

The Cryosphere Discuss., author comment AC2
<https://doi.org/10.5194/tc-2021-124-AC2>, 2021
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Reply on RC2

Anton Jitnikovitch et al.

Author 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-AC2>, 2021

Thank you for this review.

Author Reply:

I disagree on a few significant points.

- While cosmic ray neutron attenuation for soil moisture, and to a lesser extent SWE measurements, (primarily using above-ground CRNS) have been documented, our specific approach of using a modern, grounded-in-situ sensor, which is not physically buried, and being applied to an Arctic landscape as well as in an exceptionally deep snowdrift – which may be a common utilization for this instrument, has only limited testing to date. I respectfully disagree that the testing of a CRNS in the Arctic, as well as the above points, is a well-documented topic.
- The specific approach utilized in section 4.3 has not been applied or showcased in literature and is a novelty in itself – providing a unique characterization of a single significant feature. This approach can be expanded to other prominent features in known watersheds and provide valuable information significantly more efficiently and effectively in nearly all regards to currently available instrumentation. The realization that the manufacturer-provided parameters may be required to be adjusted in a continental Arctic environment is an important identification that has not been reported on.
- Section 4.2 and 4.3 exclusively use the non-linear equations. I agree that section 4.1 can be shortened and 4.3 expanded – with that said, there is value in the linear regressions, which we will clarify in the upcoming submission.

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".

Author reply: Thank you, we will consider adjusting the title of this work to: "Snow

Water Equivalent measurement in the Arctic based on Cosmic-ray Neutron Attenuation"

Abstract:

Lines 23-25 the correlation (both Pearson and R^2) 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 Arctic context is expected.

Author reply: Thank you. The abstract will be adjusted accordingly.

Text:

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

Author reply: This is referring to SWE and will be clarified in the following submission.

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

Author reply: Locations that are not representative of the broader area of the Arctic are typically chosen to be located at town sites; for search and rescue bases/stations; to improve military capabilities; to function as entities that legitimize national or sovereign claims; and to engage in multilateral actions to protect Arctic infrastructures (Goodsite et al., 2016). Additionally, many purely research-purpose Arctic environmental monitoring stations have been permanently closed (Schiermeier, 2006; Rees et al., 2014). We will provide some additional clarification for the following submission.

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.

Author reply: We will update the text and provide clarification regarding gamma methods and snow mapping in the following submission. We will note that airborne gamma methods are campaign-based and expensive, while point measurement gamma methods are not able, and are not as efficient, in providing detailed measurements across a single deep snow feature in the same way as grounded-in-situ CRNS.

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".

Author reply: Bogena et al (2021) - that comment regards 200 stationary CRNP which are primarily used to measure soil moisture and are above ground sensors. It is noted that the ground-based sensors are buried, which also differs from the approach used in this work and most others. There does not appear to be any mention in any of the above papers of testing in an Arctic landscape. These are significant variances from the approach and modern sensor type used in this work. The type of sensor in this work, and the approach, has only limited testing to date. We will further clarify that we are specifically discussing the grounded-in-situ measurement approach method and specifically in the Arctic. We will update the text to clarify the following:

- This specific model type (SnowFox™) has only limited testing to date.

- Modern versions of grounded in-situ CRNS have not been tested in a continental Arctic landscape for SWE.
- Grounded in-situ CRNS have not been tested in a transect methodology to provide detailed continuous, near real-time observations of a single snow-feature or common Arctic features (such as an Alder shrub patch) over complete winter seasons.
- Grounded in-situ CRNS have not been tested where the instrument setup occurred after the initial snow precipitation of the season – we will expand what impact this has on the SWE measurements, such as if the data is reliable. We may also clarify what steps can be taken to recalibrate the sensor to provide meaningful measurements.
- We will clarify that current studies using this model of grounded in-situ CRNS use the default manufacturer-provided parameters, while our works appeared to identify that adjusting the parameters could lead to a significantly improved measurement accuracy. We will emphasize our assertion that this is likely due to a typical Arctic subsurface being not pure water/ice, but also having mineral and organic properties, as well the hummocky subsurface being highly porous and permeable.

We will provide clarification that the majority of research involving CRNS use an ‘above-ground’, sometimes referred to ‘non-invasive’, type of sensor which although also uses an attenuation of cosmic-ray neutrons to measure the SWE, is set up, calibrated, and operates differently and has different advantages/disadvantages.

Note: Grounded in-situ CRNS refers to a setup approach where the sensor is always in contact with the soil-interface and specifically in our works, is not buried – this terminology builds upon that proposed by Gugerli et al. (2019).

Line 70: “neutrons in the fast to epithermal range”: please define this terms for non-specialists.

Author reply: Thank you. We will clarify this in the following submission.

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

Author reply: We will clarify that the time of interest is during the snow season. Thank you.

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.

Author reply: This comment is not clear to the authors. The attenuation coefficient formula was provided by the manufacturer and determined through field validation and calibration studies. This is noted in line 156 where we note to refer to Sect 3.4 CRNS Parameters – more specifically, line 225.

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.

Author reply: Thank you. We will update this in the following submission to clarify the verification was a snow-core campaign.

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.

Author reply: Thank you. We will consider moving this to the end of Part 2.

Line 230: “Additionally, we increased the a_1 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.

Author reply: We used a systematic approach on each of the parameters and observed a significant increase in data quality (in comparison to field measurements) when adjusting only the a1 parameter. We consulted and confirmed with the manufacturer regarding this approach. We acknowledge that additional research is beneficial to investigate the reasoning and will update the text to suggest for future works to investigate the impact of each parameter.

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

Author reply: These are just fitting parameters. We will refer to them as "factory parameters" in the following submission.

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.

Author reply: Thank you. We will clarify the potential benefit of the regressions. We will clarify how the regression equations could be used to demonstrate the changes due to soil water conditions between different years and will note that at the monitored sites the soil water storage is fairly similar and that therefore the functions are well transferable in time. We will also consider applying the regression approach to estimate future SWE and demonstrate that at certain sites, the regression approach may be used to predict future SWE – until now, this approach has only been utilized using an above-ground CRNS. We will also shorten section 4.1.

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 R², somehow rely on the hypothesis of a linear relation between the two variables, which is problematic here, as written above. 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.

Author reply: Thank you. We will adjust and take this comment into account.

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.

Author reply: We estimated the water capacity of the topsoil layer to be 1.3 to 2mm/cm (Blencowe, 1960 and Ball, 2001) and we assumed a 50% soil moisture. A similar approach was previously used by Sigouin and Si (2016) and values of the same order were identified. We do not consider this to be an "expert" correction. Note that this approach was only applied to non-zero SWE values while adjusting the a1 parameter would impact all values.

Lines 283-284: "it is extremely important to install CNRS 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.

Author reply: Thank you. We will update the text, however, we believe there are valuable lessons to be noted from this 'late-season installation' approach and will clarify this in the following submission.

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.

Author reply: From the authors understanding, Kodama et al. is among the first research teams dedicated to testing and publishing results using a grounded-in-situ CRNS sensor for SWE. However, we will remove this paragraph for the following submission as it is not a significant aspect of this works.

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, with is not realistic for a snow core measurement (at worst, no snow is cored).

Author reply: We acknowledge this comment and will adjust for the following submission – including incorporating a Theil-Sen regression. Thank you.

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?

Author reply: Equations 4 and 5 were used for all of the SWE measurements in this works (other than physical snow survey data). As noted previously, we will clarify the benefit of the linear equations.

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 wind and temperature measurement to confirm this, providing proxies.

Author reply: Thank you. We will look to incorporate wind and temperature data as supplemental documentation for the following submission.

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

Author reply: Yes – we will clarify this.

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.

Author reply: Thank you. We will confirm for the following submission.

Line 392-409. I felt uncomfortable to see some direct interpretations of the presented data mixed with some other considerations drawn out form 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.

Author reply: I respectfully disagree.

I am not sure what is uncomfortable about lines 392-399 or 404-409. However, I will further clarify that the statements made are our interpretations based on well-established principles and on the continuous dataset from the 5 CRNS in the transect.

As for lines 400-401, I do not find our interpretations uncomfortable – for example: “the early season melt is retained within the snowpack as liquid water is refrozen into ice (Pomeroy and Gray, 1995; Marsh and Pomeroy, 1996; Wrona, 2016)” is an established and well-known phenomenon, especially when considering 1) a considerably deep snowpack and 2) an Arctic environment with temperatures fluctuating between freezing and above freezing – as is common during a spring melt season. The citation Wrona (2016) is a conclusion from the same TVC site. I believe this is an accurate and representative conclusion. However, for the following submission, I will clarify the text to note this is our interpretation of the results using well-established principles and that we visually noticed the snowpack melting – there was clear variability in the amount of water saturation within the snow-cores on different days (which at times, appeared to refreeze into thin and weak ice layers) – which is how we came to our conclusion. I will include additional text and adjust the positioning of the references to clarify that this is our interpretation of the results based on established principles. I will also look to include local weather data.

I also disagree about lines 401-402 being “uncomfortable” as infiltration of snow to the ground/soil (or lateral runoff) is a well-known phenomenon and a well-established principle; especially during times of spring melt. For reference: “Further into the season, after sufficient melt, water is available to infiltrate the soil or runoff laterally (Quinton et al., 2010)”. I am unsure how one can directly deduce observations of a deep snowpack infiltrating in a natural, non-controlled environment, we are making interpretations and conclusions based on well-established principles. It is unclear why this is uncomfortable. However, I will include additional text and adjust the positioning of the reference to clarify the above.

We disagree about the spatial pattern being better illustrated by “plotting the ratio of local SWE v/s the averaged value”.

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..

Author reply: I respectfully disagree. While I acknowledge the text can be edited to state “This type of unique data set [...] water resource management”, the point being made is that we have demonstrated a unique capability of providing direct and continuous observations in near-real time of a single snow feature. This approach can be expanded on in future research to larger, more prominent snowdrifts or features. For example, a significant snowdrift in a known watershed at critical locations along several margin points and semi-margin points as well as in the relative center and identify the rate of melt with essentially no maintenance and minimal user operation - ideal for water resource management applications in some regions.

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

Author reply: Thank you. We will adjust for the following submission.

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.

Author reply: Thank you. We will update for the following submission.

Line 432: "the terrestrial set of parameters [...] however, the glaciary 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.

Author reply: Thank you. We will consider this comment and update for the following submission.

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

Author reply: We stand by this statement, however, will clarify the text.