effect on thermal neutrons, epithermal neutrons and sensors placed at different heights are discussed. The study is relevant since the CRNS is a method that was applied in several conditions for soil moisture measurements but the role of other hydrogen pools has to be further investigated. Overall, the manuscript (MS) could be an

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interesting publication suitable for HESS. However, it needs improvement in different directions. The story line is not always consistent, the introduction part is limited and the presentation of the results should be better organized. Finally, I think the MS could be extended with a discussion section. For these reasons I think the Authors should put some more effort to improve the manuscript before publication.

General comments

[1] The story line is built on the use of CRNS for biomass and canopy interception while a SA is conducted to explore the role of several other hydrogen pools. Moreover, in my opinion, the manuscript is relevant also because the neutron modeling explores in details the use of thermal neutrons and, for the first time, the use of sensors placed at different heights. However, these two novel aspects are completely missed in the introduction and they are taken for granted in the discussion of the results. For these reasons I think the story line is not consistent with the actual analysis reported and introduction and conclusions does not provide a clear roadmap and summary of what this study accomplishes. Overall the manuscript should be reshaped along a clearer story line more consistent with the analyses reported where the Readers should be introduced to the actual state of the CRNS applications (e.g., only moderated counter and just above ground measurements). Novelties of the study and concluding remarks about potentiality and limitations should be better clarified in the final conclusions (i.e., the use of the bare counter and ratio between bare and moderated; the use of sensors placed at different heights; the effect of several environmental conditions to the signal). Specific comments/suggestions are reported below.

[2] Despite I understand the goal of the Authors to strengthen the need of such a study, I found in the introduction several statements that are misleading (e.g., P2L19-25). Contrary to what is stated by the Authors, in my knowledge several important contributions were published to address the (wanted or unwanted) effect of additional hydrogen pools. Moreover, most of these studies focused on the effect of biomass e.g., in addition to the references reported in the MS, preliminary evaluation of biomass were

presented in (Rivera Villarreyes et al., 2011); (Franz et al., 2013) presented an approach to isolate any hydrogen pools but soil moisture and showed the estimation of the crop biomass; (Baatz et al., 2015; Hawdon et al., 2014) introduced an empirical correction to account for biomass. In comparison to biomass, the effect of snow on CRNS signal has received much less attentions. Even if the first concepts were already introduced by (Desilets et al., 2010), a preliminary analysis was just presented by (Rivera Villarreyes et al., 2011) and only recently a study with longer time series of snow was published (Sigouin and Si, 2016). Other hydrogen pools were also addressed: e.g., the analysis of the role of little layer was discussed in detail by (Bogena et al., 2013) and in (Baroni and Oswald, 2015) we presented the first measurements for quantifying also the canopy interception. Overall I believe that all these experimental studies called for additional attentions on hydrogen pools than soil moisture. In this context, the present MS is the first modeling study where complex forest is simulated and the effect of several environmental factors are explored. For these reasons I think the MS could represent a good answer to those calls and the introduction of the MS should be rephrased accordingly.

[3] I found the presentation of the results obtained with the reference model and the forest conceptualizations not clear (P11L24- P13L7). The Authors first stated about a remarkable agreement of the reference model (P12L12). Later they compared different forest conceptualizations and they found the best fit not to be unique (P13L2-4). Similarly they stated that they cannot determine which conceptualization is more realistic (P16l7-9). For this reason they conducted the SA using two conceptualizations. Overall, I believe that the mismatch should be clearly acknowledged from the beginning. Assuming that two forest conceptualizations are selected, the results of the SA could be then presented.

[4] The discussion of the results of the SA is not always clear and together with the 17 images I think the Readers are lost on the major findings of the study. In addition most of the discussion reported is a qualitative description of the figures. I would sug-

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gest searching for a way to sum up the results section (i.e., reducing the number of figures) where first the results of thermal and epithermal neutrons are discussed providing a quantitative comparison of the different effect of the environmental conditions explored. Secondly the ratio between thermal and epithermal is introduced and results are discussed for the factors that showed different response in thermal and epithermal neutrons.

[5] It would be interesting to extend the MS with a discussion section where the overall results of the SA are summarized e.g., the advantages of using sensors at different heights, the advantages of using thermal and epithermal neutrons, the misfits of model and measurements and indication for further improvements. Concluding remarks could stress the potential use of CRNS for other applications but it would be interesting to extend the discussion also on the role of the spatial sensitivity of the sensor i.e., any estimation by CRNS is a spatial weighted value of the actual target (e.g., biomass). Finally, it is stated that for a good matching between measurements and simulations it was important the correcting factor (Page 7, L10-23). Since all the probes installed so far around the world does not account for that, it would be important to know what the implications are e.g., could we aspect the same sensitivity to environmental conditions when comparing bare and moderated counter instead of thermal and epithermal neutrons?

Specific comments

Page 1, L1-2: the title's focus on biomass and canopy interception is not entirely representative of the sensitivity analysis presented in the MS, which is broader. It should be rephrased accordingly.

Page 1, L18: in my knowledge the effect of snow has received much less attention than other hydrogen pools. In addition, the analysis reported in the MS does not focus on biomass and interception but several other factors are discussed. For this reason I would rephrase the sentence in "...soil moisture but several other hydrogen pools

affect the signal".

Page 1, L22-31: in my opinion the presentation of the main results should be extended to honor also the other analyses provided in the MS (i.e., the role of the other factors).

Page 2, L12: the terminology used (static, quasi-static and dynamic) is too arbitrary. For a clearer discussion I would suggest presenting the hydrogen compartments in term of temporal scales (e.g., hours/days, season, years).

Page 2, L15: for consistency I would mention here that the signal of hydrogen pools with low temporal dynamic (e.g., lattice water, SOC etc) is usually subtracted.

Page 2, L18-25: see general comment #2 and the additional references reported to reshape the paragraph.

Page 2, L26 – Page 3, L18: the sensitivity analysis focuses on several environmental conditions. In the light of reshaping the MS to honor this, I would say that these paragraphs are not relevant and could be omitted.

Page 3, L19 – L34: summary of the aims of the paper and the methods should be rephrased to honor the actual analysis i.e., sensitivity analysis to environmental conditions to understand the role of different hydrogen pools.

Page 4, L1-2: the sentence is misleading: as reported in general comment #2 several publications were presented to estimate biomass. In (Baroni and Oswald, 2015) we have also presented the first measurements of canopy interception. Even in the case the Authors have any concerns about these studies, I think it would be part of the constructive advanced of the research field to integrate these opinions in the MS.

Page 4, L3: this section 2 could be moved and integrated in the section 3.2.4 Field measurements.

Page 7, L10-23: for a clearer description of the results, the Authors could start the section making the list of the factors analyzed and referring here also to table 5. In

addition the values presented in Table 5 could be plotted for easier comparison (e.g., bar plot).

Page 12, L4: I'm surprised: do you really think that the soil moisture profiles could explain such a difference? But in case it is relevant, why did you not evaluate this in the SA? Overall understanding the role of the different factors (i.e., environmental conditions) is the goal of the SA and of this paper.

Page 12, L12: I think the term "remarkable agreement" should be rephrased in the light of the overall discussion reported about the discrepancies and the inability to define which conceptualization is more realistic (e.g., P16L8). In addition I noticed that the thermal measurements show a regular decreasing from the ground to the canopy level. On the contrary the epithermal measurements show an inversion: the measurements decrease from ground to 5 meters and then start to increase regularly when moving to the canopy level. If I'm not wrong none of the models conceptualizations and the different environmental settings is able to reproduce this behavior. For this reason I think it could be an important result to discuss. Unfortunately this behavior is detected only for the profile measured on Mar-2014 while the measurements conducted on Nov-2013 does not have these measurements in the plot: is this a mistake in plotting or really do you not have these measurements?

Page 12, L26: if it is a SA the results should be discussed in term of sensitive or not sensitive. The term "satisfactorily" suggests that here you are still looking for a forest conceptualization that fits the profile measurements. See also general comment #3.

Page 13, L8-19: this paragraph could be titled as a new section e.g., effect of soil moisture. Possibly, the analysis could be extended to explore the effect of soil moisture profiles (see also comment Page 12, L4).

Page 14, L25 - Page 15, L8: the presentation of the results jumps from the description of the thermal neutrons to the ratio i.e., epithermal neutrons are not described. For a clearer presentation I would suggest first to discuss both thermal and epithermal

neutrons. Secondly, to introduce the use of the ratio explaining the reasons for doing that e.g., what do you expect to see with the use of the ratio instead of the single signal?

Page 18, L21 - Page 19, L5: the discussion about the results obtained with the field locations of Voulund and Harrild is very limited and it refers to analysis presented in the submitted (and not available) paper of Anderson et al. (2016). Moreover I think that at the current status, these results do not provide any new insights on the present study. Either the Authors integrate better the description, the analyses and the results obtained in these locations, or in my opinion the results obtained based on these two sites could be completely omitted in this MS.

Page 18, L19: as discussed also in previous comments. I think the term “remarkable agreement” is misleading. On the contrary I think the misfits are interesting results to highlights providing the base for further studies.

Page 18, L22: before starting speaking about canopy interception, I would introduce also a summary of the role of the other hydrogen pools explored. This would better honor the SA reported in the MS.

Technical corrections

Page 4, L11: eV

Page 13, L26: the definition (4th order, 3rd order) of the chemical complexity are not self-explained. I would suggest instead the use in the table of other definitions e.g., (SOM+Gd+Root+??+SiO2) for the more complex and so on.

Page 13, L28: what is cts? To be defined.

Page 15, L4: conditions instead of locations.

Figure 1: the domain represented in the figure is too extended and not well informative. I would suggest using this as a general overview but adding also a panel where the

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positions of the experimental sites are visualized with higher resolutions.

References

Baatz, R., Bogena, H.R., Hendricks Franssen, H.-J., Huisman, J.A., Montzka, C., Vereecken, H., 2015. An empirical vegetation correction for soil water content quantification using cosmic ray probes. Water Resour. Res. n/a-n/a. doi:10.1002/2014WR016443

Baroni, G., Oswald, S.E., 2015. A scaling approach for the assessment of biomass changes and rainfall interception using cosmic-ray neutron sensing. J. Hydrol. 525, 264–276. doi:10.1016/j.jhydrol.2015.03.053

Bogena, H.R., Huisman, J.A., Baatz, R., Hendriks-Franssen, H.-J., Vereecken, H., 2013. Accuracy of the cosmic-ray soil water content probe in humid forest ecosystems: The worst case scenario. Water Resour. Res. n/a-n/a. doi:10.1002/wrcr.20463

Desilets, D., Zreda, M., Ferré, T.P.A., 2010. Nature's neutron probe: Land surface hydrology at an elusive scale with cosmic rays. Water Resour. Res. 46. doi:10.1029/2009WR008726

Franz, T.E., Zreda, M., Rosolem, R., Hornbuckle, B.K., Irvin, S.L., Adams, H., Kolb, T.E., Zweck, C., Shuttleworth, W.J., 2013. Ecosystem-scale measurements of biomass water using cosmic ray neutrons. Geophys. Res. Lett. n/a-n/a. doi:10.1002/grl.50791

Hawdon, A., McJannet, D., Wallace, J., 2014. Calibration and correction procedures for cosmic-ray neutron soil moisture probes located across Australia. Water Resour. Res. n/a-n/a. doi:10.1002/2013WR015138

Rivera Villarreyes, C.A., Baroni, G., Oswald, S.E., 2011. Integral quantification of seasonal soil moisture changes in farmland by cosmic-ray neutrons. Hydrol. Earth Syst. Sci. 15, 3843–3859. doi:10.5194/hess-15-3843-2011

Sigouin, M.J.P., Si, B.C., 2016. Calibration of a non-invasive cosmic-ray probe for

wide area snow water equivalent measurement. The Cryosphere 10, 1181–1190. doi:10.5194/tc-10-1181-2016

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