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Interactive comment on "Dynamics of ionic species in Svalbard annual snow: the effects of rain event and melting" *by* Elena Barbaro et al.

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General comments

The purpose of this study is to investigate the effects of the rain and snowmelt on ionic species in Svalbard snow. The authors carried out intensive snow-pit observations and snow sampling for about two months on a glacier in Svalbard. They present valuable data obtained from daily snow pit sampling, which requires substantial effort. However, I'm not convinced if their observations, sampling and data analyses were performed adequately to reach their goal. This might be partly due to many typographical/ grammatical errors and lack of detailed description, which make some parts of the manuscript hard to understand.

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A: We completely the entire manuscript to correct the typographical/ grammatical errors. We clarify parts of the manuscript as suggest by both referees.

R:The manuscript gives only qualitative information on different behaviours of biogenic and sea spray ions and elution sequence of ions, without a firm basis. Without analysing how much melt (and melt/rainwater percolation) caused how much elution of ions, the manuscript doesn't add much to the previous studies.

A: The statistical approach used in the manuscript has the main aim to define quantitatively evaluate the effect of specific events (rain on snow and melting periods) in the elution sequence of ions. This is the first time that a robust multi-layer statistical approach is used, allowing to define if the changes occurred to the ionic composition and give an overview of a possible elution sequence. The results highlight that the elution sequence is strongly dependent by different parameter (most likely linked to the snow physic) and a unique elution sequence could be difficult to define.

R: Such quantitative analysis needs calculation of mass balances of H2O and ions in each layer of the snowpack, as was done by e.g. Suzuki (1982, https://www.jstage.jst.go.jp/article/rikusui1931/43/2/43_2_102/_article/-char/en). But the manuscript gives no quantitative information about the amount of fresh snow deposited each day and amount of ions contained in it, nor how much ablation (melting etc.) took place each day. Although the data are unique and important, I don't think the manuscript in its current form provides enough new insight on the effects of the rain and snowmelt for publication in the Cryosphere.

A: Fresh snow deposited were evaluated each day and for this reason and present in the section "Snow pit depth correction". The accumulation data, different compare the snow deposition, were obtained by daily measurements of the height of the eight plastic poles compared to the snow surface. A mean value of these measurements was calculated to obtain the average daily loss or gain of snow. The average daily standard deviation of the accumulation measured form the six poles was 3.5 cm. We

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must note that we are evaluating the accumulation over the snow pack after a snow fall instead of total snow fall deposition (instrumental measured), since part of the snow fall can be removed by wind blow.

Specific comments

R: Lines 67-69. I couldn't understand this sentence. I don't understand the definition of the seasonal snow layer. I don't think we usually call the snow layer accumulated above the equilibrium line "seasonal snow layer". What do you mean by "ground"? Line 69. Does "seasonal snow layer" mean "seasonal snow cover"?

A: We clarified the sentence as follows: "The seasonal snow layer can be defined as the snow accumulated and present on the ground (including glacier surface) during the year and melts during the summer."

R: Line 77. I think temperature, as well as temperature gradient, is important.

A: As suggested by referee#2, we modified as follows: "Snow metamorphism is defined as the change of macrophysical snow properties, such as density, grain size, and shape, and it is a function of temperature, as well as temperature gradient within the snowpack (Colbeck, 1982)"

R: Line 111. I don't think that snow on a glacier can be called "seasonal snowpack".

A: The snow accumulates during one year on a Svalbard glacier is transformed, above the equilibrium line, into firn (density above 500 kg m-3) during the summer period. The snow firn transition is clear, this is the reason why we can define the seasonal snowpack also above glacier surface. In the glacier below the equilibrium line, the snow deposited along one year is completely removed\melted. In both cases is possible distinguished the seasonal snow pack.

R: Line 118. A reference for the annual precipitation data is necessary here.

A: We obtained the annual precipitation data through the eKlima database (eklima.no).

R: Lines 118-122 Where in Ny-Alesund area were the annual precipitation and temperature measured? I think this should be clarified because these values vary with elevation and may vary due to orographic effects.

A: We agree with the referee, the measurements derived from the NyA AWS are not fully representative of the temperature at 270 m a.s.l. Since no operative weather station is available on the glacier and in particular on the sampling site, we can adopt and correct the temperature of 0.65°C\100 m, a value often used in meteorology. To estimate the temperature in our sampling location, the temperature determined in NyA decreased of 1.76°C. The new dataset obtained will not affect the interpretation since the two "rain on snow" events recorded from January to April are characterized by temperature up to 3\4°C positive. The text in the manuscript has been revised as also suggested by the other referee: "Air temperature is measured at sea level while our sampling site is located at 270 m a.s.l. Negative differences between our site and Ny-Alesund are expected considering the temperature decreasing of 0.65°C\100 m in adiabatic atmospheric condition (the temperature measured NyA should be correct for 1.76°C). However, this is meteorological approximation in the free atmosphere and this calculation is used in a synoptic condition affect. For this reason, the surrounding orography could lead to over- or under-estimation of value."

R: Line 134. How deep were the plastic poles dug into the glacier ice? Were the poles stable during the whole observation period, not affected by the snowmelt and rain? If the poles moved due to snowmelt or rain, how did the authors correct snow depth readings from the poles?

A: The poles were 2.40 meters long and dug of one meter into the snow pack until the hard layers determined at 100 cm depth. In addition the humidity sensor install at 75 cm never measured liquid water content until the end of the experiment (as well the temperature never rise above -3°C) suggesting the stability of this layer where the poles was settled. R: Lines 141-142. What were the daily loss or gain of snow? I think this information is very important, although the manuscript only gives average daily

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standard deviations of accumulation.

A: The daily loss\gain of the snow is simply summarized on the snow elevation in all figures.

R: Lines 144-146. I couldn't understand "Before sampling the snow, surface was scratch". If the surface snow was removed and not collected as a sample, the authors should clarify from what depth (with respect to the snow surface) the samples were taken. I think it is important to collect a sample from the very surface of the snowpack because it is the snow most strongly affected by rain and melting. Furthermore, if new snow deposited after the previous sampling, the newly added amounts of H2O and ions to the snowpack can be measured with this surface sample. This is important to calculate mass balances of H2O and ions.

A: We apologize if the text was not clear enough. What we meant was different as explained in the revised paper: "Before sampling, the snow wall was cleaned, removing the first stratum with a dedicated clean polyethylene shovel in order to prevent a possible contamination by the aluminum shovel."

R: Each day the authors collected only 10 samples from a depth (from the snow surface) which is not written in the manuscript.

A: We reported this sentence in the manuscript: "The snow wall was sampled using polyethylene pre-cleaned tubes with a depth increment of 10 cm so that 10 samples were taken each time."

R: I'm not convinced if this sampling resolution is enough to trace the temporal changes of same snow layers within the snowpack. To trace the temporal changes of same layers, one should collect samples at a higher resolution or collect samples from the same layer defined by visual stratigraphy (please see Suzuki et al., 1982). I think until heavy snowmelt occurs, one can usually trace a same snow layer by visual stratigraphy of a snowpack. As the thickness of each snow layer within a snowpack changes due to

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snow densification even if there is no rain events or fresh snow deposition, the authors might have collected snow from different layers on different days, although each sample was collected from a same depth (with depth correction).

A: The investigation of the same snow strata for two months is not easy since some strata might completely transformed during warm events (see the stratigraphy in figure 2). Both the two strategies suggested by the reviewer have already been adopted leading to similar results. Considering the enormous amount of the snow pit dug we preferred to adopt a constant depth interval for two reasons: 1) use a standardized method that allows to avoid different operation-specific interpretations, and 2) simplify the data comparison among different days. The aim of the paper is not to follow the strata but to investigate the concentration and its changes of the upper 100 cm of the annual snow pack; there are different opinions about the presence of specific elements in particular snow layers and for sure this could be an interesting topic to further investigate.

R: Lines 190-193. What is the elevation of AWIPEV. The air temperature at AWIPEV could be different from that at the sampling site. If the manuscript uses the air temperature data at AWIPEV, the temperature difference should be at least estimated.

A: See reply for comments line 118-122

R: Lines 199-201. If the daily precipitation was measured near the sea level, it could be different from that at the sampling site. Some part of the rain at the meteorological station could have precipitated as snow at the sampling site at 270 m a.s.l.

A: We agree with the referee, but the temperature during the rain events in Ny-Alesund reached the 4°C. Considering the temperature decrease with altitude (0.65°C\100 m), the temperature at the sampling site might be colder of 1.75 °C, this is true also during the melting phase. However this value are used in the free troposphere and might not be representative for site where the orography cannot be excluded and might increase the atmospheric mixing efficiency. Anyway during the rain

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events if the 16th of April the system for snow temperature and LWC was burn due to water infiltration. Temperature data are also available at the Zeppelin station (http://ebas.nilu.no/Pages/Plot.aspx?key=DBCBA03A77D54265868218D4E5E63521). This station is located at 475 m a.s.l. During the 16th of April 2015 temperature reached 0°C.

R: Line 232 gives the impression that visual stratigraphy was observed. But later, Line 288 reads that only hardness was recorded. Was visual stratigraphy observed? Visual stratigraphy is very important to deduce the degree of melt effects.

A: As also suggested by referee#1, we added the Figure 2 in the main manuscript to show the evolution of the snow stratigraphy in the first meter of annual snow.

R: Line 240. Is “close to the melting point” correct? Isn’t this “at the melting point”?

A: Thanks. We correct “at the melting point”

R: Lines 245-261. I couldn’t understand how the statistical analysis was performed to investigate the effects of the rain event and snowmelt. More detailed information on statistical analysis is required.

A: The statistical analyses discussed in the paper are aimed at the identification of significant change of levels due to the melting or the rain effects. In the paper was written that the statistical method used to identify such change of levels is “linear regression fitted using a robust M estimator”. In fact, this is a simply a t-test with the change of level not estimated using the difference of the sample means before and after the event, but using a robust statistical procedure that is “resistant" to the presence of outliers. The choice of this form of robust t-test in place of the tradition t-test was necessary to avoid incorrect conclusions based on anomalous isolated observations. The two academic statisticians who carried out the statistical analyses acknowledge that their description of the statistical methods used in the paper did not suit well the typical audience of the journal. For this reason, the description of the statistical analysis was completely

rewritten adding more details about the employed methodology. We hope that the new description of the statistical methods contains all the elements necessary to evaluate our results.

R: Why was the snowpack divided into three levels for the biogenic ions and four levels for other ions? I couldn't see from Figs. 2 and 3.

A: As also suggested by referee #1, we clarified the main reason of this simplification as follows: "To simplify the huge dataset and to better understand the processes occurred in snow wall, three different concentration levels can be distinguished for biogenic ions (MSA, NO3-, C2 and C5) at 10-20 cm, 30-50 cm and 60-100 cm while four concentration levels can be identified for other ions at 10-40 cm, 50 cm, 60-90 cm and 100 cm. These layers were defined by considering the different ion sources and by evaluating the profiles reported in Fig. 3 and 4."

R: How were p-values calculated and what do they mean?

A: As explained in the revised description of the statistical analyses, the p-values are computed from robust t statistics and they identify whether the average levels of the concentrations change significantly during the week subsequent the melting or rain event.

R: Line 251. How was the concentration data interpreted using the "logarithmic scale"?

A: The logarithm transformation was necessary for the optimality of the statistical analyses. Since the logarithm is a monotonic increasing transformation, then it does not affect the interpretation of the results: that is identification of significant "jumps" in the concentrations after the events.

R: Line 267. Is the data correct? Isn't it 16th April?

A: Thanks, we corrected the mistake.

R: Line 272. Why "but"?

A: We removed “but still”.

R: Line 274. Does Fig.2 mean Fig. 1?

A: Thanks for noticing the wrong figure numbering, now fixed in the revised paper.

R: Lines 275-286. I’m concerned about the LWC measurement after the rain event. Was the measurement carried out adequately at the depths other than 50cm? Meltwater often percolates through a water channel. If this happens, some part of the snowpack contains liquid water, but other parts at the same depth may not. Have the authors checked the horizontal inhomogeneity in liquid water content? As the authors dug a snow pit every day, they should have observed visually how the meltwater percolated and/or affected the snowpack.

A: The sensor was located (and later stuck to snow settles) into the snow and was not possible to carry out a spatial variability with the instrument. Moving the instrument for temperature and LWC measurements meant to dig several snow pit and to modify the original structure of the snow pack. The measure of the spatial variability of the LWC was possible only with more than one device set up on the glacier or using more sensors located at the same depth. The device used was calibrated, also in term of energy consumption to work with the number of sensors described in the manuscript. We agree with the referee that more sensors are necessary to better check the spatial variability of LWC, a parameter that is quite complex to measure in the snow pack. The horizontal homogeneity was checked every day during the snow chemical sampling. The snow pit was characterized by a sampling wall of 1.5 m wide where the main features (i.e. ice layer, melt refrozen strata) were evaluated. The main features showed a good homogeneity, although it must be note that the hard layers (melt and refrozen layer) at 50 cm and 100 cm were used as reference since their homogeneous distribution in the snow pack was investigated. Some thin layers produced by the rain event as well by the temperature rising at the end of experiment (in general <0.5 cm thick) were inhomogeneous. As wrote in the manuscript, we know that preferential percola-

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tion in specific channels is possible, however the LWC device was used to determine a change in the liquid content and not the absolute value as remark in the manuscript.

R: Line 294. How did the authors conclude that this layer consists of the early snow accumulated during late autumn or early winter? Line 124 reads that average snow depth of 2.5 m at the upper part of AAB. What is the average snow depth at the sampling site? If the average snow depth at the sampling site is similar to that at the upper part of AAB (_2.5m), is the layer at 1 m depth really the snow accumulated in late autumn or early winter? Isn't 1m too shallow?

A: Two heavy rain events occur during 2015. The first one on January 22nd and 23rd and the second the 16th of February. Is not always easy to date a snow pit since wind blow, sublimation ect can modify the pristine snow pack. However, the rain events left a clear sign into the snow pack. Most likely the melt refrozen strata at 50 cm depth was caused by the events occurred the 16th of February while the hard strata at 100 cm depth was caused by the events occurred in January. Since only two layers in the snow pack have been detected, we can have a roughly estimation of the snow pack aged. The full depth of the snow pack was 1.65 m. In general the higher accumulation of snow occurred between January and April (Spolaor et al., 2016).

R:Line 298. What does "re-allocated" mean?

A: As suggested by referee, we modified as follows: "the hard layers shifted to the bottom of the snowpack"

R: Line 314. What does "both" mean?

A: We removed "both".

R: Lines 313-320. Although the authors divided the ions into two groups, I'm not convinced about the grouping. Some of the ions have different sources. For example, nitrate could originate from anthropogenic NOx as well as from biogenic activities. Sulphate could originate from DMS emissions (i.e. biogenic activities), sea spray and an-

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thropogenic SOx. Different sources of an ionic species could have different chemical forms, which could result in a difference in elution sequence.

A: We tried to simplify the huge dataset, considering the profile reported in the Figures 3 and 4. We agree with the referee that the sources of these species are not unique and the main goal of this paper was to define the behavior of these species during specific events. The sources of each ion were described but it is not the main focus of this manuscript. Considering that MSA atmospheric concentration in NyA are dominated by biological bloom and sulphate and nitrate showed similar behaviour, the biogenic input was considered as the predominant source.

R: Line 329. Ions are not compounds.

A: As suggested by referee, we substituted “compounds” with “ion species”

R: Lines 328-360. Hard to understand. Descriptions on temporal changes in each ion do not seem to correctly correspond to Figures and Tables.

A: We corrected the numbers of figures and tables.

R: For example, although Lines 328-329 read that MSA showed rather homogeneous concentrations until the end of April, Table S1 shows very different values for three different depth intervals. There are other similar discrepancies between the text and Figures/Tables.

A: We modified the sentence (Lines 328-329) as follows: “The ion species related with the biogenic emission, in particular MSA, NO3-, and C5 had low concentrations without a specific stratification until the end of April.” This is a general consideration about the behaviour of these species until April. We checked the concentration values reported for MSA but we confirmed those reported in manuscript.

R: The color plots in Figs. 2 and 3 are not easy to understand. Line graphs showing values are necessary here.

A: We thank the referee for the suggestion. We try to use line graphs, however the charts in the manuscript represent three variables, time (bottom axes), depth (y axes) and concentration (colors) and inspire from the chart used for the atmospheric particles load.

R: The authors argue that concentrations of biogenic ions increased in upper layers of the snowpack due to the input of primary production. Supporting evidence is necessary to show that the primary production did increase, air masses with high concentrations of biogenic species did reach AAB, and that these ions did penetrate into the snowpack is necessary.

A: To demonstrate the input increase of biogenic species in May, we inserted a reference of Park et al. (2018) where the authors described the "Atmospheric DMS in the Arctic Ocean and Its Relation to Phytoplankton Biomass" also in the spring 2015. I also wonder why sea spray species did not increase. If air masses from the open sea brought marine biogenic species to AAB, they could have also brought sea spray species. The reason to explain the difference is needed.

A: The referee affirms that "If air masses from the open sea brought marine biogenic species to AAB, they could have also brought sea spray species". The transport of ionic species depends to their particle size distribution in the atmospheric aerosol. The sea salt species are usually distributed in the coarse fraction of the aerosol and they have a local source because these coarse particles can deposit close to source. On the contrary, the biogenic species, such as MSA, are usually distributed in the fine fraction (<1 μm) of aerosol and the can undergo a long range atmospheric transport. The main difference between sea salt and biogenic species consists in the different particle distribution and so different source sites (Barbaro et al., 2019; Barbaro et al., 2017; Barbaro et al., 2017b).

R: Why were median concentrations, not averages, used?

A: We prefer to consider the median concentration in order to reduce the influence of

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outliers.

R: The authors discuss increases and decreases. Are they statistically significant?

A: We used the statistical approach described (now with more details) in the section "Statistical Analysis" to establish the significance of the increases and decreases in the concentrations after the melting and rain events.

R: There should be spatial variability in ion concentrations in a snowpack. As concentrations were measured in different snow pits on different days, initial spatial variability should be taken into account to discuss temporal variability. I think authors can do that using the daily data from P1.

A: We adopted a sampling scheme to reduce the spatial variability. Snow pits were dug daily, perpendicular to the ice flow, and every new snow pit was dug approximately 30 cm upstream from the previous one. Although the results clearly show only the effects of melting and rain event, we cannot exclude a possible effect of spatial variability. To confirm this aspect, we added this sentence in the main manuscript and we introduce a new figure S2: "The spatial variability was evaluated by considering the total ion concentrations for each layers in five consecutive days relative to a period (7th – 11th April) without phenomena of snow, rain or wind that can modified the strata composition. The total ionic concentration (Fig. S2) showed the same profile in stable meteorological condition, suggesting that spatial variability was negligible compare to other processes that can modified the snow strata."

R: Line 333. Affected by what?

A: To clarify, we modified the sentence as follows: "The superficial layer (10-20 cm) was the stratum affected by the most evident variations of MSA and its concentrations varied from 16 ng g-1 to 34 ng g-1 in the P1 and P2, respectively, and 136 ng g-1 in the P3."

R: Lines 363-365. As mentioned above, this part needs more detailed explanation.

A: We are sorry for the quality of presentation. We revise the text of the discussion to make clear that the results are based on the methods described in the section entitled “Statistical Analysis”.

R: Line 369. Where were the values 15.1mm and 15mm recorded? The values would be different at the sampling site.

A: We reported the details about the meteorological station in the “Methods” section as follows: “. . . the daily precipitation data were recorded in Ny-Ålesund by the Norwegian Meteorological Institute (station n. 99910) and downloaded through the eKlima database (eklima.no).” From the observatory at on Mt. Zeppelin (http://ebas.nilu.no/Pages/Plot.aspx?key=DBCBA03A77D54265868218D4E5E63521) is possible to obtain temperature data but the precipitation data are not available. We had to refer to the data measured at NyA.

R: Lines 387 and 391. Line 387 reads that C5 and C2 are biogenic organic ions, but line 391 reads that these ions originated from continental pollution sources. This is confusing.

A: The source attribution of these compounds is also debated but we consider these species biogenic because they had the same behavior of MSA. These diacids are directly emitted to the atmosphere by fossil fuel combustion and biomass burning and are produced in the atmosphere by secondary photochemical oxidations of anthropogenic and also natural organic compounds (Kawamura and Sakaguchi, 1999). We agree with referee that this aspect is quite confuse and for this reason we added reference and we modified the manuscript as follows: “Biogenic organic ions (C5 and C2) showed a weakly increase of concentration in the upper part of snowpack, due to deposition of secondary aerosols (Fig. 3). These diacids are directly emitted to the atmosphere by fossil fuel combustion and biomass burning and are produced in the atmosphere by secondary photochemical oxidations of anthropogenic and natural organic compounds (Kawamura and Sakaguchi, 1999). They can be long-range transported in the atmo-

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sphere because they are mainly distributed in the fine aerosol particles (Kawamura et al., 2007)."

R: Line 400. Does "temperate" mean "temperature"?

A: Thanks. We corrected it.

R: Lines 405-406. What does "an improve of concentrations" mean?

A: We modified "improve" with "increase"

R: Lines 407-420. As stated above, I couldn't understand how the elution sequences were derived.

R: Lines 421-434. Unless ion concentrations in the rain and fresh snow deposited on the snow surface are known, I think it is difficult to discuss the causes of the changes in ion concentrations.

A: We partially agree with the referee since we cannot estimate the concentration in the rain during the 16th of April. However, we can estimate the concentration of the fresh snow fall since we sampled them in the upper 10 cm. We are interested in what remains on the surface of the snow pack and not in the concentration in the deposition. This is another interesting topic but beyond the focus of the manuscript. For example, we suggest in the manuscript that iodine deposit mainly during the snow fall events since every time there is a snow accumulation we note an increase of its concentration on the surface layer. In addition, the concentration in the snow deposit on the ground changes with the time, with likely higher concentrations in first snow flakes deposit due to initial higher atmospheric load, and lower in the later atmospheric load. Considering the wind effect is particularly difficult to associate which layers are maintained on the surface of the snow pack and which one is removed, brought to an over estimation of the data. To evaluate this point, a specific study and protocol should be adopted, as suggested by the referee, with a higher temporal resolution. We focus our study only in what's happen in the snow pack and the effect of melting events.

R: Lines 440-442. I couldn't understand English here.

A: We tried to clarify the sentence as follows: ": during the rain event, ion species undergo a vertical remobilization while the effect of ion mobilization produced by the melting period is horizontally distributed and uniform in the whole snow pack."

R: Line 453. Fig.5?

A: We checked all figure numbers and we corrected the mistakes.

R: Line 480. How is "From a millennial scale" relevant to this study?

A: We removed it.

R: Was the density of each snow layer measured? Density measurement is essential for mass balance calculations.

A: The density was measured ones and ranged between 0.35 to 0.45 (using 10 cm resolution) except the fresh snow fall. In our study we are measuring concentration and not flux where the density measurements are necessary. The aim of the paper is to identify a change in concentration for a specific depth (considering the correction adopted for the snow gain\loss) and not the flux for each snow layer, giving indication of the depositional flux for a specific events.

R: Fig. 1. First, figure title, a, b, and c are missing.

A: Sorry, but the figure title is reported in the figure and in the caption.

R: Second, the color plots are not easy to understand. In the uppermost plot, value zero is very important, but difficult to see. In the lowermost plot, the melting point is very important, but difficult to see. Line plots showing changes in values are needed here. Third, the manuscript needs to explain how the lowermost plot was made from the 11 temperature probes.

A: As suggested by referee#2, we modified the figure 1 to better show the difference in

the temperature. As suggested by referee#1, we have also modified the air temperature plot with a line plot.

R: Fig. S1. Visual stratigraphy should be presented. At least new snow and ice layers should be marked.

A: As also suggested by referee#1, we inserted the Figure 2 in the main manuscript with the evolution of the snow stratigraphy in the first meter of annual snow. All data are corrected in function of the accumulation. The layers detected are classified by their resistance of penetration (hardness) using the hand test. The hand test identifies very soft snow with the number 1 while hard snow was described by the number 5.

R: There are also other typos and errors that I haven't pointed out.

A: We checked all manuscript to remove the errors.

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