

Interactive comment on “Calibration of a non-invasive cosmic-ray probe for wide area snow water equivalent measurement” by M. J. P. Sigouin and B. C. Si

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Dear Authors,

I take the chance of the open discussion provided by the Journal for adding a short comment. I hope this could help for further improving of the manuscript.

We have experienced the use of Cosmic-Ray neutron sensing (CRNS) since 2010. Our studies focused mainly on soil moisture measurements. However the role of snow was also detected and a preliminary concept for possible quantification was provided (see fig. 9 in Rivera Villarreyes et al., 2011). After that experience, we realized that CRNS has several opportunities to estimate not only soil moisture. For this reason we

C1

put some efforts to show the possibility to identify additional hydrogen pools (Baroni and Oswald, 2015). Similarly, I believe that also your contribution for snow estimation is a valuable and important study to explore new applications.

Independently from the target of the study (soil moisture, snow etc.), I think one of the main challenge that we are facing now for the applicability of the method is the characteristics of the footprint. The temporal variability of the penetration depth of the CRNS as a function of hydrogen pools was already underlined in the earlier publication (Zreda et al., 2008). The need of a vertical weighting function was developed later (Franz et al., 2012). Recently, Köhli et al. (2015) showed that also the spatial footprint shrinks in space and a spatial weighting function is also needed. Overall we have to take into account that the water estimate by CRNS is a weighting value within a footprint that changes in time. So far the studies focused on soil moisture but we could expect that the same happens in snow conditions. One could even speculate that the role of snow could be even stronger i.e., smaller footprint and stronger time variability. Exactly for this reason I would suggest the Authors to include in the analysis a spatial and vertical weighting function for the point snow measurements. The same comment was underlined by the Reviewers (e.g., Reviewer 1: the author should then recalculate the regression using only the nearest points, and see if the regression improves) but I write to emphasis that a time dependent weighting function (horizontal and vertical) might also be necessary i.e., the weights might change in each campaign.

A small final remark is also that I did not find information about the altitude of the experimental site. Since this effects the dimension of the footprint (more precisely by the relation between altitude and air pressure) I would suggest the Authors to provide additional information and in case to extend the discussion. For an estimation of the footprint as a function of pressure see eq. 21 on (Desilets and Zreda, 2013).

In conclusion, I would suggest the Authors putting more effort on the analysis of the data and to extend the discussion accordingly. With these, the manuscript could represent more than an additional proof of concept on the use of CRNS for snow measure-

C2

ments but it could show some new insight on how to use the method for this application.

Best regards,

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