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The Cryosphere
Discussions

Interactive comment on “A *synthetic* ice core approach to estimate ion relocation in an ice field site experiencing periodical melt; a case study on Lomonosovfonna, Svalbard” *by* C. P. Vega et al.

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To the referees:

The authors truly value the general and specific comments and suggestions made by the referees which have been very helpful when revising the manuscript. We thank the time taken by the referees and we appreciate their detailed review. We agree in most of the comments made by the referees, and we have included their valuable suggestions in the revised version (Supplement) as long as it was possible. Each of our responses has been noted as CV. following each of the referee’s comments.

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Response to anonymous referee #1

Major comments

1. The synthetic ice core comparison to a melt-affected core is creative, but I am not convinced that the method works as intended as an indicator of ion-specific melt-induced elution. Ice core chemistry records are inherently log-normally distributed with large spikes, as shown in Figure 7, which is typical of ice core sites even unaffected by melt. Thus, the subtraction of one “spikey” record from another will inevitably lead to large positive and negative differences (see Figure 8) if the records are slightly offset in time and/or if there is any spatial variability of the chemistry data. Temporal uncertainties of at least +- 0.1 years would be assumed for even the most well dated snowpits/cores. For example, consider a series of snow pits collected at Summit, Greenland where summer melt is extremely rare and robust seasonal changes in chemistry result in a well constrained depth-age scale. If one were to stack a series of 3 or 5 snowpits on top of one another and then subtract those values from a core collected the following year, I would hypothesize that you would see large positive and negative spikes in the difference plot (equivalent to Figure 8) even though there is no meltwater percolation present. This analysis could actually be done quite easily with the publically available data from the GEOSummit monthly snowpits and several ice cores collected at summit over the past 10 years (data available at: https://www.aoncadis.org/project/core_atmospheric_measurements_at_summit_greenland_environmental_observatory.ht I would encourage the authors to conduct this analysis at Summit as a proof-of-concept of the synthetic ice core method. In fact, Summit would be ideal because there was a single melt event in 2012 with abundant on-site observations including hourly weather data. So one could do this analysis in 2004-2011 to assess whether one sees any indication of melt elution and deposition from this method (i.e. large positive or negative difference spikes) when it is known that no melt occurred. If the method passes this initial test, then you could test the method on the 2012 melt event to see if differential elution is observed.

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CV. The referee points to a very interesting test to try the method we propose. Therefore, from the snow pit dataset available online at https://www.aoncadis.org/project/core_atmospheric_measurements_at_summit_greenland_environmental_observatory.ht we selected two snowpits: one sampled on July 1st and the other one sampled in July 27th (after the melt event of July 12 2012). Since no time scales for the snowpits are published we used the density profile and the chemistry record to find an appropriate and realistic match between the series. The comparison of the density profiles is shown in Figure R1. The depth scale of the snowpit sampled in July 27th was adjusted to find a reasonable match with the density profile of the snowpit samples in July 1st. It was necessary to shift the depth profile by 3 cm which is realistic for the interval of 1 month between the sampling and the amount of water that could have melted in mid-July at the site. According to Nghiem et al. (2012), the melt event reported at Greenland on July 12th created a 2 cm ice crust at the surface of the snowpack at Summit, which is evidenced in the density profile in Figure R1 (red line). Therefore, there is no evidence of intense melt at this site that could have generated melt water that percolated further down the snowpack. Therefore, we applied our method by comparing the snowpit sampled in late July with the snowpit sampled in July 1st. Following exactly the same methodology as described in the manuscript (with the exception of using the depth scale instead of the time scale), the ion normalized concentrations in the snowpits were calculated and shown in Figure R2. As referee nr.1 pointed out, the ion record is spiky, as expected for any record of this kind, especially if no melting and percolation occurs. We proceeded then to calculate the difference between the snowpit sampled in July 27th (analogous to the LF-11 core) and the snowpit sampled in July 1st (analogous to the LF-syn unperturbed core). We also filtered out high frequency variations using a lowpass filter (half-year moving average), as we did in the manuscript. The results are shown in Figure R3. As it can be observed in Figure R3, the filtered series do not deviate from the mean value of the concentrations, therefore, no percolation can be inferred from them, as it can be expected to be the case for Summit during the period selected. We therefore consider

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that our method is applicable in Svalbard cores since it does not show false positives for the Greenland snowpits. We did not test our method in any Greenland ice core as suggested by referee nr.1 because, unfortunately, we did not find snowpit data that is already dated or a successive ice core that can be compared with the ice core data available online at the database recommended by the referee.

2. Summit is the ideal case, and even if the synthetic ice core method works at Summit there may be reasons why it would not work at Lomonosovfonna. The largest difficulty in my mind is that the Lomonosovfonna synthetic ice core contains no summer snow. The authors convincingly show in Fig. 6 that summer receives the least precipitation of any season, but it does receive *some*. This leads to a rather confusing situation where the synthetic core has summer values in the time series plots, even though we know that no summer snow was actually collected.

CV. We want to clarify that the synthetic core does contain summer snow as it can be inferred from Table 2 (there is no discontinuity in the time scale vs depth). We believe what we stated in lines 8–13 in page 5060 “This consists in building an unperturbed ice core using only the top meter snowpack record from different ice cores (top meters of the LF-08 and LF-09 cores corresponding to approx. 3/4 of the year previous to the drilling date) and the SP LF-10 snowpit), thus, constructing a snow-firn record covering over early spring, previous winter and fall of each year of the period 2007–2010 (Table 2).” is misleading since we constrained the extent of each synthetic piece to “spring, previous winter and fall” but it should say instead “. . .a snow-firn record covering the previous year until the date of the sampling/drilling of the previously sampled/drilled snowpit/core” as it can be corroborated in Table 2.

Continuation (Ref. 1): This will also contribute to timescale offsets that will lead to large spikes in the difference plots even without melt, as mentioned in #1 above. What is the mean ion concentrations of summer snow? If there is dry deposition or wet deposition from fog or rime then summer concentrations could be high, and their exclusion from the synthetic core would be problematic.

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CV. As previously noted, summer snow has been included (Table 2); therefore, no error due to exclusion exists in our synthetic core. However, we are aware that the summer snow in the synthetic core has most probably experienced melting at each interval, i.e. in the summer of 2009, 2008 and 2007, correspondently. Since each new segment of the synthetic core starts in the spring of each year (Table 2), ions that could have been relocated from the summer layer above are not present in the new segment because they have a completely different segment origin (Table 2). We have now described this in more detail in section 2.3. Seasonal ionic concentrations of the synthetic core are shown in Table R1. Seasonal concentrations calculated in the LF-syn core are in agreement with previously reported snowpit data by Virkkunen et al. (2007) for the summit of Lomonosovfonna (2002–1999), depicting lower values during summer. Most of ions (Cl-, SO42+, Na+, Ca2+ and Mg2+) show higher concentrations during summer in the LF-syn core than in the snowpit from Virkkunen et al. (2007). Seasonal WNaMg values in the LF-syn are much lower than the observed in the period 2002–1999 (Virkkunen et al., 2007) which contains one exceptionally warm year (2001) and long summer (2000).

Continuation (Ref. 1): Spatial variability of the chemistry between the two core sites may also make Lomonosovfonna more problematic than Summit. Table 3 shows that the 5-year smoothed records have low r values, and even several negative correlations for the same ion at different sites. Even the strongest positive correlations ($p<0.05$) have ∼50% of common variability – and these are the 5-year smoothed values. Based on the authors' interpretation, this cannot be due to ion elution since the ions do not elute beyond 1-2 years. Therefore, either their ion elution interpretation is incorrect, or there is large spatial variability that makes the synthetic ice core approach unviable at this site even without melt.

CV. The referee points out an interesting issue to be discussed. As described in the authors' reply to referee nr.1 (question 5), It has been decided not to use the complete LF-08 core due to strong evidence of perturbations on the chemical record due to the

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presence of ice from 6.4 down to 10.6 m (for more details, please refer to the authors' answer to referee nr.1, question 5). This will not change the results of the LF-syn core, however, it changes part of the results previously shown in Table 3. Since the overlapping period between LF-08 and LF-09 is only between 2000–2008, we consider that a Pearson correlation is not the best way to compare how similar the ion signals are between the two cores; therefore, we used a non-parametric rank sum test (Wilcoxon) to evaluate the hypothesis that ion concentrations in the different LF-cores are samples from continuous distributions with equal medians (Table 3). Spatial variability is present at Lomonosovfonna; however, we consider that comparing cores LF-97, LF-08 and LF-09, the results presented in Table 3 (top) show a significant coherence between the ion concentrations in the different cores in the 5-year moving averages annual data. Referee nr.1 points that "...Based on the authors' interpretation, this (low r values, and even several negative correlations for the same ion at different sites) cannot be due to ion elution since the ions do not elute beyond 1-2 years." In the manuscript, the authors suggest that within the period 2007–2010 the relocation of ions shows no evidence of elution beyond of 2 years for most of the ions. However, we do not claim this to be the case for the period 1957–2007, in which it have been reported higher WNaMg values (Virkkunen et al., 2007) than in the LF-syn core. Based on the results in Table 3 we consider that using a 5 year moving average smoothing of the annual ion data is the minimum smoothing average adequate to observe the main chemical and climatic patterns in the core (Figure 2). Moreover, in the manuscript section 3.4, we also discuss the validity of using the LF-syn core to interpret ion relocation by comparing the annual mass loading of the ions in the LF-97 and LF-09 ice core and comparing them with the loadings in the LF-syn core.

3. Perhaps the strongest concern I have with this method is displayed in Figure 8 and Table 4. The authors interpret the positive peaks in Figure 8 (the LF11-synthetic plot) as indicating deposition from meltwater percolation, and negative peaks as indicating meltwater elution. They then calculate "relocation lengths" to determine the relative mobility or elution potential of each ion by finding the distance between positive (de-

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position) and negative (elution) peaks, as shown in Table 4. The implication mass has been eluted to the depth to which is that the measured "relocation length" represents the depth from which mass has been deposited. However, all of the "relocation lengths" are based on the distance between a HIGHER (shallower depth/more recent) deposition peak and a LOWER (deeper depth/more distant) elution peak. This does not make sense to me. Mass should be moving DOWN through the firn with the meltwater, not up. How can this "relocation length" be indicative of elution if the two peaks are not matched? In other words, the deposition peak closer to the surface must have been mobilized from higher up in the snowpack, not deeper down. One difficulty with this problem is highlighted on page 5067, lines 10-18. In this section the authors are describing the elution sequence (most easily eluded to least eluded) based on Figure 8 and Table 4. Their results suggest that nitrate is the least mobile ion. However, this does not agree with previous research at these sites, and the authors reconcile this by selecting a second deposition peak for nitrate that switches it to one of the most mobile ions. Ignoring for a moment that this deposition peak is ABOVE the elution peak and therefore in the wrong direction as described above, there is no a priori reason to select the second deposition peak for nitrate as 'correct' but ignore the second deposition peak for other ions like Cl, Na and Ca. This highlights a fundamental weakness with this method. How does one know *which* deposition peak matches with a particular elution (negative) peak? Certainly it makes no senses to me to pair shallower deposition peaks with deeper elution peaks. But even if deeper deposition peaks were selected, how would one choose which pair is correct? With a longer record there would undoubtedly be several possible elution-deposition peak pairs.

CV. The referee is right, and we have now corrected our interpretation and recalculated the percolation length. Consequently, Section 3.4 has been rewritten and Figure 8 and Table 4, modified accordingly. Basically, we corrected the difference to estimate the percolation length, which has now been calculated over the time series of the LF-11 – LF-syn results in which high frequency processes were filtered out by using a half-year moving average. We then defined four periods based in the minima (elution) and maxima (deposition) observed in the filtered time series. In this way, high frequency variability that is superimposed to the elution-deposition signal is eliminated. Unfortunately, our synthetic ice core is truncated and it is not possible to observe a full elution/deposition cycle. However, we have estimated this, assuming a elution minimum in Period I, which allows to estimate the percolation length during Periods I–II. We consider that these results are relevant to the interpretation of ice core data from Lomonosovfonna since we estimate the temporal scale in which most of ions will most likely be affected by percolation of melt water (in this case, a lower boundary). This has the consequence that all ion information from ice cores interpreted at a resolution higher than 1 year is highly doubted to be unperturbed by percolation of meltwater. Consequently, this effect must be considered when interpreted sub-annual data.

4. The box and whisker plots in Figure 3 and 4 should show 95% confidence intervals to assess whether median concentrations in the snow, ice and firn are truly different as described in the text (see Krzywinski and Altman, 2014; http://www.nature.com/nmeth/journal/v11/n2/full/nmeth.2813.html). The reader is unable to verify the claims in Section 3.3 about differences in concentration between snow, ice and firn without these confidence intervals. Pairs with overlapping 95% confidence intervals cannot reject the null hypothesis that they are the same.

CV. Plots in Figure 3 and Figure 4 have now been modified to show the 95% confidence intervals (notches) according to Krzywinski and Altman (2014). We also corrected Figure 4 in which some ice samples were misclassified as firn-facie instead of as ice-facie; this correction did not change the overall boxplot results shown in Figure 4. According to Krzywinski and Altman (2014), the minimum number of data points needed to use a boxplot is n=5, consequently, we decided not to include fluoride in the boxplots since it has less than 5 samples in some of the stratigraphic units. Following the suggestions in Krzywinski and Altman (2014), we have now noted the number of samples per stratigraphic unit in Figure 3 and Figure 4. Section 3.3 has now been modified accordingly.

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5. The wide range of melt percent (12-70%) values determined through the four methods does not inspire confidence in any of them. I wonder about the use of the annual average 4.4 C/km lapse rate from Pohjola et al (2002) given the work of Gardner et al. (2009) showing that summer lapse rates are higher than that of other seasons, at least in Arctic Canada.

CV. We have follow the work by Pohjola et al. (2002) and Claremar el al. (2012) which have used a constant lapse rate of -0.0044 âĄřC m-1 over different Svalbard glaciers (e.g. Kongsvegen, Nordenskiöldbreen and Vestfonna). We are aware that lapse rates in certain Svalbard sites show inter-seasonal differences, with major differences ranging from +0.01 to -0.01 âĄřC m-1 (Longyearbyen) (Etzelmüller et al., 2011), and less marked differences, ranging from -0.0036 to -0.0053 âĄřC m-1 (Austfonna) and -0.0038 to -0.0051 âĄřC m-1 (Vestfonna) (Jörpeland, 2014); however, to our knowledge, such differences have not been reported for Lomonosovfonna. Considering the lapse rate values reported for Austfonna, Vestfonna (Jörpeland, 2014), and Ny-Ålesund (Wright et al. 2005) as reference, we would not expect larger inter-seasonal differences at Lomonosovfonna. Consequently, we consider that the use of an average annual lapse rate of -0.0044 âĄřC m-1 is adequate in this study.

Continuation (Ref. 1): The authors use the depth-density model in Figure 12 to argue for a 45% MP. However, if one were to use LF-08 instead of LF-09, one would argue for MP>70%.

CV. The referee points out to a very interesting issue. Density profiles of the LF-97, LF-09, LF-11 and the snowpits were obtained by measuring bulk density. On the other hand, the density profile of the LF-08 core was obtained using high resolution dielectric profiling (DEP) with a threshold value of 900 kg m-3, i.e. values above that were set to NaNs. Anomalous elevated density values are a consequence of the interaction between the firn permittivity and conductivity and are recognized to be a problem in the method, leading generally to exaggerated estimates of the density. The deviation of the density profile of the LF-08 compared to the LF-09 and LF-11 cores occurs at an approximate depth of 6.4 m (corresponding to the year 1999.8 in the time scale). By inspecting the photos taken to the LF-08 core, ice is observed from 6.4 m down the core, with absence of firn. This is not registered in the LF-09 core (the LF-11 core only covers until 2004) or in the snowpits obtained by Virkkunen et al. (2007). The photographs of the LF-08 core show that the ice from 6.4 m and further deep until the end of the core at 10.6 m, is anomalously clear, i.e. it is not firn that has been soaked with meltwater. Members of the team that drilled the LF-08 core have noted that there were many crevasses observed at the site during 2008; consequently, the most feasible explanation to this clear ice section of the core is that at the depth of 6.4 m the team drilled into a water-filled crevasse, and therefore, the LF-08 core has not preserved the atmospheric signal from 6.4 m deep. This is also observable in the water stable isotope record which shows a lower seasonal amplitude between 6.4 m and the end of the core, compared with the top section. We consider that the ice found in the LF-08 was not formed due to a simple melt-percolation-refreezing event. If this would have been the origin of the ice column, we should expect to observe such intense event in the other Lomonosovfonna records; however, this signal is absent. Therefore, we have used the LF-08 record only down to 6.4 m in this study. This will not change the results of the LF-syn core since only the top part of the LF-08 was used to construct the synthetic core. Figures 2, 5 and 12 have been re-made accordingly. Table 3 has been also corrected including core LF-08 only down to 6.4 m. Sections 2.2 and 3.1 have been modified accordingly.

6. I have difficulty accepting some of the authors' key conclusions: (a) that "using 5 year moving averages of the ionic data allows having comparable records when different ice cores are used", and "we estimate that the atmospheric ionic signal remains preserved i n recently drilled Lomonosovfonna ie cores at an annual or bi-annual resolution." The negative correlations between 5-year smoothed LF-08 and LF-09 records in Table 2 and the corresponding differences between the 5-year smoothed records in Figure 2 contradict this statement.

CV. Please refer to response to referee nr.1, question 2.

Continuation (Ref. 1): (b) "we reiterate that the different ice core records from Lomonosovfonna all share the same climatic and chemical features. . ." See (a) for the "chemical features" part, and the large differences in density with depth between LF-08 and LF-09 shown in Figure 12 are not consistent with assertion of the same climate conditions.

CV. Please refer to response to referee nr.1, question 5.

Minor comments

Continuation (Ref. 1): P. 5056 line 18: Missing word "it" between "making" and "difficult"

CV. The word "it" has been added.

Continuation (Ref. 1): P. 5057 lines 27-28: I'm unclear about the meaning of "about 25 to 55% of the annual accumulation. . . suffered melt". Does that mean that each year 25 – 50% of the annual snowpack is converted to liquid water and percolated down into the underlying snow/firn? Or does it mean that 25-50% of the annual snowpack is affected by meltwater percolation? I suspect the authors mean the former, but please clarify.

CV. The sentence means that 25 to 50 % of the annual accumulation may melt and percolate into the firn. We have corrected the sentence in the manuscript and now says: "Pohjola et al. (2002) found that at Lomonosovfonna, about 25 % to 55 % of the annual accumulation over the 20th century may melt and percolate into the underlying snow/firn."

Continuation (Ref. 1): P. 5060 line 3: "scaling" should be "weighing"

CV. Corrected.

Continuation (Ref. 1): P. 5060 line 8: "consists in" should be "consists of"

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CV. Corrected.

Continuation (Ref. 1): P. 5060 line 9: “top meter snowpack record from different” should be “top meter OF THE snowpack from different” (insert “of the”; delete “record”)

CV. Corrected.

Continuation (Ref. 1): P. 5061 line 3: “snow as function” should be “snow as a function”

CV. Corrected.

Continuation (Ref. 1): P. 5061 line 6: delete comma after “(2013)”

CV. Corrected.

Continuation (Ref. 1): P. 5062 line 11: “description on” should be “description of”

CV. Corrected.

Continuation (Ref. 1): P. 5062 lines 14-15: Is the automated d18O cycles counting routine published or described in detail anywhere? This is not trivial, especially in a melt-affected site

CV. This routine has been first proposed, described and applied to LF-cores by Pohjola et al. (2002). We basically applied the same routine in the ice cores LF-08, LF-09 and LF-11. Additionally, results obtained for the LF-09 core were cross checked with visual d18O cycles counting and volcanic horizons (Vega et al. 2015b).

Continuation (Ref. 1): P. 5063 line 8: The equation is not necessary – this is generally well known.

CV. We have decided to leave Eq.(1) for the moment to avoid any lack of information about the normalization process. If Eq.(1) is considered to be unnecessary in the text, we are open to remove it in a later state of the manuscript.

Continuation (Ref. 1): P. 5063 line 12: “associated to” should be “associated with”

CV. Corrected.

Continuation (Ref. 1): P. 5063 line 13: “uncertainty on” should be “uncertainty of”

CV. Corrected.

Continuation (Ref. 1): P. 5063 lines 23-25: Are the 95% significance values corrected for the reduced degrees of freedom introduced by the 5-year smoothing? Please clarify and be sure to do this if not already done.

CV. The significance values in Table 3 were not corrected for the reduced degrees of freedom introduced by the 5-year smoothing. We have now corrected the values using a table of critical values for Pearson’s r and a level of significance for a two-tailed test. This has now been described in Table 3. In view that the interpretation of the LF-08 core has now been restricted between 2000–2008, we have decided to apply a non-parametric test to compare cores LF-08 and LF-09 in Table 3 instead of the Pearson coefficients. Therefore, we used the Wilcoxon rank sum test to test the null hypothesis that data in the LF-08 and LF-09 cores are samples from continuous distributions with equal medians, against the alternative that they are not. We have included the results in Table 3.

Continuation (Ref. 1): P. 5065 line 2: “melting is most probably confined to a particular time period” is a truism. Everything is confined to a particular time period – what is the time period? I’m unsure of the point the authors are making here.

CV. The sentence was indeed unclear. We have now changed it to “Positive WNaMg values indicate that melting has most probably occurred”.

Continuation (Ref. 1): P. 5066 line 8: “Having in mind” should be “Keeping in mind”

CV. Corrected.

Continuation (Ref. 1): P. 5066 lines 14-15: I don’t understand the statement “To avoid any bias for the snow accumulated after the spring 2010 and 2011 this period was not

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considered in the normalization of the LF-11 ionic concentrations". This seems like it could be relevant to my point #2 above, but this should be clarified and expanded upon.

CV. As Mentioned in the manuscript text and in Table 2, the top part of the LF-11 core (0–0.6 m w.e.) was not used to construct the LF-syn core, therefore, we did not contemplate this section of the LF-11 core when we normalized the ion data. We have now written in section 3.4: "Since the top part of the LF-11 core (0–0.6 m w.e.) was not used to construct the LF-syn core (Table 2) and to avoid any bias caused for the snow accumulated after the spring 2010 and 2011, this period was not considered in the normalization of the LF-11 ionic concentrations."

Continuation (Ref. 1): P. 5066 line 17: "associated to" should be "associated with"

CV. Corrected.

Continuation (Ref. 1): p. 5067 line 24: "ice layer" should be plural

CV. Corrected.

Continuation (Ref. 1): p. 5068 line 8: What about the minimum in 1982, which is larger than the minimum in 1995 (Fig 10)? I disagree with the statement that "both approaches" show a minimum around 1995.

CV. We agree with the referee; the sentence should have been removed at an earlier stage of the manuscript. The sentence has now been removed since what we want to emphasize in the paragraph is that the production of meltwater at Lomonosovfonna has increased during the 1989–2010 rather than pin-point particular maxima or minima.

Continuation (Ref. 1): P. 5068 lines 11-12: I disagree that figure 11 shows "stable values" of melting. How can the values be "stable" and also have "alternating warm and cold years". The latter description is more appropriate.

CV. The sentence has been corrected accordingly. It now says: "When considering the period between 2007–2010 (Figure 11), the melting shows alternating warm and cold years. . .”

Continuation (Ref. 1): P. 5069 line 17: Avoid using qualitative statements like “moderate melting”. How much melting is a “moderate” amount?

CV. A “moderate melting” could be assumed to be less than 50% of the annual snowpack, but of course this is completely arbitrary. Consequently, we have now removed the term “moderate” from the manuscript. The sentence now says: “However, the calculated MPs using the PDD approach and the snow-energy do not agree with the results obtained by the synthetic core approach which suggest that melting during the 2007–2010 period has not been high (i.e < 50 %) which is also supported by the stratigraphy observed in the LF-09 and LF-11 cores (Figure 9).”

Continuation (Ref. 1): P. 5071 line 18: I disagree that it is a “fact” that “ion relocation took place a moderate depths”. This is your hypothesis, but not a fact.

CV. The referee is right. We have changed the word “fact” for “hypothesis” in the line.

Response to anonymous referee #2

Major comments

1. In the latter part of 3.4 Synthetic ice core, the authors estimated relocation length of ion species from the distance between maximum (deposition) peaks and minimum (elution) peak in Figure 8. When surface melting occurs, melt water washed out the ion species from initial depth to deeper depth. Therefore, elution peaks should appear above deposition peaks. However, elution peaks of all ion species appeared below deposition peak in Figure 8. Consequently, the distances between elution peaks and deposition peaks in Figure 8 are not relocation length. The authors should clarify their discussions about the relocation length. In addition, I could not understand the reason why the secondary relocation length was adopted for only nitrate.

CV. The referee is correct. We have now corrected our interpretation and recalculated the percolation length. Consequently, Section 3.4 has been rewritten and Figure 8 and Table 4, modified accordingly. Basically, we corrected the difference to estimate the percolation length, which has now been calculated over the time series of the LF-11 – LF-syn results in which high frequency processes were filtered out by using a half-year moving average. We then defined four periods based in the minima (elution) and maxima (deposition) observed in the filtered time series. In this way, high frequency variability that is superimposed to the elution-deposition signal is eliminated. Unfortunately, our synthetic ice core is truncated and it is not possible to observe a full elution/deposition cycle. However, we have estimated this, assuming a elution minimum in Period I, which allows to estimate the percolation length during Periods I–II. We consider that these results are relevant to the interpretation of ice core data from Lomonosovfonna since we estimate the temporal scale in which most of ions will most likely be affected by percolation of melt water (in this case, a lower boundary). This has the consequence that all ion information from ice cores interpreted at a resolution higher than 1 year is highly doubted to be unperturbed by percolation of meltwater. Consequently, this effect must be considered when interpreted sub-annual data.

2. The authors estimated amount of melt water product from PDD estimation, snow-energy model and snow densification model in the chapter 3.5. I suggest that the discussion about the comparisons of these values to melt water product estimated by ion relocation behaviors such as melt index should be included in this chapter. The authors mentioned them only in abstract, but did not describe the details of the estimation in text and conclusions. I believe that the evaluation of melt product estimated by the profiles of ion species is important information to develop the studies of “wet ice core”.

CV. We have the following paragraph in section 3.5: “Figure 13 shows a comparison between the LF-11 and the LF-syn ice core depth–time scale. It is clear from Figure 13 that the depth differences between the ice cores (black and grey lines) can be related to a partial melting of the snowpack and refreezing of the meltwater taking place between the 2007–2011 period, as evidenced by the ionic relocation. An estimate of the decrease in depth of the LF-syn ice core by the effects of snowpack melting considering a MP = 20, 30, 48 and 70 % during 2007–2010 as suggested by the firn-densification model, MPs obtained using the PDD approach (using instrumental temperatures) and the snow-energy model is shown in Figure 13. It can be observed that a MP = 20–30 % results in a LF-syn ice core depth-time scale highly similar to the depth-time scale of the LF-11 ice core during the 2007–2010 period, confirming that the MP during that period is coherent with the results of the firn-densification model but not with the PDD results shown in table Table 5." We think that this is what the referee is asking for; otherwise, we would be pleased to get a clarification of what the referee intended that we describe in more detail.

Minor comments

In chapter 3.2 and Table 3, the authors showed that the correlation coefficients in LF-08 and LF-09, and it was different from that in LF-97 and LF-08. The authors should describe discussion about the results, and relations of the result to the subject of this study.

CV. Table 3 has now been modified in view of the LF-08 reduced record used in the manuscript (for more info, please refer to the reply to questions 5 and 2 of referee nr.1). Section 3.2 has been modified accordingly.

Continuation (Ref. 2): I could not read how the authors estimated the snow accumulation by LF-syn core in Figure 6 right. Please describe the details of the estimation.

CV. We have now added the following sentence in section 3.4: "The snow accumulation in the LF-syn core was calculated as the length (in m w.e.) between two LF-syn samples which in average represent 1.2 months of accumulation." In addition, the Y-axis legend in Figure 6 (right) and the X-axis legend in Figure 6 (left) have been added.

Continuation (Ref. 2): Geographical name (Sveagruva and Nordenskioldbreen) should be described in the map of Figure 1.

CV. Figure 1: it has been now corrected to include the geographical locations of Svea-

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gruva and Nordenskiöldbreen.

Continuation (Ref. 2): P.5063 L. 16:"LF-09 and LF-09"-> "LF-08 and LF-09"

CV. This has been corrected accordingly.

Continuation (Ref. 2): Table 3: Description about the bottom table (LF-08 and LF-09 ice cores) should be added.

CV. Description about the bottom table (LF-08 and LF-09 ice cores) has now been included accordingly.

References

Jörpeland, J. 2014. Analysis of Temperature and Precipitation Trends at Svalbard 1989-2010. Självständingt arbete Nr 112. Inst. för Geovetenskaper, Uppsala University.

Krzywinski and Altman. 2015. Visualizing samples with box plots. Nature Methods, 11(2), 119–120.

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Wright et al. 2005. Modelling the impact of superimposed ice on the mass balance of an Arctic glacier under scenarios of future climate change. Ann. Glaciol., 42, 277–283.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C3206/2016/tcd-9-C3206-2016-supplement.pdf

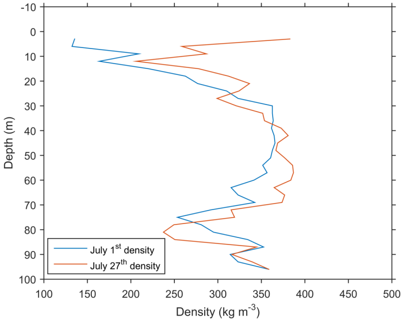

Figure R1. Density profiles in the snowpits sampled at Summit in July 1st and July 27th 2012. The depth scale of the snowpit sampled in July 27th was adjusted to find a reasonable match with the density profile of the snowpit samples in July 1st.

Fig. 1. Fig. R1

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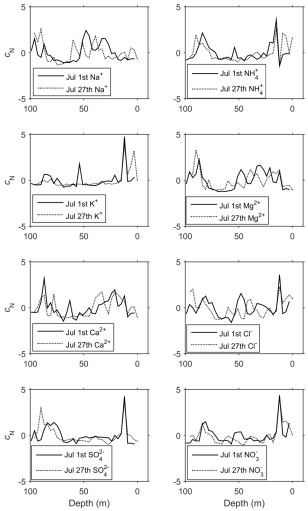

Figure R 1 Comparison between the snowpit samples in July 1st 2012 and the snowpit sampled in July 27th 2012. Ionic concentrations are normalized (c_N).

Fig. 2. Fig. R2

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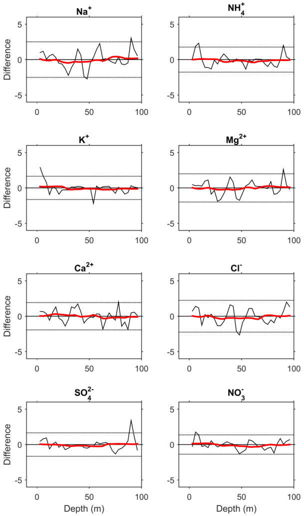

Figure R 1 Difference between the normalized ionic concentrations (c_N) in the snowpit sampled in July 27th 2012 and July 1st 2012. Twice the standard deviation and zero values are also shown. Deposition is a positive excursion and elution zones are negative excursions from the mean in each diagram.

Fig. 3. Fig. R3

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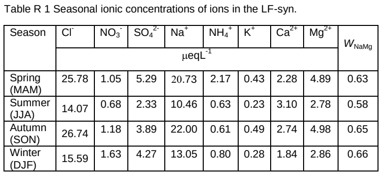

Table R 1 Seasonal ionic concentrations of ions in the LF-syn.

Season	Cl^-	NO_3^-	SO_4^{2-}	Na^+	NH_4^+	K^+	Ca^{2+}	Mg^{2+}	W_{NaMg}
	μeqL^{-1}								
Spring (MAM)	25.78	1.05	5.29	20.73	2.17	0.43	2.28	4.89	0.63
Summer (JJA)	14.07	0.68	2.33	10.46	0.63	0.23	3.10	2.78	0.58
Autumn (SON)	26.74	1.18	3.89	22.00	0.61	0.49	2.74	4.98	0.65
Winter (DJF)	15.59	1.63	4.27	13.05	0.80	0.28	1.84	2.86	0.66

Fig. 4. Table R1