Dear Referee

Thanks for helping us in improving the manuscript.

We will consider all your comments in the revised version of the manuscript. The title, as you suggest and in agreement also with the second Referee, will be: Estimating snow water equivalent on remote and glacierized high elevation areas (Forni Glacier, Italy).

We will therefore modify the "Method" section in order to better explain the approach applied for estimating the site-average-new-snow density ($\rho_{new\ snow}$) and the sonic-ranger-depth-derived *SWE*. In the previous version of the manuscript, we performed the analyses using the mean new snow density (140 kg m$^{-3}$) that was obtained by Senese et al. (2012) considering the 2005-2009 dataset, and then we discussed to what degree this value is able to describe the data of the following years. In the revised version of the manuscript, we will start with updating the site average new snow density estimation exploiting all the available dataset and we will perform all the subsequent analyses using this value.

The text concerning this issue in "Method" section will therefore be:

In addition to the measures performed by means of the AWSs, since winter 2005-2006, personnel from the Centro Nivo-Meteorologico (namely CNM Bormio-ARPA Lombardia) of the Lombardy Regional Agency for the Environment have been carrying out periodic snow pits (performed according to the AINEVA protocol, see also Senese et al., 2014) in order to estimate snow depth and *SWE*. In particular, for each snow pit *j*, the thickness ($h_{ij}$) and the density ($\rho_{ij}$) of each snow layer (*i*) are measured for determining its snow water equivalent then the total $SWE_{snow\text{-}pit\text{-}j}$ of the whole snow cover (*n* layers) is obtained:

$$SWE_{snow-pit-j} = \sum_{i=1}^{n} h_{ij} \cdot \frac{\rho_{ij}}{\rho_{water}} \quad (1)$$

where $\rho_{water}$ is density of water. As stated in a previous study (Senese et al., 2014), the date when the snow pit is dug is very important for not underestimating the actual accumulation. For this reason, we considered only the snow pits carried out before the beginning of snow ablation. In fact, whenever ablation occurs, successive *SWE* values derived from snow pits show a decreasing trend (i.e. they are affected by mass losses).

The snow pit *SWE* data were then used, together with the corresponding total new snow derived from sonic ranger measures, to estimate the site average $\rho_{new\ snow}$, in order to update the value of 140 kg m$^{-3}$ that was found in a previous research on data of the same site covering the 2005-2009 period (Senese et al., 2012a). We need updating $\rho_{new\ snow}$ as it is the key variable for estimating *SWE* from sonic ranger new snow data. Specifically, for each snow pit *j*, the corresponding total new snow was first determined by:

$$\Delta h_{snow-pit-j} = \sum_{t=1}^{m} \left(\Delta h_{tj}\right) \quad (2)$$

where *m* is the total number of days with snowfall in the period corresponding to snow pit *j* and $\Delta h_{tj}$ corresponds to the depth of new snow of day *t*. In particular, we considered the hourly snow depth values recorded by the sonic ranger in a day and we calculated the difference between the last and the first reading. Whenever this difference is positive (at least 1 cm), it corresponds to a new snowfall. All data are subject to a strict control to avoid under- or over-measurements, to remove outliers and non-sense values, and to filter possible noises. $\sum_{t=1}^{m}(\Delta h_{tj})$ is therefore the total new snow measured by the Campbell SR50 from the beginning of the accumulation period to the date of the snow pit survey. The average site $\rho_{new\ snow}$ was then determined as:

$$\rho_{new\ snow} = \frac{\sum_{j=1}^{k} SWE_{snow-pit-j}}{\sum_{j=1}^{k}(\Delta h_{snow-pit-j})} \quad (3)$$

where *j* identifies a given snow pit and the corresponding total new snow and the sum extends over all *k* available snow pits. Instead of a mere average of $\rho_{new\ snow}$ values obtained from individual snow pit surveys, this relation gives more weight to snow pits with a higher $SWE_{snow\text{-}pit}$ amount.

The *SWE* of each day (*t*) was then estimated by:

$$SWE_{SR-t} = \begin{cases} \Delta h_t \frac{\rho_{new\ snow}}{\rho_{water}} & if\ \Delta h_t \geq 1\ cm \\ 0 \quad if\ \Delta h_t < 1\ cm \end{cases} \quad (4)$$

We will also apply the leave-one-out cross-validation (LOOCV, a particular case of leave-p-out cross-validation with p = 1) to ensure independence between the data we use to estimate $\rho_{new\ snow}$ and the data we use to assess the corresponding estimation error. Specifically, we will apply Eq. (3) (see answer above) once for each snow pit (*j*), using all other snow pits in the relation ($\rho_{new\ snow}$ LOOCV) and using the selected snow pit as a single-item test ($\rho_{new\ snow}$ from snow pit *j*). In this way, we will avoid dependence between calibration and validation dataset in assessing the new snow density.

The results will give evidence that the standard deviation of the differences between the $\rho_{new\ snow}$ LOOCV values and the corresponding single-item test values ($\rho_{new\ snow}$ from snow pit $j$) is 18 kg $m^{-3}$. The error of the average value of $\rho_{new\ snow}$ will then be estimated dividing this standard deviation for the square root of the number of the considered snow pits. We will show that it is 6 kg $m^{-3}$. We will finally show that the new and the old estimates of $\rho_{new\ snow}$ (149 and 140 kg $m^{-3}$, respectively) do not have a statistical significant difference.

We will also discuss this issue focusing on the snow pit layers (see Eq. (1) – answer above).

Finally, before uploading the reviewed manuscript, as suggested by the second Referee, the standard of English spelling and grammar will be improved by a professional, mother-tongue consultant.

Point-by-point answers to your comments follow.

*Discuss the potential errors in individual events and how this impacts your peak estimation. A large event (big increase in snow depth) that has a large deviation from the mean density will result in larger errors (e.g. a heavy wet snowfall). What is the potential for this to occur at this site? Add a discussion about missing data as this is the greatest threat to failure of the technique. Can you do gap filling with photographed snow stakes? Would you recommend redundant sensors? More specific comments are listed below.*

We will address these issues in the "Discussion" section of the new version of the manuscript.
As far as large events are concerned, they are rather rare at the studied site: only 3 days in the 11-year period covered by the data recorded more than 40 cm of new snow (the number of days decreases to 1 if the threshold increases to 50 cm). More in detail, we have the following distribution of new snow: 382 days with values between 1 and 10 cm, 95 days with values between 10 and 20 cm, 33 days with values between 20 and 30 cm, 11 days with values between 30 and 40 cm. Beside investigating the distribution of new snow values, we checked also if the days in the different new snow intervals have significantly different average temperatures. We did not find any signal.
We agree that missing data is a relevant issue. The introduction of the second sonic ranger (Sommer USH8) at the end of the snow season 2013-2014 was an attempt to reduce the impact of this problem. The second sonic ranger, however, was still in its process of testing in the last years of the period investigated within this paper (we e.g. changed the sensor model). We are confident that in the next years it can help reducing the problem of missing data. Redundant sensors are indeed highly recommended.
The four stakes installed at the corners of the snow pillow at the beginning of the 2014-2015 snow season were another idea to have more data. Unfortunately, they were broken almost immediately after the beginning of the snow accumulation period. They can be another way to face the problem of missing data, provided we will find out how to avoid they will break during the winter season.
Finally, we will add in the revised version of the paper some comments about the relevance of the single snow pit layers to the total snow pit SWE.

*Note that the units for SWE should be reported in mm water equivalent (w.e.) or kg m-2 and not m w.e. . Snow depths should also be reported in cm and not m. It would also be useful if you used an abbreviation for "sonic ranger-derived SWE" such as $SWE_{SR}$ and use this throughout the paper.*

We will modify all the manuscript accordingly.

***Abstract***

*Page 1, Line 15: "…on the Forni Glacier in Italy."*

We will modify the sentence accordingly.

*Lines 19-20: This sentence misses the mark. From what I have read, you are not really assessing the mean value of new snow density…this was done elsewhere. I think you miss explicitly stating the aim of the analysis and the value of this paper. You should state here that you are using mean new snowfall density and automated depth measurements to estimate the SWE of new snowfall and accumulating to estimate peak SWE and evaluating against other methods.*

We will modify the sentence accordingly.

*Line 21: "rather good" is a vague and subjective description of the estimation. Avoid this and/or quantify the estimation.*

We have calculate the RMSE between SWE derived from SR50 sonic ranger and measured by means of snow pillow and we have found a RMSE of 45 mm w.e. We will add this information in the abstract accordingly.

***Introduction***

*Page 2, Line 37: "…often only snowfall measurements are available…"*

We will modify the sentence accordingly.

*Line 38: "assess" should be "calculate" and "depending" should be "depends". Fresh snow density also depends on surface conditions, correct?*

We will modify the sentences accordingly. We will add "surface conditions" in addition to "atmospheric conditions".

*Line 50: CryoNet is more of a network attached to the GCW initiative, rather than a "project"*

We will modify the sentence accordingly.

*Line 71: "detail" not "details"*

We will modify the sentence accordingly.

*Page 3, Lines 89-93: For item ii, I'm not sure that you are "defining the reliability of…" because you don't really have a solid reference (more on that later) to be able to do that. I would rather you said that you were "assessing the capability of…". You also use the term "obtained SWE" here…now I assume that you are referring to the derived SWE from the depth measurements so you need to be more specific here.*

We will modify the sentence accordingly: "ii) assess the capability to obtain SWE values from the depth measurements".

***Data and Methods***

*Page 3, Line 100: You refer to "These sensors" but you should rather say "These measurements were made at the two stations…" since not all of the measurements were made with sensors.*

We will modify the sentence accordingly.

*Line 106: Was the T/RH sensor shielded? I assume yes.*

The thermo-hygrometer is with shield. We will modify the sentence accordingly adding "shielded".

*Page 4, Line 110: You should cite Beaumont (1965) here when introducing the snow pillow methods. Also some grammar issues with the first sentence on this page.*

We will modify the sentence accordingly citing Beaumont (1965) and checking the grammar issues.

*Line 116: I think "constrictions" should be "challenges".*

We will modify the sentence accordingly.

*Line 121: "represented" should be "provided" or "supplied". "deep" should be "thorough".*

We will modify the sentences accordingly.

*Line 122: "working" should perhaps be "operation".*

We will modify the sentence accordingly.

*Line 123: "…due to ice flow, etc.) is required before installation…"*

We will modify the sentence accordingly.

*Lines 125-128: Not sure if this paragraph adds anything to the paper*

We will delete this paragraph accordingly.

*Line 130: I think that I know what you mean by "adjust" but you should clarify this.*

We will modify the sentence accordingly in order to better explain what we mean.

*Line 132: What are ring faults?*

The ring faults develop as a series of circular or semicircular fractures with stepwise subsidence, caused by englacial or subglacial meltwater creating voids at the ice-bedrock interface and eventually the collapse of cavity roofs.
We will add the definition in the new version of the manuscript.

*Line 146-147: "…from snow depth acquired by sonic rangers and estimated new snowfall density." Clarify what you mean by the last sentence "In particular…". This is a crucial piece of the methodology and you should describe this better. What time did you use for start/end? Did you do any noise filtering? How do you account for redistribution, settling, and sublimation during the day? (should also be discussed in the Discussion section).*

In order to estimate the "daily positive differences in depth (*Δh*)", we considered the hourly snow depth recorded by the sonic ranger in a day and we calculated the difference between the last and the first reading. Whenever this difference is positive, it corresponds to a possible new snowfall:

$$\Delta h = \begin{cases} reading\ at\ h23 - reading\ at\ h00\ (if\ positive) \\ 0\ (if\ negative) \end{cases}$$

This issue will be better explained in the revised version of the manuscript (see answers to the main comments).

As far as settling is concerned, $\Delta h_{snow-pit-j}$ from eq. 2 (see answers to the main comments) would indeed be higher if $\Delta h_{tj}$ were calculated considering a shorter interval than 24 hours. However, on one side this would not be possible because the sonic ranger data have too high error to consider hourly resolution, and on the other side, new snow is defined by WMO considering a 24-hour period. In our opinion, therefore, we do not need considering settling as new snow defined by WMO does already include the settling that occurs in the period used to measure new snow.

Obviously, our sonic ranger data may be affected by snow transported by wind. The effect that is potentially more relevant is new snow that is recorded by the sonic ranger and then blow away in the following days. It is therefore considered in $\Delta h_{snow-pit-j}$ and not in $SWE_{snow-pit-j}$ which causes an underestimation of $\rho_{new\ snow}$ (see eq. 3 – answers to the main comments). Also snow transported to the measuring site can influence $\rho_{new\ snow}$ even if in this case the effect is lower as it measured both by the sonic ranger and by the snow pit. In this case, the problem may be an overestimation of $\rho_{new\ snow}$ as snow transported by wind has usually higher density than new snow. We considered the problem of the effect of wind on snow cover when we selected the station site over the glacier. Even though sites that are not affected by wind transport do not exist, we are confident that the site we selected has a position that can reasonably lower this issue.

Sublimation would have an effect that is similar to those produced by new snow that is recorded by the sonic ranger and then blow away in the following days. We did not yet investigate it. We underline however that the value we find for the site average new snow (i.e. 149 kg m$^{-3}$) does not seem to suggest an underestimated value.

As regards the quality-check activities, we submitted the dataset to a strict control to avoid under- or over-measurements, to remove outliers and non-sense values, to filter possible noises. We will modify the paragraph in the "Method" section accordingly in order to better explain our approach (see answer to the main comments).

*Page 5, Lines 152-153: What do you mean by "unique date"?*

We will modify the sentence accordingly in order to better clarify the approach (see answer to the main comments).

*Lines 150-153: This is the methodology that isn't clearly described (see note above).*

In the new version of the manuscript, we will modify this part of the "Method" section (following also the suggestions of the second Referee) in order to better clarify our procedure in estimating the density of the new snow ($\rho_{new\ snow}$) used for deriving SWE from snow depth data (see answers to the main comments).

***Results***

*Beside expressing the errors or biases in absolute units, it would be helpful if the relative bias was stated. Same can be said for subsequent sections.*

We will modify the previously showed values accordingly adding the error.

*Lines 159-161: Can you comment here or in the discussion on any potential impacts on the analysis due to the site move.*

The dislocation of the AWSs could influence snow conditions, as there could be a different snow accumulation due to a different radiation input and diverse wind regimes. However, as the distance between the two sites is about 500 m, the difference in elevation is only 44 m and the aspect is very similar, we do not expect a noticeable impact of the site change on snow depth. However, we will add some discussions about the site move in the revised manuscript following also the suggestions of the second Referee.

*Lines 166-169: The intercomparison between the SR50 and the USH8 is interesting but largely irrelevant for this paper. I would omit this. What you could mention is that the two instruments had a correlation of ?? and that from 2015 onwards, redundancy in the snow depth measurement could mean better data for the SWE estimate.*

We will omit the intercomparison and the relative figure accordingly. We will move this part into the “Method” section, specifying the correlation between the datasets from Campbell and Sommer sensors.

*Line 174: What is “a very good agreement”?*

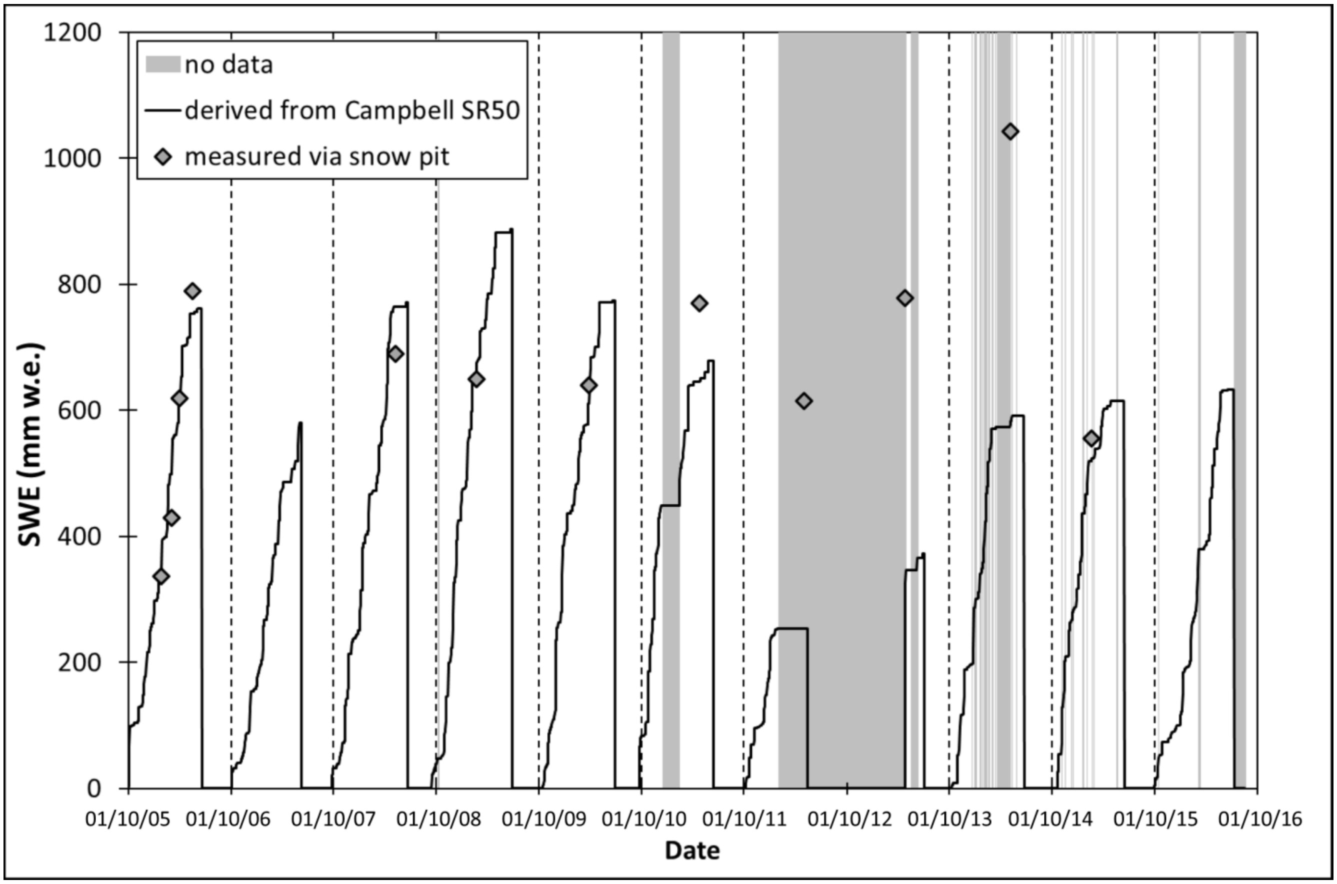
We will add the root mean square error, that is equal to 58 mm w.e.

*Line 180: “elaborate” should be “accumulate”*

We will modify the sentence accordingly.

*Figure 4: it would be very useful if the missing SR50 data could be indicated (e.g. different colour line, etc). This would certainly help with the interpretation of the graph. One might argue that all of the estimated SWE should be set to missing after large gaps in the snow depth data since it is an accumulated result.*

We will modify the figure showing the period without snow depth data. The new version of the figure is:




*Page 6, Line 188: "…thus suggesting a correct working of the sensor." This is more than a sensor, but rather a technique or process. This sentence is awkward and should be reworded.*

We will modify the sentence accordingly.

*Line 192: typo "derided".*

We will modify in "$SWE_{SR}$" in according to your previous comment.

*Line 194: Change "raises" to "increases".*

We will modify the sentence accordingly.

*Lines 196-197: You mention the snow course data using the snow tube and suggest a "large spatial variability". Perhaps you could report the average and standard deviation of this data and in the discussion, relate this variability to the differences you see between the SR50 SWE estimation and the snow pillow and snow pit.*

We will modify accordingly adding the mean value of 165 cm and the standard deviation of 29 cm. In addition, as reported in the following comment, we will discuss this variability in the "Discussion" section.

*Lines 200-201: Oversampling of the tube is a potential error and should be included in the discussion and not in the results. You need to estimate this error and put it into context with the differences between the methods.*

As stated in Johnson et al. (2015), numerous studies have been conducted to determine snow tube accuracy in determining SWE (Freeman, 1965; Work et al., 1965; Beaumont, 1967; Peterson and Brown, 1975; Goodison, 1978; Farnes et al., 1982), the most recent being by Sturm et al. (2010) and Dixon and Boon (2012). The most recent comparison of snow tubes by Dixon and Boon (2012) examined the Standard Federal, Meteorological Service of Canada, and SnowHydro snow tubes. The earlier studies had found that snow tubes tended to over-measure SWE from about 4–11%, whereas the recent studies by Sturm et al. (2010) and Dixon and Boon (2012) found that snow tubes could under-measure or over-measure SWE from -9% to 11%. Even if we consider that our snow tube measures can be affected by errors of about $\pm$ 10%, the high SWE variability is confirmed.

This part will be discussed in the "Discussion" section of the revised manuscript.

***Discussion***

*The start of the Discussion paragraph needs a couple of sentences of introduction to "frame" the problem and set up the following paragraphs.*

We will add a paragraph accordingly in order to better introduce the "Discussion" section. The initial part of the "Discussion" section will therefore state that defining a correct algorithm for modelling *SWE* data is very important to evaluate the water resource deriving from snow melt. We will also highlight that the approach we suggest to derive $SWE_{SR}$ is highly sensitive to the value used for the new snow density, which can vary substantially depending on both atmospheric and surface conditions. In this way, the error of SWE from individual snowfall events could be quite large. Moreover, the technique depends on determining snowfall events, that are estimated from changes in snow depth, and the subsequent calculation and accumulation of $SWE_{SR}$ from those events. Therefore, missed events due to gaps in snow depth data could invalidate the calculation of peak $SWE_{SR}$.

This first part of the "Discussion" section will explain why we analyzed how an incorrect assessment of $\rho_{new\ snow}$ or a gap in snow depth data may affect the estimation of the *SWE*.

*Lines 205-208: The whole success of technique hinges on the approximation of the average new snowfall density but yet this only gets a few sentences in the discussion. This needs to be expanded. When would you expect errors to be greatest? At this site, can you potentially get large snowfalls that have much greater (or lower) densities than the average, which could then bias your accumulated SWE? Any suggestions on how to better estimate new snowfall SWE, perhaps at sites where you don't have snow pits to back-calculate the density.*

We will modify this paragraph accordingly. In particular, for validating our procedure in estimating the new snow density, we will apply the leave-one-out cross-validation (LOOCV, a particular case of leave-p-out cross-validation with p = 1) in order to assess both the error of the estimation of the average site $\rho_{new\ snow}$ and the error we perform if we estimate $\rho_{new\ snow}$ of each single snow pit by means of our approach.

In addition, we will investigate the *SWE* sensitivity to changes in $\rho_{new\ snow}$: we will calculate $SWE_{SR}$ using different values of new snow density ranging from 100 to 200 kg m$^{-3}$ with a step of 25 kg m$^{-3}$.

We will also add to the "Discussion" section some new information on the occurrence of outliers in our data set (the highest new snow value is slightly higher than 40 cm) and on the distribution of new snow. We will then underline that we do not have a significant link between new snow and corresponding daily average temperature that could e.g. indicate that higher new snow values are more frequent at the beginning and at the end of the snow season. Finally, we will present a first analysis of the snow pit.

*Line 205: "Once our procedure was verified, we performed…"*

We will modify this paragraph accordingly to the previous comment.

*Line 209: What is a "general good agreement". Quantify this.*

We will modify the sentence accordingly from "Beside to a general good agreement between the measures performed with the different sensors, there are also some problems." to "As regards the instrumentations, we could assure the correct working of all sensors. Nevertheless, we found some issues related to the derived snow information".

*Line 214: "not constant" should be "inconsistent"*

We will modify the sentence accordingly.

*Page 7, Lines 223-231: You reference some issues with the snow pillow and then state that your snow pillow only seems to be working with depths greater that 50cm. Can you relate any of the referenced potential issues to the errors that you are seeing at the site? I'm not convinced that your issues are related to those referenced so this discussion needs to be stronger.*

The results from the snow pillow are difficult to explain as this sensor has been working for only two winter seasons and then we are still in the process of testing. Analyzing data of the next years will allow a more robust interpretation. However, we have searched for a possible explanation of this over-weighing accordingly and perhaps this error may be due to the configuration of the snow pillow.

*Lines 232-246: This paragraph is more of a justification for using this technique and is more appropriate for the introduction than the discussion. The sentences (Lines 246-250) about the snow pillow is relevant, however. The subsequent discussion about the snow pit should have its own paragraph.*

We will modify this paragraph accordingly, moving part of it into the "Introduction" section. In addition, we will discuss about the snow pit measurements in a separate paragraph.

*Line 248-249: You should refer to Fig 5 and discuss this error in context with what you are seeing at the site. This should be combined with the snow pillow discussion. Also, is there a reference for this statement about minimum snow pillow measurements?*

We will modify this section as described in the previous comment.

*Line 252: What do you mean by "whole glacier accumulation amount"?*

We will modify the sentence accordingly from "whole glacier accumulation amount" to "total snow accumulation amount".

*Line 253: "…the peak cumulative SWE…" Same applies for the next sentence.*

We will modify the sentences accordingly.

*Line 257: Typo "closed" should be "close"*

We will modify the sentence accordingly.

*Lines 259-261: The two sentences beginning with "Finally…" seem out of place or awkward. Please re-organize these.*

We will modify the sentences accordingly from "Finally, with data acquired by the SR50 sonic ranger a correct curve of SWE was derived. The unique issue is represented by the definition of the beginning of the accumulation period, but this can be overcome using albedo data." to "In spite of all these issues, the SR50 sonic ranger features the unique limit represented by the definition of the beginning of the accumulation period, but this can be overcome using albedo data.".

*Page 8, Lines 261-263: The discussion about the SR50/USH8 is largely unsubstantiated and should be omitted.*

We will delete the discussion regarding the USH8 sonic ranger accordingly.

*I would like to see me recommendations in the discussion about how to make this technique better and how to adapt it for other remote sites. Perhaps make some recommendations on further testing, possibly to take advantage of other data collected during SPICE.*

We will modify the "Discussion" section accordingly, following all your previous suggestions and comments.

***Conclusions***

*Line 267: Was the acronym SPICE not defined earlier in the paper? If it was, it doesn't need to be defined here.*

We will delete the definition of the acronym accordingly.

*Line 269: The sentence beginning with "This has allowed…" needs some commas to read correctly.*

We will modify the sentence accordingly.

*Line 270: "…SWE values using a fresh snow density…"*

We will modify the sentence accordingly.

*Lines 270-273: I have some problems with the statement "The results achieved…". You only tested two sensors for measuring snow depth so I wouldn't go as far as saying that the SR50 is the "most suitable device"…perhaps it is but you don't have enough information to substantiate this. You present a technique for estimating peak SWE on a glacier and you compare this technique against another technique (e.g. the snow pillow) but you don't really have a true reference so I'm not sure you can say that it is "most suitable". Rather, I would reference your relative error and despite the issues, suggest that the technique is "suitable" for estimating SWE.*

We will modify the sentences accordingly from "The results achieved during the SPICE experiment support our procedure for deriving SWE values and the applied fresh snow density of 140 kg $m^{-3}$ (Senese et al., 2014), and suggest that, once $\rho_{fresh}$ snow is known, the SR50 sonic ranger can be considered the most suitable device on a glacier to record snowfall events and to measure snow depth values in order to derive the point SWE." To "We found that the mean new snow density changes based on the considered period: 140 kg $m^{-3}$ in 2005-2009 (Senese et al., 2014) and 149 kg $m^{-3}$ in 2005-2015. The difference is however not statistically significant. Once $\rho_{fresh\ snow}$ is known, the SR50 sonic ranger can be considered a suitable device on a glacier to record snowfall events and to measure snow depth values in order to derive *SWE* values."

*Line 274: What are the relative variations as a result of the density change?*

We will modify the sentence accordingly adding the relative variation: "In order to evaluate the effects and impacts of an incorrect $\rho_{new\ snow}$ value in the derived SWE amount, we found that a change in density of ±25 kg m-3 causes a mean variation of 17% of the mean total cumulative SWE considering all hydrological years."

*Instead of ending the conclusions with a comment about the error, perhaps end with a comment on the applicability to other remote sites.*

We will add accordingly: "The generally used sensors (e.g. heated tipping bucket rain gauge, heated weighing gauge, or disdrometer) can provide more accurate measurements compared to the ones installed at the Forni Glacier. The problem is that in remote areas like a glacier at a high alpine site it is very difficult to install and maintain them as the main constrictions concern i) the power to be supplied to instruments that is represented only by solar panels and lead-gel batteries, and ii) glacier dynamic, snow flux and differential snow/ice ablation that can compromise the stability of the instrument structure. Therefore, for our limited experience in such remote areas a sonic ranger could represents a useful approach for estimating SWE since it does not require expert personnel or it does not depend on the date of the survey (such as manual techniques like snow pit and snow weighting tube), it is not subject to glacier dynamics, snow flux or differential ablation (such as graduated rods installed close to an automated camera and snow pillow), and the required power is not so high (such as heated tipping bucket rain gauge). The average new snow density has however to be known either by means of snow pit measures or by the availability of information from similar sites in the same geographic area."

We very much appreciate the time and effort you put into the comments.

Sincerely,

Antonella Senese and Co-author