

Note to the editor: Comment on tc-2021-163

There are 4 referees comment on "Review article: Performance assessment of electromagnetic wave- based field sensors for SWE monitoring " by Alain Royer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-163>, 2021

Referee 1 RC1 : Craig Smith

Referee 2 RC2: Charles Fierz

Comment CC1: Floriant Appel

Comment CC2 : Yves Choquette (personale communication). The responses to its review was also included. It was sent to me directly by e-mail instead of being submitted by the journal.

We thank all reviewers for the helpful and constructive comments that helped improve the manuscript. Every comment was addressed, and a detailed list of modifications is attached (one document per reviewers).

Note that the title was changed following comments.

The revised article was proofread by an English-speaking scientific translator, but not the responses/explanations to the reviewers.

We also integrated three important references which were missing:

Gugerli, R., Salzmann, N., Huss, M., and Desilets, D.: Continuous and autonomous snow water equivalent measurements by a cosmic ray sensor on an alpine glacier, *The Cryosphere*, 13, 3413–3434, <https://doi.org/10.5194/tc-13-3413-2019>, 2019. (**CRNP** and **Snow core**)

Wallbank, J.R., Cole, S.J., Moore, R.J., Anderson, S.R., Mellor, E.J.: Estimating snow water equivalent using cosmic-ray neutron sensors from the COSMOS-UK network. *Hydrological Processes*, 35:e14048. <https://doi.org/10.1002/hyp.14048>, 2021. (**CRNP**)

Steiner, L., Meindl, M., Fierz, C., and Geiger, A.: An assessment of sub-snow GPS for quantification of snow water equivalent, *The Cryosphere*, 12, 3161–3175, <https://doi.org/10.5194/tc-12-3161-2018>, 2018. (**GNSSr**)

We also added a reference to another Cosmic Ray Neutron probe commercialized by Geonor: COSMIC RAY DETECTOR (CRD), www.Geonor.com, GEONOR Inc., 51 U.S. Highway 206, Suite 201, Branchville, NJ 07822 USA.

Appendix

Moreover, we have also deepened the analysis of the uncertainty of manual in situ measurements by snowpit and snow core with a thorough analysis of the significant literature.

To lighten the text, we have reported in Appendix* the analysis of this manual ground measurements, including a new figure from our archives. This figure well illustrates the

issue of the uncertainty in the measurements with the snow core. We also added a lot of references to estimate a mean accuracy of such reference approach.

* could be an Appendix or a Supplementary material? But the references given in this Appendix are not separated from those of the article.

Alain Royer, on behalf to the co-authors. 27 August 2021

Comment on tc-2021-163

Craig D. Smith (Referee)

Referee comment on "Review article: Performance assessment of electromagnetic wave- based field sensors for SWE monitoring " by Alain Royer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-163-RC1>, 2021

Comments from the reviewer in blue

Answer in black

Text added in black and italic

The review article by Royer et al. provides some insight into the measurement principles, accuracy, and advantages/disadvantages of four instruments that employ electromagnetic waves to measure in situ snow water equivalent (SWE). Instruments reviewed are the Cosmic Ray Neutron Probe (CRNP) (deployed in two configurations: above and below the snowpack), Gamma Ray Monitoring (GMON) sensor, Frequency Modulated Continuous-Wave Radar (FMCW-Radar), and Global Navigation Satellite System (GNSS) receivers. The paper is a combination of reporting results from previous evaluation and intercomparison studies and the reporting of results from intercomparisons at two Canadian research sites: Foret Montmorency and the Site Interdisciplinaire de Recherche en ENvironnement Extérieur (SIRENE). The purpose is to inform the reader of the relative advantages and disadvantages of each of these systems in the context of employing them in operational monitoring networks.

I thought that the paper offered a complementing balance of previous assessment results and associated measurement issues with fresh results to build on the experience. SWE sensor evaluation is complex because advantages and disadvantages are only realized in differing measurement (snowpack) conditions which are impossible to assess at one, or even several, concurrent intercomparison sites. In this way, the paper is both interesting and contributes to the body of knowledge related to automated in situ SWE measurements.

Thank you for your comments

Having said that, there are several deficiencies in the paper that require revision before the paper can be published in TC.

These are incorporated into the attached annotated manuscript, but the major issues are as follow:

1)There are general wording and grammar issues and some terminology inconsistencies that need to be fixed. These are noted in the annotated manuscript. Done.

2)The abstract is quite vague. It would benefit from some additional detail about each of the sensors assessed and perhaps a short overview of the offered recommendations.

Done. More details about the performance/recommendation of each instrument are given. But this makes the summary very long!

3) There are some redundancies and therefore readability to be gained in Sections 3/4/5. For example, by the time we get to Section 4 (in particular, lines 700-702), we shouldn't be reading more about instrument description, such as the field of view of the CRNP, as this should be outlined already in Section 3. I think that Section 4 can be organized a bit better to improve readability, perhaps by converting the bulleted list starting at line 637 to paragraphs using heading names organized by instrument. Section 4 (Strengths and Weaknesses of Instruments) was re-organized to improve readability.

4) Similar to point #2, the Conclusions section contains redundant details, such as the bulleted list starting on line 740, that should only be briefly summarized at this point. Since your pros and cons are a bulleted list in Section 4, summarize these in a short paragraph in Section 5, followed by some brief recommendations to potential users.

Section Conclusion was briefly summarized.

Specific comments from the annotated manuscript.

L49 The techniques that you are about to describe are not an "automation" of a "manual survey" but rather (generally) a point measurement approximation. I would prefer that you stated this as "The automation of SWE measurement networks..."

Done

L100 "experimental setting" Maybe "measurement principle conceptualization" would be a better way to put this.

Done

L138 and 140 I think some of these details should be left out of the figure caption and discussed in the text of the section. The figure caption is quite descriptive, even without the details.

Shortened caption. Details reported in the text

L172. This sentence seems out of place here. I think flow would be better if you moved this someplace else, perhaps at the beginning of Section 2 or the end of Section 1.

Done

L208 "which is generally the case when soil remains frozen" You are right, this is very general, but far from universal. Infiltration into frozen soils is not uncommon, especially for sandy/silty soils and I think this can be a relatively large source of error with these measurement principles, especially during spring freshet and mid-season thaw cycles. I think that this should be addressed with a bit more detail. This also applies to the GMON discussion.

Gray, D.M., Granger, R.J., Dyck, G.E.: Overwinter soil moisture changes, Transactions of the ASAE, 28(2), 443-447, 1985.

Gray D.M., Toth, B., Zhao, L., Pomeroy, J.W., Granger, R.J.: Estimating areal snowmelt infiltration into frozen soils, *Hydrological Processes* 15(16), 3095–3111, 2001.

L256 Thanks for the citation :)

You can see in our results how the intercomparison with the snow scale at Sodankyla showed hysteresis during snow melt, which we felt was an indication of infiltration into the frozen sandy soils at this site. This didn't seem to happen at the site in the Alberta Rockies where the soils had more clay content.

I'm not saying that intra-seasonal change in soil moisture is a universal problem, but I think that it merits a note of caution, and as you (and we) point out, could be an advantageous measurement.

This aspect has been developed and put in a specific paragraph of Section 4 for the 2 sensors CRNP and GMON.

References added.

L333 This sentence doesn't read correctly. Try re-wording.

Done

Over the Arctic, snow cover can generally be characterized by a two-layer snowpack structure, composed of with a dense wind slab layer overlaying less-dense depth hoar (Rutter et al., 2019; Royer et al., 2021). Thus, assumptions can be made on the mean refractive index of each of these layers, thereby allowing bulk SWE to be estimated (Kramer et al., 2021).

L360 This sentence is a bit confusing and should be re-worded.

Description of different retrieval algorithms was presented, including a new reference added (Steiner et al., 2018).

L382 Why not just say "Evaluations of these sensors are available in the literature and a comprehensive review is presented, including estimates and a discussion of their respective accuracies."

Done

L440 I think for flow, this statement should be incorporated where you introduce fig 3 above.

Paragraph restructured.

L447 Was this ice layer able to be included in the manual measurement? If not, this could also explain why GMON is high compared to the manual measurement.

Rewritten sentence.

As this ice layer was not present in snowpits (the amount of water in an ice crust being otherwise difficult to measure), this could possibly explain the difference between GMON and manual measurements.

L469 This wording makes it sound like you stopped monitoring after 120 cm. This would read better as "A maximum snow depth of 120 cm was measured during the season, ..."

Done

L489 It would be helpful to revise this to include the air temperature threshold that you use.

Rewritten sentence.

...wet snow conditions (open black squares, Fig. 4) which correspond to melting periods with measured air temperature above 0° C,...

L493 ok, I see what you are trying to say here, but it comes across as a bit awkward. Suggestion: During the accumulation period, the GMON shows a relatively smooth and consistent evolution in SWE leading to a maximum of 465 mm on 19 April 2018 while the FMCW-Radar time series is more erratic and requires filtering to remove low SWE outliers.

Good suggestion. Rewritten sentence.

L504: “remains always close to in situ observations.” This is a vague term. You should quantify it.

Quantification added. This was indicated in Table 2.

(RMSE compared to snowpit for the GMON and GNSSR are respectively of 34 mm and 32 mm, Table 2)

L558 I think bridging would be considered an error more so than an uncertainty.

I agree

L559 This sentence is a bit awkward and should be re-worked.

Modified sentence

... leading to disconnection of the weighing mechanism of the overlying snowpack and the surrounding snowpack.

L577 “(0 – 1 000 mm) “I assume that this is SWE and not depth, but you should probably indicate this.

Yes :

mm w.e.

L590 can you elaborate on this a bit?

Rewritten sentence.

The uncertainty in wet SWE retrieval could result from approximations in the retrieval algorithm used. For example, the wet snow refractive index varies linearly with LWC, with a slope significantly dependent of the snow density (see appendix in Pomerleau et al., 2020).

L628 “footprint of the sensor” footprint or response area? I think response area is more appropriate since footprint more likely describes the area taken up by the installed instrument, rather than the area that it is sampling.

The definition of the “footprint” was defined:

i.e. taken here in the sense of the area from which emanates the measured radiation having interacted with the snow

L634 and after: I'm wondering if this bulleted paragraph can be organized better, perhaps under individual instrument headings, since you seem to more or less discuss the instruments individually anyways. I think it would make it easier to read, and complement your bulleted list below.

Done

L699 I feel that these details should be covered in the descriptions in above sections and that this section should just be used for listing pros and cons. For example, this should just state that CRNP above snow has a relatively large (give area) response area compared to other sensors.

All the paragraph was restructured

L718 You should convert all currency to CAD or USD for comparison.

In the text, the prices are given in both money : US\$ and in the money of country where the instrument is manufactured, and in the Table 2, they are all put in US\$.

L721 Doesn't this sensor also need ancillary measurements (snow depth)? That should be mentioned here as well.

Yes. Added.

This approach requires to measure the snow depth to be able to retrieve SWE.

L740 I feel that there is too much redundancy between this section of the conclusions and the previous discussion. The paper would have better flow if your conclusions only included a very brief summary and your recommendations.

The conclusion was rewritten.

Table 3 I'm not sure if it's a pro or a con, but one thing that I would like to see documented for these instrument suites are the communications/logging protocols. For example, I know that the CS725 uses SDI12 communication protocols. Some are probably RS482 (as an example). Can they be connected to data loggers or do they require onsite computers?

This technical specification can be found related papers.

Also, you mention in the text that each of these instruments can be operated with DC/Solar power. Stating the max power consumption would also be useful for many readers, considering the objective of informing decisions for network operation.

We added a line given the exact power consumption. Good suggestion.

Comment on tc-2021-163

Charles Fierz, SLF, Davos (Referee)

Referee comment on "Review article: Performance assessment of electromagnetic wave-based field sensors for SWE monitoring" by Alain Royer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-163-RC2>, 2021

Comments from the reviewer in blue

Answer in black

Text added in black and italic

General comments

This paper presents an assessment of methods to measure autonomously and continuously the water equivalent of snow cover. The presented data (one winter each on two sites) are augmented by a wealth of already published data, including own ones. This allows for a thorough analysis of pros and cons of these instruments that are increasingly used worldwide. Furthermore, an emerging, truly low-cost method based on FMCW radar is presented. These two last points form the major strength of the paper while I cannot really call it a 'review'.

Thank you for your comments. This paper first includes a review of all field sensors based on radiation measurements, and then, a large part of the analysis is based on a review of their performance with a comparison between them. We combine in this analysis our own measurements to complement the comparison. Our manuscript thus meets the requirements for a "review paper".

On the other hand, it is sometimes difficult to find out whether the authors speak about their own data or data taken from the literature. This ambiguity is one of the major drawback of this contribution, that also reflects itself in a poor structure of Section 3 (see comments below).

Section 3 has been restructured and the distinction between the literature review and our own experiences has been clarified. See also answer below

Another major problem is the 'classification' of the Cosmic Ray Neutron Probe as an 'electromagnetic wave-based field sensor'. With that respect the authors are required to make necessary corrections throughout the text.

Yes, you right, this is a mistake. Cosmic radiation consists of high-energy charged particles flux, particles that have a mass! They thus cannot be classified as EM. We thus modified the text where appropriate to use the term "radiation-based field sensor" (see further as well).

Furthermore, the author should eliminate quite a number of weaknesses I address below and in the manuscript. This to make the paper less ambiguous and easier to read, thereby increasing its value for the community.

The modifications suggested by the authors were made.

In summary I recommend accepting the paper after the authors addressed the issues below and consider the revisions as suggested in the annotated manuscript.

Specific comments

Title:

In view of my general comments and because CRNP is not based on EM waves, I would suggest to change the title to, "**Performance of autonomous field sensors for continuously monitoring the water equivalent of snow cover (SWE)**"

Thank you for your suggestion, but "autonomous field sensors for continuously monitoring SWE" can include a lot of instruments that are not considered here, such as those based on pressure and load cell sensors (snow pillow), or on snowmelt lysimeters, or on dielectric sensor (e.g. the SNOWPOWER system, commercially available as the Snowpack Analyzer) or acoustic sensors (see Kinar and Pomeroy, 2015). All

these listed sensors do not meet the targeted criteria of ease of installation, or consumption (autonomy), or non-invasive or non-operational. This remark has been added in the introduction

We thus suggest a title more specific to the scope of this paper:

“Performance assessment of radiation-based field sensors for monitoring the water equivalent of snow cover (SWE)”

The cosmic ray neutron probe can be considered as a radiation-based sensor.

Line 25: *‘relatively low cost’*

In Table 3, cost is named as a drawback for at least three instruments! I would also argue it is a matter of appreciation whether one considers USD 10 000.- to be *‘low-cost’*. I would thus propose to drop this argument, at least in the abstract, the introduction, and the conclusions.

This is a delicate but important aspect of the comparison of instruments. The instrument’s price is also a very relative argument depending on the purchasers (Hydro-Quebec equips its SWE measurement network with about a hundred GMONs!). This criterion can also influence the choice of decision makers or researchers.

This said, we agree that the term ‘low-cost’ can be more contextualized, therefore we modified the text and Table-3 such that:

Modified text and Table 3:

- the price of the instrument itself, knowing that the cost of the system may vary in cases where additional instruments are required for the SWE measurements. Also, the cost that is associated with on-site maintenance during winter should be considered here, but in our case, the 4 instruments are considered on the same basis, i.e., autonomous, with no need for intervention; - and the possibility of other applications.

The cost criterion is somewhat subjective, which can influence the choice of decision-makers or researchers, depending upon the intended application (e.g., large network, in remote areas, among others) and also on the purchasers.

Line 40: *‘kg m⁻², or in mm’*

You nicely define water equivalent of snow cover as ‘its mass per unit area’. Why then use mm as a unit? I would prefer by much seeing only kg m⁻² throughout the paper.

In theory, I agree with this remark in accordance with the SI unit system (kg m⁻²). However, it is also a common practice to consider SWE in mm (sometimes in cm).

While these units are well defined at the beginning, there is no possible confusion.

Line 50: *‘reliable and automatic instrument alternatives exist’*

This is questionable. At least I could not find corresponding evidences in the references provided.

Sentence changed as:

“The automation of SWE measurement networks is an essential medium-term prospect, especially since reliable and automatic instrument alternatives exist (Dong, 2018; this study).”

Section 4:

Consider shortening the last paragraph (lines 367-376) or moving it to the introduction as it is not further considered in the paper.

Other sensors based on a similar principle are mentioned in each of the four instruments presented (here the GNSS). We think this complements and adds interesting value to the literature review.

Lines 533-534: *‘winter period, uncertainty can be introduced by well-known local SWE spatial variability that can occur at fine scales around the sensors.’*

I would argue this variability is less present in bulk snow density. Thus a comparison of SWE recomputed with regard to one reference snow depth may have been beneficial.

Yes we agree. Snow depth is significantly more variable than bulk density.

However, bulk density can also spatially vary, as showed for example by Rutter et al. (JGR, 119, doi:10.1002/2013JF003017, 2014) in the Arctic.

Line 653: clearly, in your own words, CRNP is NOT EM wave!

Corrected

Comments on structure

Section 3 is quite difficult to follow and not free of ambiguities. I'd suggest the following to improve its readability:

Rename Section 3 as '*Results*' and start with a description of both sites (Sub-section 3.1). Adding a figure showing the location of the instruments at each site as well as a summarizing table would extremely helpful.

Then Section 3.2 would become '*Validation of measurements*' with separate sub-sections on SIRENE and Neige-FM.

3.3 '*Uncertainty of measurements*' would end Section 3.

This section was restructured as suggested.

Comments on terminology

'*snow water equivalent*':

Consider switching to "**water equivalent of snow cover**" as defined by WMO (for example, see WMO 2018): Guide to instruments and methods of observation: Volume II - Measurement of Cryospheric Variables, 2018th ed., edited by: WMO, World Meteorological Organization, Geneva, Switzerland, 52 pp., 2018.)

The SWE acronym is used in the paper to define « **water equivalent of snow cover** »

'*accuracy*':

Consider switching to "**uncertainty**", see the VIM <https://jcg.m.bipm.org/vim/en/index.html> or <https://jcg.m.bipm.org/vim/fr/index.html> 'CNRP' (see line 23):

Corrected. Yes "uncertainty" relates to the characterization of the dispersion of measurements compared to a reference (expressed by the standard deviation or root mean square difference, regression analysis, or statistical distribution of the quantity values from series of measurements, or probability density functions ...), while the accuracy is defined as the closeness of agreement between a measured quantity value and a "true" quantity value (accepted reference value).

Please correct to "**CRNP**" throughout the text.

Corrected

'GNSS' (used 16 times) vs 'GNSSR' (used 34 times):

The distinction between 'GNSS', that is the satellite system, and 'GNSSR', that is the instrument used, is sometimes misleading in the text. Also note that GNSSR can easily be misunderstood as GNSS reflectometry. Thus I'd suggest the following: on lines 24-25 and 67 write, "**Global Navigation Satellite System receiver(s) (GNSSr)**". Using GNSS alone later should then be clear too.

Corrected. Good suggestion!

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2021-163/tc-2021-163-RC2-supplement.pdf>

All annotations were taken into account. Except for (Line numbers refer to the annotated pdf document):

Fig.2 comments: For the sake of clarity, I would separate set-up illustrations (Figs. 2a-c) and pictures of your sites in two Figures. Also, a picture of the CRNP set-up at SIRENE would be great!

These photos are only illustrative. To separate this figure will make the article longer. This maybe would be interesting to add a photo of the CRNP and GMON at SIRENE. Unfortunately, we can't find such a photo (the CRNP and GMON are no longer in operation at SIRENE). Instead we changed the photo (a) where the CRNP is more visible than previous photo, and we add, in insert, a close-up of the probe we had in Sherbrooke, that is exactly the same than the EDF NRC sensor.

L 294 “precision” : Degree of internal agreement among independent measurements made under specific conditions (ISO Publications, ISO 3534-1, Statistics - Vocabulary and symbols - Part1: Probability and general statistical terms, International Organization for Standardization (Geneva, Switzerland), 1993).

L 391 and elsewhere : We give here the station coordinates in order to situate them in the world. Five digits do not appear necessary. The exact locations can be found in the cited references.

Citation: <https://doi.org/10.5194/tc-2021-163-CC1>

Comments by Florian Appel, VISTA GmbH. on behalf of the SnowSense team. Referee comment on "Review article: Performance assessment of electromagnetic wave- based field sensors for SWE monitoring " by Alain Royer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-163-CC1>, 2021

Comments from the reviewer in blue

Answer in black

Text added in black and italic

Line 101 (table)

GNSSR ... Measures also Snow depth and Liquid Water Content
maybe change to:

Measures also Liquid Water Content and gives estimation of Snow Depth and Snow Density

Done

Line 106 (figure)

e) please remove lines from antenna 2 to antenna 1 and the snow surface to antenna 1, there is no interaction between the antennas. Antenna 1 is receiving GNSS signals directly (through atmosphere, antenna 2 is receiving GNSS signals through atmosphere and affected by traveling through the snow. the independent measurement at antenna 1 and antenna 2 are analysed for differences.

Done

Line 351

... one placed under the snow and the other above the snow, both signals that are measured under dry-snow conditions can be compared and SWE derived (Fig. 1e)
Maybe change to:

... one placed under the snow and the other above the snow, carrier phase measurements of both receivers can be compared and SWE derived onboard the measurement hardware (Fig. 1e)

Done

Lines 357f

This relatively recent and novel approach has been validated (Koch et al., 2019; Apple et al., 2020) and is now commercialized by VISTA Remote Sensing in Geosciences GmbH, Munich, Germany (SnowSense©, <https://www.vista-geo.de/en/snowsense/>).

maybe changed to:

This relatively recent and novel approach has been developed and validated (Koch et al., 2019; Henkel, et al. 2018, Appel et al., 2019) and is now commercialized by VISTA Remote Sensing in Geosciences GmbH, Munich, Germany (SnowSense©, www.snowsense.de).

Corrected. Reference to Appel added. Sorry for this omission

Line 773 (Acknowledgment)

GNSSR installation was performed within ESA business development demonstration project SnowSense (<https://business.esa.int/projects/snowsense-dp>), if you want to include ESA in the text

Added

Comment on tc-2021-163

Review from Yves Choquette (CC2)

Yves Choquette

Consultant Électrochimie et Chimie

Formerly at IREQ, Hydro-Quebec

Referee comment on "Review article: Performance assessment of electromagnetic wave- based field sensors for SWE monitoring " by Alain Royer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-163-CC2>, 2021

Comments from the reviewer in blue

Answer in black

Text added in black and italic

L 29 (Abstract). It should be mentioned that this accuracy is determined inside this study and from other papers and not the accuracy reported by the manufacturers. This means distinguishing equipment accuracy (calibration in the manufacturer process) and accuracy of the instrument at the field (considering the field variation of the snowpack). The distinction in the accuracy between the field experiment and the calibration by the manufacturer has been made in the text. (good point).

At the beginning of the Sect.3.4 Accuracy analysis, we added the sentence:

Note that we only consider here the differences between instruments in the field and do not address errors derived from the calibration by the manufacturer.

L438. The GMON is calibrated by the manufacturer. It should be mentioned that the calibration is for the site where the GMON is installed. If no additional information is described for calibration parameters of all sensors, the reader should assume the best parameters set was selected but without describing the way parameters are selected weakens this study. I would like to see more about this subject (site calibration). Oftently, this is a key process to get reliable and accurate data.

We have to distinguish between “calibration”- site issue due to soil moisture (inherent to this sensor and also for the CRNP), and the calibration instrument from the manufacturer, that can be found in references relative to each instrument. In order to avoid any confusion, the term "calibration" was removed when it came to correcting (adjusting) the signal for differences in soil water storage before snowfall or during the winter.

L452. This paper did not talk about terms and semantic but a ice sheet under the GMON is associated to a SWE number and not to an overestimation of the SWE. We do not ascribe crystal ice inside the snow cover to an overestimation so an ice sheet should not too.

When there is ice under the snow, we can talk about “ice water equivalent” and not about “snow water equivalent”. In our case, there was no more snow on the ground! For the total amount of water, there is no overestimation, you are right.

The sentence was corrected:

... leading to a significant GMON overestimation in terms of snow w.e. but not in terms of total water.

L457 (Figure 3). Important to mention how the GMON parameter 9 (soil humidity) was determined because it can explain some of the difference vs manual SWE mm. The GMON was adjusted to take into account the soil moisture prior to snowfall accumulation, but not after, during the winter.

Figure 4 This site is an "easy" site for the site parameters of the GMON because the soil is highly drained meaning a relative constant for the soil humidity so the results should be more accurate than other sites (more organic at surface) for SWE data. This is indeed the case at the Foret-FM site and it was specified.

L597. Table 2 do not reflect the accuracy obtained by HQ. We have sites that are very accurate (less than 5 mm vs SC and SP) and others in the range of 20-30mm.

and

L600 It is true. At HQ, we got better results than 15 mm accuracy for SWE < 300 mm (however not for all sites but a pretty much significative number of sites). We do not have a lot of data for accuracy determination for SWE > 300 mm but the 15% is really in very bad situation where K counts are weaks.

For sites having high K counts level, it is < 15%.

For Table 2, we did not find statistical analysis performed by Hydro-Quebec. But, we specified that:

If SWE reference data and site adjustment process are well done, the GMON is able to report SWE inside an error as low as 5% (Wright, 2011; Choquette et al., 2013; Wright et al., 2013).

L661 "is not continuous" Again, I believe also here that it is a question of definition and semantic. A continuous monitoring for example of temperature is surely defined at a minute frequency but for the SWE data, what frequency is defined as "CONTINUOUS"? Being historically manually done at the best at week frequency, a data reported at each hour or 2 hours could be considered as "CONTINUOUS". Regarding an hydrological model operating 2 to 4 times per day surely deliver "CONTINUOUS" information so again this is a question of semantic.

Note (a) in Table 3: Not at ease with the "discontinuous" term regarding the need (water forecast for hydraulic power generation, model simulation, etc..)

Yes, this point about the time sampling rate has be defined:

- the temporal sampling rate, i.e., whether they were capable of quasi continuous SWE measurement capability, although the notion of continuous SWE measurements is relative to the application, such as for seasonal SWE monitoring, for hydrological model validation or to follow an event of a short winter storm

L665. The drawback (until more R&D investigations that are on going at ULaval) of this approach is the lost of the soil humidity variation information from the top ground surface monitored by the GMON. This soil humidity variation is also helpful in the hydrological model for off-snow water forecast.

Yes, but the objective here is to be able to monitor the SWE. To strengthen the gamma ray emission by such a process is an interesting hint that can counteract this limitation and make the use of GMON more universal (e.g. measurement on glacier).

L708 "...footprint (2-3 m)" How this number was determined?

Table 3 “footprint GMON” Not true, see the paper of Ducharme. Depend also on the snow layer thickness.

The foot print was defined by the diameter sensed within the IFOV: $H \times 2 \times \tan(\text{IFOV}/2)$, where H is the height of the sensor above the snow surface. But, attenuated gamma rays that emanate from the snowpack can effectively come from further away than this geometrical footprint by scattering. This was adjusted.

The size of the area effectively monitored by the GMON (“footprint”) extends to 10 m from the detector when there is no snow or water on the ground (Ducharme et al., 2015). The size of the sensed area exponentially decreases with increasing SWE and is estimated to be of the order of 5 m radius (50 – 100 m²) for 150-300 mm w.e. (Martin et al., 2008; Ducharme et al., 2015). This relatively large foot print is an advantage of this sensor.

L710 “uncertainty of instrument”. It could be also semantic but a systematic error (called bias) comes from an under or over estimation of the soil moisture. So, at one site, it is not considered as "uncertainty" but more a "bias error". However, when more than one GMON site is considered (ex. in a hydrological model), then an uncertainty can be generated, which is a "spatial" uncertainty.

Corrected

Yet, it needs systematic site adjustment for soil moisture-induced error, which can increase the bias of measurements.

L710 “particularly at the end of the season “ Which one? Fall? or Spring? From our knowledge, a "typical fall" will generate a soil saturated. If the fall is highly dried and winter comes early with high freeze, soil humidity will not be saturated but with snowpack accumulation, the soil will get saturated.

We spoke here about the winter:

particularly at the end of the winter when the soil becomes potentially saturated during snowmelt.

L724 A brief snapshot about cost. First, authors should consider reporting the same base money (US\$ or Euros or CND\$). Second, the most significant data is the cost of ownership and maintenance because, by exemple, a cheaper instrument which need many maintenance will cost more after some time in operation especially for distant remote area which are oftenly the case for snow monitoring stations.

The price was noted in the money form the manufacturer in the text, and has been translated in USD in Table 3.

The cost of the maintenance has been mentioned.

L738 Rio Tinto did already use the GMON since more than 3 years for its entire meteo network.

Added

L754. Total cost should be considered, and also, the cost of the sensor vs the total cost of establishment of a meteo station. Ex. CNRP needs other equipement to estimate the SWE so increase the real total cost.

This was noted.

L768. This is well done with this paper but authors have already a major pro for the GNSSR when they write "strongest potential for a wide range of applications". I think that much more parameters must be included in the analysis before making a conclusion. The free maintenance (the real free maintenance) is of huge of importance for operational network and nothing is mentionned. The footprint and the area where the equipment could be deployed is another real big issue. Ex. it looks like GNSSR could not be deployed in forest area and this is a major drawback for south watershed in Quebec. So, to be more interesting for decision makers who want establishing a meteo network, this paper should be more explicative.

The potential of GNSSR mentioned here concerns its lightness and compactness for snow monitoring in the Arctic, as example.

The issue of the footprint of the GMON was detailed (see above)

As the GNSSR signals are normalized between the direct and snow-attenuated radiation in the processing, the effect of the forest is not a major drawback.

All instruments analyzed in this study are free of maintenance, they were thus considered as such.

Table 2 I didn't see how the calibration of the site (ex. for the GMON) was done which can influence the SWE data so this is a miss in this report. Same comment for other equipments.

The principle of the "calibration" for the GMON and the CRNP needed for soil moisture-induced bias correction was mentioned in the text. The detailed procedures on how to include this correction in processing data is well explained in given references (see Choquette et al., 2013, or Desilets et al., 2017).

Table 2 I think I did not see this in the text which is very important to mention: my understand is that the accuracy was evaluated at the highest SWE, which is just fine, but should be more explicit in the text.

The accuracy derived for the regression analysis was calculated for the whole range of SWE data