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Interactive comment on "Representativeness and seasonality of major ion records derived from NEEM firn cores" *by* G. Gfeller et al.

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italic: referee 2

bold: Gfeller et al.

General comments:

Assessing the spatial representativeness of ionic impurity records obtained from polar firn and ice cores is of great relevance in the light of the wide range of paleoclimate information inferred from these proxies, especially in the deep polar ice cores. The role of spatial variability in snow deposition has been addressed so far mostly for stable water isotope records and at drilling sites affected by wind scouring. The post-depositional effect of wind reworking the snow surface has received somewhat less attention for

ionic impurities, especially at drilling sites considered to be a closed system with respect to snow deposition. The manuscript by Gfeller et al. gives a very thorough report of a well organized study to quantify both representativeness and seasonality of ionic impurities in firn cores obtained in the vicinity of the NEEM deep drilling site. The authors have deployed a cleverly designed multi-core array to quantify the seasonal, inter-annual and spatial representativeness from their state-of-the-art impurity measurements. The role of seasonality in snow accumulation was additionally taken into account. Although exclusively based on the technique developed by Wigley et al., their evaluation of spatial representativeness convincingly shows the need for replicate coring when attempting to reconstruct inter-annual variability in aerosol concentrations at this site. The authors additionally discuss the broader relevance of their findings with respect to glacio-meterological conditions found at other polar drilling sites. The manuscript is written in a concise way with good use of the English language and the references are thorough. Great effort was made to produce comprehensible graphics featuring highly condensed information. I believe this paper provides new and unique insights and should be published in TC with only minor changes.

Specific comments:

Page 2533, line 10-13: In view of the spatial variability, it would be interesting to know the precise location of the snow pit relative to the five dice cores

The location of the snow pit is a few metres south of the location of the dice five cores. However, the snow pit has been dug mainly for dating purposes. As we only dug one snow pit, we did not include it in our spatial variability analysis. This has been clarified in the manuscript.

Page 2534, line 27-28: H+ and conductivity are reported to be "very similar"- can this statement be expressed in a more precise way? I mention the use of this somewhat imprecise term as it occurs again at other occasions, e.g. Page 2544, line 3-5 "very small", "somewhat larger", Page 2547, line 28 "agree well".

With the term "very similar" we are referring to the seasonality. Due to the very high conductivity of H^+, the total conductivity (sum of conductivity of all ions) reflects to a large part the H^+ signal. This has been clarified in the manuscript.

The terms "very small" and "somewhat larger" have been changed to the difference in percent between the approximation and the real representativeness.

The retrieved seasonality of Withlow et al. agree well within a month with our seasonality. This has been added to the manuscript.

Page 2535, line 21-23: Can you elaborate on how this procedure affects the later calculated correlation values e.g. by referring to a exemplary correlation value where this processing step has been omitted? It seems you are already improving your crosscore correlation here, although I suspect it is unlikely to affect the main results.

We agree that this step improves the cross correlation between the cores. However, this step is necessary to set the tie points in a more objective way. In a first approach the tie points are basically set by eye. Calculating correlations without this step would give as a more arbitrary result biased by the person setting the tie points. This has been clarified in the manuscript.

Page 2538, line 25: One may ask to what extent your results depend on the choice of methodology. Could you elaborate on your reasons for choosing the method of Wigley et al. over e.g. cross-wavelet correlation methods used by Karlöf et al. (2006)?

With their wavelet method, Karlöf et al. 2006 are aiming at answering the question of which are the finest timescales on which ice core data (ECM and δ^{18}O) correlate within a given area. The method of Wigley et al. 1984 on the other hand provides answers on how well the cores correlate on a given timescale (such as annual and monthly). In addition Karlöf et al. 2006 states that to successfully reject the null hypothesis of zero cross correlation in their δ^{18}O dataset they would need to have a series of minimum 380 years. Assuming a similar length for our

dissolved ions dataset, which is smaller in the order of one magnitude compare to Karlöf et al. 2006, we doubt that using their method would give us further insight.

Page 2539, line 1-5: The range of the parameters should be given, e.g. n=1,...,5 etc. Parameter T appears undefined (total length of time series?)

The variable n goes indeed from 1 to N and T depicts the length of the time series. A sentence has been added to clarify this.

Page 2543, line 22-27 and Fig. 6: It may be worth mentioning here how much the seasonal representativeness values change if calculated as fluxes using one of the accumulation scenarios?

For scenario 1 (where we assume the same accumulation rate throughout the year) the difference between single month representativeness in concentrations and fluxes is the same as the difference between annual representativeness in concentrations and fluxes (i.e. consistently higher representativeness in fluxes compared to concentrations for Na^+ and H_2O_2 and about the same for the other species). This is due to the fact that in order to calculate the accumulation for each month in scenario 1 we just divide the annual accumulation by 12.

Doing the same for accumulation scenario 2 (twice the accumulation during summer than during winter) yields equal results, meaning that the different accumulation distribution does not have a notable influence on the single month representativeness.

Page 2548, line 6-7: I feel that the discussion would benefit from a short additional elaboration of this statement. E.g. regarding: Will depositional noise have a relatively smaller effect on the preservation of the seasonal cycle as compared to inter-annual variability, since the signal of seasonality (e.g. the summer-winter contrast) is larger in amplitude as compared to the difference in inter-annual means?

The fact that the monthly representativeness values are commonly larger than the annual representativeness lies within the use of the Pearson correlation coefficient when applying the method of Wigley et al.. As the Pearson correlation coefficient is not corrected for autocorrelation bias, the representativeness values are higher if there is an autocorrelation present. However, if we are only interested in the seasonality we can assume that the effect of depositional noise is smaller due to the present autocorrelation. To remove the effect of the autocorrelation, single month representativeness values have been calculated in the manuscript.

Page 2549, line 3-16 and Table 5: Can the choice of representative values to be greater than 0.5 (line 12) and 0.8 (line 14) be justified? I assume this paragraph being referred to in the conclusions on Page 2554, line 2, where "at least 5 replicate cores" are suggested- however, one may be interested in how large the tradeoff would be in e.g.drilling only 4 cores. Making a more general remark in this context: It would be interesting to see not only values for R1,inf and R5,inf but also R2, R3 and R4, in order to judge the increase in correlation depending on the number of cores. This could be done by adding to Table 5 or simply by discussion in the text.

As stated in the manuscript, the value 0.5 is chosen so the total signal is dominated by the atmospheric variability and not other processes, The value 0.8 is kind of arbitrarily chosen, but we feel that a signal-to-noise ratio of at least 4:1 appears to be desirable, when for example making conclusions on changes in atmospheric transport patterns. We added a sentence clarifying this. A figure has been added to show all monthly and annual R values (see Fig. 1).

Interactive comment on The Cryosphere Discuss., 8, 2529, 2014.

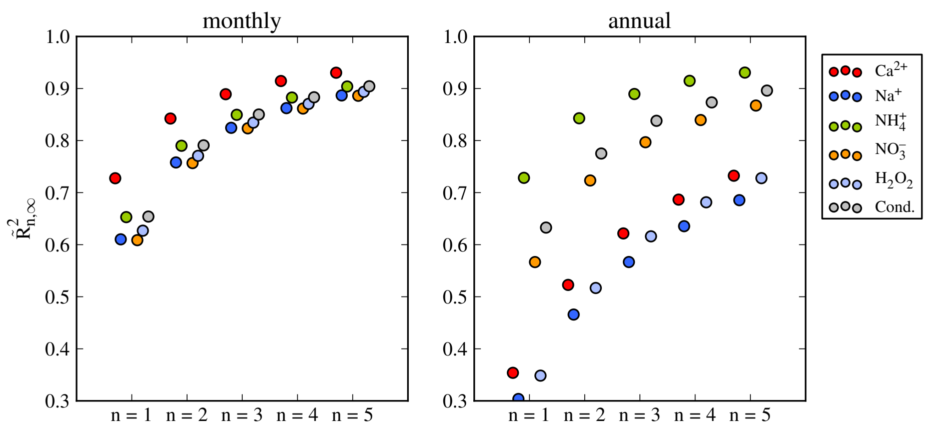

Fig. 1.