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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.

Anonymous Referee #1

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This manuscript describes a novel (to my knowledge) approach to assess the influence of meltwater percolation and ion elution on ice core glaciochemical records from Lomonosovfonna, Svalbard. A number of previous publications have attempted to quantify the degree of ion-specific chemical redistribution in melt-affected ice core sites, particularly from Svalbard. Here the authors propose a "synthetic" ice core approach whereby the top $\sim$1 m of snow is collected and chemically analyzed in 3 successive springs (2008-2010) prior to the onset of summer surface melt. Thus, each spring's samples span from the spring to the previous fall (spring, winter, fall), and end at the previous summer's melt affected firn/ice. They then "stack" these non-melt-affected columns into a synthetic ice core, and compare the chemistry of the synthetic core to

the chemistry of a melt-affected core collected in 2011 from the same area. Through this method, the authors hope to be able to quantify the amount of ion redistribution caused by melt – a "before" melt vs. "after" melt comparison over 3 years. The authors conclude that acidic ions including sulfate, nitrate and ammonium have the greatest mobility from elution, similar to previously published conclusions from Svalbard. Given the relatively short (<1 m) relocation lengths of the ions in this analysis, the authors further conclude that the Lomonosovfonna ice core chemistry time series is preserved on annual to bi-annual timescales.

In a separate analysis, the authors assess the amount of melt at the ice core site using several techniques: a traditional positive degree day technique (PDD), a snow-energy balance model, the Polar WRF regional model, and a comparison between modeled and measured density-depth profiles. The four techniques provide widely varying melt percentage results ranging from 12% (WRF) to 70% (energy balance model). The authors conclude that estimating melt percentage on Lomonosovfonna is "not straight forward", and that a melt percentage of 30% is "most probable" from 2007-2010.

The paper is generally well written with appropriate tables, figures and references. Figures are clear and captions informative. I applaud the authors' creativity in establishing a novel method for assessing melt effects on ice core chemistry. However, I am simply not convinced that the method is viable due to a number of shortcomings that would need to be addressed before publication in my opinion. I outline these shortcomings and concerns below, and follow with a detailed list of technical revisions.

Detailed 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_ 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.

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. 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. 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 $\sim$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.

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 (deposition) and negative (elution) peaks, as shown in Table 4. The implication is that the measured "relocation length" represents the depth from which mass has been eluted to the depth to 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.

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.

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. 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%.

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

in 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. (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.

Minor Comments and Technical Corrections:

P. 5056 line 18: Missing word "it" between "making" and "difficult"

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.

P. 5060 line 3: "scaling" should be "weighing"

P. 5060 line 8: "consists in" should be "consists of"

P. 5060 line 9: "top meter snowpack record from different" should be "top meter OF THE snowpack from different" (insert "of the"; delete "record")

P. 5061 line 3: "snow as function" should be "snow as a function"

P. 5061 line 6: delete comma after "(2013)"

P. 5062 lne 11: "description on" should be "description of"

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.

P. 5063 line 8: The equation is not necessary – this is generally well known.

P. 5063 line 12: "associated to" should be "associated with"

P. 5063 line 13: "uncertainty on" should be "uncertainty of"

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.

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.

P. 5066 line 8: "Having in mind" should be "Keeping in mind"

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 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.

P. 5066 line 17: "associated to" should be "associated with"

p. 5067 line 24: "ice layer" should be plural

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.

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.

P. 5069 line 17: Avoid using qualitative statements like "moderate melting". How much melting is a "moderate" amount?

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.

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