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The Cryosphere Discussions

Interactive comment on "Relation between surface topography and sea-salt snow chemistry from Princess Elizabeth Land, East Antarctica" *by* K. Mahalinganathan et al.

Anonymous Referee #1

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Review of "Relationship between surface topography and sea salt chemistry from Princess Elizabeth Land, East Antarctica" by K. Mahalinganathan et al.

Summary

This paper presents a detailed field study of the variations in ionic snow chemistry along a portion of the East Antarctic coast. Specifically, the paper examines the various environmental factors (wind patterns, distance from coast, elevation, slope) that may best explain the observed variations in the seasonal and annual levels and ratios of sodium, chlorine and sulfate deposited in snow. The authors hypothesize that the combined effects of converging winds (from the coast, and katabatic from the inland

ice) and slope control on air mass transport result in localized chlorine depletion in snow.

Overall assessment

This is not a ground-breaking paper, but it does offer a new interpretation for some spatial variations in Antarctic snow chemistry which, to the best of my knowledge, has not been offered before. The findings may be applicable elsewhere in Antarctica and could serve, for instance, to guide the optimal choice of ice-coring sites (for paleo-climate /paleo-atmospheric studies) or interpret glaciochemical records obtained from such cores. The snow chemistry data are new for this otherwise poorly studied sector of East Antarctica. The methods employed for obtaining or interpreting the data, however, are not new. In the conclusions, the authors offer a hypothesis to account for some of the observed spatial variations in snow chemistry documented in coastal Antarctica. The hypothesis is not strikingly new, as it refers to known processes (e.g., heterogeneous phase reactions of sea salt and acidic aerosols). However it does stress the potential importance of slope aspect in controlling ionic fractionation in snow, which has not been previously highlighted, to my knowledge. In this regard, the conclusions of the paper are substantial.

The approach and methodology are generally sound. The authors are well acquainted with the recent literature on the topics of Antarctic snow and atmospheric chemistry, and the citations are adequate. I find some shortcomings in the interpretation (see below). I do not think that these shortcomings necessarily undermine the main conclusions of the paper, but by addressing them, the authors might be able to consolidate or expand their conclusions. It may be desirable to provide the full dataset (snow chemistry and d18O values) to allow other researchers to make use of these data, or verify the soundness of the conclusions. This is a suggestion rather than a requirement.

Generally, the presentation of results is adequate. The text is concise and reasonably clear. The quality of the English is highly variable, however, and there are many sen-

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tences that are constructed in an odd and confusing way, which makes reading at times difficult (see below). Some words are used inaccurately. A thorough editorial reading for improving the language alone would be desirable. I also find some minor issues with certain figures (see detailed comments below).

Point of discussion: The interpretation offered by the authors for the pattern of chlorine depletion in snow with distance from the coast rests partly on the supposition that the mechanisms involved are seasonal in nature, either because the Cl-depleting chemical reactions are different (H2SO4 vs HNO3 reactions; discussion page 2977, line 8) or because of seasonal interactions between coastal and katabatic wind regimes. Now if the d18O and ionic profiles shown on Fig. 2 are indeed representative of the snow isotopic and chemical stratigraphy along the traverse, and if the winter -summer contrasts are so clearly delineated in snow cores, then it should be possible, in principle, to compute seasonal (summer, winter) averages of sodium, chlorine and sulfate levels at the various sampling sites from these data. This could then allow the authors to verify if the observed chlorine depletion pattern in the mean annual Na/Cl ratios (Fig. 4) is more pronounced in one season, rather than the other, and this would in turn help clarify the process(es) responsible for the depletion. It seems such an obvious thing to do that I wonder why it wasn't done, or presented. The authors should consider looking at these seasonally-resolved variations, or explain if there are valid reasons for not doing so.

Detailed editorial comments:

There are many many sentences that could be improved in the text for clarity. It would take a lengthy review (too long to be posted here) to correct all language ambiguities. I strongly encourage the authors to get a professional writer to review and edit the quality of the English in the manuscript before submitting the final version.

Abstract, line 5: The word "snow" is missing: "....variations in Cl-/Na+ ratio in snow have been attributed..."

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Interactive Comment

Note: The abstract in its present form does not really describe how slope may affect sea salt chemistry in snow, which is really what the authors get into in their discussion. I recommend modifying the abstract to shorten the descriptive sentences (lines 8-14) and expanding on the nature of the hypothesis offered by the authors on the role of slope aspect on snow chemistry (their discussion on page 2978, lines 1-19.) This is the real substance of the paper.

Page 2969, line 9: With regards to this sentence: "A thorough knowledge of all these parameters are critical, for instance, in calculating the precise mass balance of the ice sheet ..." It is not clear to me how sea salt chemistry relates to ice sheet mass balance.

Page 2972, section 2.1., line 16: The word "annuity" is inappropriate. An annuity is a financial concept. In the context of this sentence, I suggest instead: "...ïĄď18O values were used to delineate individual years in the snow cores along the transect..." Page 2973, first paragraph, line 1: Remove the citation (Ciciarelli 1991). One does not need to cite an authority for such a simple concept as the definition of a slope. The next sentence ("The relationship between...") and accompanying citation is equally superfluous: The conversion of a slope gradient into an angular measure is just basic math: it does not need to be justified by a reference. As long as the use of units for the slope (m/km or degrees) is consistent in the text, that is all that matters. Presently the text mixes both usages. Maybe distinguishing between "slope" and "slope angle" in the text would help.

Page 2973, section 3, line 13: Replace "dramatic" (too subjective) by "steep".

Page 2973, section 3, line 17: This paragraph repeatedly refers to "seasonal variations" in snow chemistry, whereas Fig. 3 does not discriminate between seasons. It would be better to speak of "seasonal range" of variations, as this is what the box and whisker plots show.

Page 2973, section 3, line 27: Replace "reduced" by "declined".

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Page 2976, section 4.2, line 20 and after. The expression "chlorine depletion events" is questionable. The word "event" suggests some punctual episode with a finite duration. There is nothing in the data or discussion that supports the view that chlorine depletion occurs in an episodic (rather than continuous) fashion. I recommend dropping the word "event".

Page 2977, line 4: The sentence that refers to chlorine depletion in the tropics seems out of place in this discussion. Also, it creates confusion as the next sentence after it (which begins by "Ma et al. (2010) have shown that...") refers to coastal Antarctica.

Suggestions about Tables and Figures:

Table 1, footnote: When referring to the significance levels, use "95 %" and "99 %" instead of 0.05 and 0.01. This is closer to standard usage.

Table 2: It would be simpler to put the mentions "slope", "slope and distance from sea", and "slope, distance from sea and elevation" directly under column header 1, rather than as footnotes.

Figure 1: Many of the fonts used in the figure are far too small to be readable. In particular, the wind rose is much too small. I suggest enlarging the wind rose as an inset.

Figure 2. Presenting the ïĄď18O and ionic profiles as "Representative" is demanding an act of faith on the part of the reader. It would be preferable to show several such examples (maybe taken at different distances from the coast) to convince the reader that the ones chosen are indeed "representative".

Figure 3. The whiskers in box-and-whisker plots do not represent outliers, but the upper and lower quartiles of the distribution. The outliers are the symbols (in this case: "x") that plot outside the whiskers. The caption is incorrect.

Figure 5. The ternary plots are almost too small to read. It might be better, for the sake of clarity, to plot the topographic profile (divided in a, b and c sectors) in a panel, and

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the ternary plots (enlarged, and labelled a, b and c) in separate panels on their own. Also, it is not clear if the slope values that are shown on the topographic profile are values for specific segments, or mean slope values for sectors a,b and c. This could be clarified in the figure caption.

Interactive comment on The Cryosphere Discuss., 5, 2967, 2011.