

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-124

Anonymous Referee #2

Referee comment on "Snow water equivalent measurement in the Arctic based on cosmic ray neutron attenuation" by Anton Jitnikovitch et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-124-RC2>, 2021

Referee Report of « Cosmic-ray neutron method for the continuous measurement of Arctic snow accumulation and melt» (TC-2021-124)

General comments:

The manuscript presents the results of two tests of cosmic ray neutrons sensors in Canada during two consecutive winters, one site being in the Canadian Arctic with a transect of five sensors allowing to continuously measure a snowdrift.

The presented study is somehow a technical report of an interesting in-situ test (especially the Canadian Artic tests), but it brings very few technical or scientific novelty on this already well documented topic.

Furthermore, a significant part of the manuscript deals with the calibration of linear models for the neutron count-SWE relationship, whereas this relationship appears to be highly non linear, according to the manufacturer of the sensors (given the calibration function provided) and the available literature. And in the following parts of the study, the non-linear relation is finally used to produce a continuous SWE signal from the CRNS and to assess the snowpack evolution throughout the season. This work on linear relationship appears then to neither statistically sound nor useful in this study.

I would recommend then a major revision of this paper, in order to tackle with those main issues.

Detailed comments/questions:**Title:**

I see two issues in the proposed title: 1) "Cosmic-ray" is a physical process on which the measurement method is based, not a method by itself, and 2) the proper physical variable measured here is the SWE, which deserves to be mentioned in the title. An alternative proposition for the title could be: "Snow Water Equivalent measurement in Arctic based on Cosmic-ray neutron attenuation".

Abstract:

Lines 23-25 the correlation (both Pearson and R²) and RMSE values provided here are not very informative in an abstract, especially considering the limits of the use of linear regression in this context (see comments below). A more qualitative indication on the accuracy/reliability of such sensors in the Artic context is expected.

Text:

Line 35: "often less than 300 mm": is it snow height or SWE?

Line 45: "often located at non-representative locations": what does it mean? Why such locations have been chosen then?

Lines 51-52: "using gamma attenuation [...] but again are limited to point measurement": as mentioned line 65, airborne gamma methods can provide snow mapping, thus are not limited to point applications.

Line 67: "Cosmic ray attenuation methods have not been extensively tested": this is not true. Several wide-scale fleets of CRNS are currently in operation, for industrial use, e.g. in France (about 40 sites, the oldest since about 20 years now, see Paquet, E., & Laval, M. T., 2006) or Spain (Cobos et al., 2010). The French example is anyway evoked later in the manuscript... Bogena et al. (2021) indicate that "worldwide, about 200 stationary Cosmic Ray Neutron Probes have been installed since the introduction of the method".

Line 70: "neutrons in the fast to epithermal range": please define this terms for non-

specialists.

Line 134: "neutron count during the time of interest": please define this time of interest.

Line 154: please provide the reference in which this attenuation coefficient has been introduced, and the physical / statistical processes to which the parameters are linked.

Line 213: "the surveys included accumulation and snowmelt conditions": was it more than a snow-core campaign? If not, the term "snow-core measurement" is more appropriated.

Lines 223-226: this part deserves to be moved at the end of the Part 2, just after Equation 5, as it provides details about the calibration parameters used in this equation.

Line 230: "Additionally, we increased the a1 parameter in order to create a site-specific calibration": why this parameter? Given the equation 5, it adds/removes counts from the corrected N value, accounting for attenuation not related to SWE.

Tables 1&2: the captions refer to "weighting function parameters" of the Equation 5, although this equation describes an "attenuation coefficient" (line 152).

Lines 251-252: "a bivariate analysis [...] using a linear regression": I think this is the main issue of this study. Why using a linear model considering the highly non-linear link between N and SWE, given Equations 4 and 5? Furthermore, a detailed formulation of the attenuation coefficient has been introduced just before, allowing a local calibration of its parameters. Why not using it? If it is useless or difficult here, thus leading to a simpler model, this deserves to be detailed and justified.

Lines 255-284: this part is not easy to read, the numerous values and statistical scores provided deserve to be gathered in a table. The correlation coefficients R, both Pearson and R^2 , somehow rely on the hypothesis of a linear relation between the two variables, which is problematic here, as written above. The Pearson correlation has very high absolute values, it only shows the obvious anti-correlation of N and SWE, which is the very base of the measurement method. The RMSE should be related to the maximum SWE value to be informative. As written above, one expects a comparison with the no linear model $SWE=f(N)$ based on equations 4 and 5.

Lines 274-277: Once again, I don't understand why such an "expert" correction (which is not of second order here, 5 mm have been added to SWE of about 15-35 mm) has to be

introduced before inferring a linear model, whereas such an effect could be compensated thanks to parameter a1 in Equation 5.

Lines 283-284: "it is extremely important to install CRNS prior to the start of the snow covered season": considering this, I am not sure that the Feb-Mar 2017 data at Elora are usable in this study.

Lines 285-289: I wonder why the Kodama approach (somehow the "father" of the cosmic-ray based SWE measurement) is not appropriated here, but neither the model used nor the comparison to snow core SWE are provided to illustrate this issue.

Lines 298-329: Same remarks as for Elora results. Furthermore, the data shown in Figure 5 look like a typical pitfall for linear regression, with most of the data grouped between N values of 200-300, and few others above 600. At least a Theil-Sen regression could have been used for a more robust estimate. It is not clear whether the confidence interval on the SWE values of the Figure 5 is the one of the snow core measurement. In that case, for low SWE values, the lower part of this confidence interval goes to negative SWE, which is not realistic for a snow core measurement (at worst, no snow is cored).

Line 334: "Using Equation 4, or the relationships between neutrons counts and SWE": according to the caption of the Figures 6 and 7, the continuous SWE signal is computed thanks to the non-linear relation given in Equation 4. Then what is then the point of the linear models presented before?

Line 348: "snowfall, snowmelt, sublimation and wind erosion/transport": at this point, these are only guesses of the involved processes. The CRNS measurements should be completed at least by local wind and temperature measurement to confirm this, providing proxies.

Line 372: Does this mean that the SWE signal presented in the Figure 7 is averaged across the 5 CRNS in the transect?

Line 377: "Peak SWE occurred [...] one week prior to the onset of the snowmelt": Considering the "noise" of the SWE signal, the maximum snowpack seems to be reached almost one month before. Same remarks applies to line 381.

Line 392-409. I felt uncomfortable to see some direct interpretations of the presented data mixed with some other considerations drawn from literature and not directly deducible from the observations, like in lines 400 to 404. This is somehow an over-interpretation of the presented results. The spatial pattern of the snowdrift could be better

illustrated by plotting the ratio of local SWE v/s the averaged value, which could show that, at least for the 2016/2017 season, the spatial pattern of the snowdrift is rather stable throughout the season.

Line 410-412: "This unique dataset [...] water resource management": this conclusion is somehow quite emphatic considering the results presented, interesting but limited in time and space.

Figure 8: The caption is too long and too detailed, some of the details are (or could be) given in the text.

Line 423: "A strong negative correlation was found between then counts and the manual SWE measurements": given the principle of measure of the CRNS, the contrary would have been a great surprise. I am not sure it is significant conclusion to be put here.

Line 432: "the terrestrial set of parameters [...] however, the glaciär sets of parameters": please refer to the tables 1 & 2. Once again, the conclusions here, like the chapter before, deal with the "non-linear" formulation whereas the "linear models" have been extensively documented in the paragraph 4.1, but finally poorly used in the study. A detailed and illustrated scoring of the "non-linear" formulation deserves to be presented instead.

Line 438-440: Same remark as for lines 410-412.

Suggested additional references:

Bogena, H. R., Herrmann, F., Jakobi, J., Brogi, C., Ilias, A., Huisman, J. A., ... & Pisinaras, V. (2021). Monitoring of Snowpack Dynamics With Cosmic-Ray Neutron Probes: A Comparison of Four Conversion Methods. Innovative Methods for Non-invasive Monitoring of Hydrological Processes from Field to Catchment Scale.

Cobos, G., Francés, M., & Arenillas, M. (2010). Le programme ERHIN. Modélisation nivo-hydrologique pour la gestion de l'eau du bassin de l'Ebre. La Houille Blanche, (3), 58-64.

Paquet, E., & Laval, M. T. (2006). Retour d'expérience et perspectives d'exploitation des Nivomètres à Rayonnement Cosmique d'EDF. La Houille Blanche, (2), 113-119.