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

**Anonymous Referee #2**

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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.



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Interactive Discussion
Discussion Paper

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.

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".

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 cross-core correlation here, although I suspect it is unlikely to affect the main results.

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)?



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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?)

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?

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?

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.

Technical corrections:

Page 2531, line 26: inter-annual

Page 2546, line 24: differences

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