

## ***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 #2**

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### General comments

The authors present new snow chemistry data from short cores extracted along a 180km long transect in a coastal region of East Antarctica. The results are interpreted in terms of spatial variation of sea salt content of the annual snowpack. Emphasis is placed on the correlation between Cl/Na variation with distance from the coast and the topography. The authors conclude that the chloride depletion observed in snow from the steepest part of the transect results from the convergence of low sea-salt-laden air masses from inland with sea-salt and moisture rich air masses from the ocean enabling the sea salt dechlorination by reaction with atmospheric acids.

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This atmospheric reaction involving particulate sea-salt and gaseous acids is already well known. The katabatic wind stream competing with Na-rich coastal air masses is also basic knowledge for Antarctic chemists. That said, the impact of that specific meteorological situation on snow chemistry and its more precise localization relative to the coast could be of great importance for paleo-atmospheric circulation reconstructions from ice cores for instance. To me, this is how the new data presented in this work could have the potential to serve as a reference for further glaciochemical studies in Princess Elisabeth Land. However, the way they are presented and discussed in the paper does not lead the reader to reach any substantial conclusion on that matter. Also I wonder how it is possible to address the relation between surface topography and snow chemistry without ever calculating or mentioning any precipitation or accumulation rate or accumulation rate gradient along the transect. If the slope has to impact the local snow chemistry, it should be mostly via the favored precipitating conditions it creates locally or via worsening of the wind scouring phenomenon. Unfortunately, none of these (i.e. precipitation regime and wind scouring impact) is addressed in the paper. In addition, important sea-salt fractionation processes, such as the formation of mirabilite on newly formed sea ice for instance (Rankin et al., 2002), have been forgotten when processing the results and in the discussion about Cl/Na. Cl/Na variations cannot be interpreted in terms of chloride enrichment/depletion before sodium variability is also examined (which has not been done here). I could make the same comment about post-depositional processes involving chloride, such as HCl re-emission from snow, that the authors mention but do not calculate for how much this process accounts in the chloride depletion. Though one should acknowledge the authors attempt to first rule out the altitude effect on sea salt fractionation (section 4.1) and then to differentiate the site specific chloride depletion from the seasonal Cl/Na variation (section 4.2), the conclusions there rest on rather general assumptions that are not thoroughly supported by the results. I would also recommend some more work should be done on the description of the seasonal variations of each species measured. As the manuscript focuses on sea-salt content of snow, the sea-salt and non-sea-salt fractions of ions discussed

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(especially sulfate. Sulfate depletion should also be discussed along with Cl depletion) should be rigorously calculated. Besides, the section dealing with the peculiar wind regime over the steepest slope, which is the most interesting and most important one, is rather short and would deserve to be developed.

The general lack of structure and the variable language quality certainly prevent to follow the logical reasoning of the argumentation but still the manuscript is lacking some important details that are needed to evaluate the validity of the claims it makes.

Technical corrections

I agree with referee #1's comment about the language issue, I cannot provide here a complete list grammatical mistakes and approximations, rather I try to point out parts of the text that should be clarified, completed or changed.

Abstract

Line 5: replace "with" by "to"

Line 6-7: there is two times "records" in the same sentence

Line 11-13: "A rapid increase [...]", this sentence is descriptive and should be removed from the abstract.

Line 13: "steepest" instead of "largest"

Introduction

Note: in this section, the authors very often use vague expressions such as "the association" (line 23), "various factors" (page 2968, line 19), "as a result of differences" (page 2969, line 3), "changes" (page 2969, line 9), "various mechanisms" (page 2969, line 10) to refer to important post-depositional processes that should be explicitly named and explained a minimum in the introduction. Actually the whole section should be rewritten or better structured. The part presenting the background chemical studies (the state of the art if Antarctic coastal chemistry) could be developed and improved

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(the authors seem to jump from one subject to another telling first about sea-salt then atmospheric acids and then sea-salt again, etc. . .)

Page 2968

Line 3- 5: the subject of this sentence should not be "the coastal region". What do the authors mean? What is exactly that has an impact on the "coastal climate"?

Line 7: "very few studies have attempted to elucidate [...]", please give references here. Line 16-17: "high concentrations of H<sub>2</sub>SO<sub>4</sub>" what is "high"? Please give a range of concentrations here.

Line 24: the common way to write this equation is:  $2\text{NaCl} + \text{H}_2\text{SO}_4 \rightarrow 2\text{HCl} + \text{Na}_2\text{SO}_4$

Line 25-26: this sentence does not make sense. By "NO<sub>3</sub> flux observed in snow" do the authors mean "snow concentration in NO<sub>3</sub>"? By the way, this is the only time in the whole manuscript the notion of "flux" is brought up which is a pity because trying to calculate fluxes instead of concentrations would actually be a relevant thing to do in this study.

Page 2969

Line 2-3: "sea ice contributes to the sea-salt concentration." Here, there is an occasion to explain in which way sea ice can be a source of sea salt. References are needed there too.

Line 19: "Previous studies", please say which studies (reference).

Line 22: "open ice shelves", the authors probably mean "open sea ice".

Sampling and methodology

Note: in this section, more precisions should be given about the date of the field campaign, the coordinates of the investigated area, the altitude etc. . . About the sampling operation: was there any clean equipment (gloves, clean suits, face masks, . . .) worn

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by the operator to avoid contamination?

Page 2970

Line 7- 9: there are contradictory statements here: “[. . .] coastal area is dominated by a steep escarpment zone influenced by katabatic winds” and “the prevailing wind blows from ENE”. When does the katabatic blow exactly?

Line 13: “a faster way of snow coring sampling [. . .]” how is it possible to drill a core of soft snow without disaggregating it? I guess the snow compaction by wind prevents the disintegration of the core. But something should be said about that here (since no density measurement is provided further in the result section).

Line 22: “clean protocols”: which are they?

Line 23: “50 m upwind from landing site at each location”, out of curiosity: did you reach each of the 21 drilling sites by plane?

Page 2971

Line 6: “cations were analyzed” which cations? (Same question for “anions” line 7)

Line 10: “[. . .] better than 5% for the ions”, please say for which ions.

Line 17-18: what does mean “annual accumulation of the sea-salt concentration”?

Line 21: “[. . .]  $\delta^{18}\text{O}$  records were obscured”: obscured by what?

Line 22: what are the criteria used to determine the annual layers? This is a very important point. The method used to define an annual layer should be further explained here and should also be interpreted in terms of accumulation rate. Was the 1 m of snow always enough to get entire annual cycles in the chemistry?

Section 2.2. is not needed though it should be written somewhere what is the reference for the topography? What kind of map or DEM, GPS were used?

Results

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Note: Do the authors consider the topographical details as new results? If not, the first paragraph should be moved to the site description (section 2). Also, wouldn't it be simpler to refer to the elevation profile shown in figure 5?

Page 2972

Line 17: “the seasonal variations [. . .] (fig.3)” this figure is not the most appropriate to show seasonal variations. How have the seasons been defined?

Line 19: “range between. . . and. . .” instead of “range above”.

Line 20-25: there is no order or consistency in the results description.

Line 26-27: “High concentrations [. . .] have been recorded [. . .] (Wagenbach et al., 1988).” Why this reference? Where was this recorded? At what depth? This comes out of the blue, what is it supposed to show here?

Line 27: “concentrations decrease” instead of “reduced”.

Page 2973

Line 2: “beyond” instead of “above”

Line 9: “annual mean Cl/Na”, what is the annual mean ratio supposed to represent? “The” is missing before “lowest”

Line 11: “Cl/Na” gradually increased inland till ~50km”, please give a trend for that increase.

Line 12: “a mean value [. . .] from the Lambert glacier basin area”: why this sentence here? What is it supposed to mean?

Line 13: “the annual average Cl/Na ratio remained close [.]”, give the value of that ratio.

Discussion

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Line 17: “measured” instead of “observed”

Line 22: “calculations revealed [...]”: which calculations? Was the sea-salt-sodium calculated in some way? How?

Line 23-25: How could the nunataks be a source of sodium? It should be specified here that the authors refer to terrestrial sodium (nssNa).

Page 2974

Line 2: “[...] studies have shown that the moderate winds, rather than high winds regulate the concentration of the sea-salt particles in the atmosphere (Hall and Wolff, 1998)” This statement is taken a bit out of its context and is out of place in the text. Actually, Hall and Wolff (1998) have shown that moderate winds are more likely responsible for high sea-salt loadings (aerosols and snow) events occurring in winter. Citing these authors cannot serve as an argument to rule out the altitudinal influence on the chemical variations observed in snow.

Line 3-5: “It is therefore clear from our study that altitudinal differences [...] do not play an important role [...]” No, this is not clear at all.

Line 17-19: “[...] studies have shown a Cl depletion occur predominantly during summer months [...]” To what exactly it due this depletion? The point should be made clear.

Line 23: “[...] the seasonal records showed a very low Cl/Na ratio [...]”: to which season corresponds this low ratio?

Line 26-27: “[...] Cl depletion is resulted when the acidic species [...] react with NaCl [...]” when does is occur exactly?

Section 4.3.

Note: this is difficult to comment on this section since the reasoning is based on figure 5 that is too small and unreadable.

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Page 2975

Line 13-15: “ternary plot [...] showed a strong sea-salt fractionation resulting in a composition dominated by Na” What about the role of mirabilite crystallization in the sea-salt fractionation? This could be discussed here for instance.

Line 25: “[...] the H<sub>2</sub>SO<sub>4</sub> or its precursors like MSA”, no, MSA is not the precursor of sulfuric acid, there is a dangerous shortcut here.

Line 27: “[...] sea-salt modifications were predominant throughout the year [...]” again, no clear seasonality is put in evidence. I would like to see a plot of concentrations vs time.

Page 2976

Line 23: “[...] winds originated as a result of atmospheric circulation”, reformulate the sentence.

Line 24-30: “A strong directional constancy [...]” Meteorological data (wind direction and speed) should be shown if they are available.

Page 2977

Line 5: “[...] a localized frontal wedging that would influence the deposition of sea-salt aerosol particles [...]” This is the most important hypothesis presented in this manuscript to explain Cl depletion in snow from the steepest part of the transect. The authors cannot miss to explain here HOW that frontal wedging influence the deposition of sea-salt (is it by triggering precipitation?). Why would this precipitation happen only there above the steepest slope?

Line 14-15: these lines (the very last lines of the discussion part) seem to evocate wind scouring effect which was not addressed at all before.

Conclusion

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Line 22-23: “[...] chloride depletion resulting in a low Cl/Na [...]” there is a redundancy here.

Line 26: ” [...] we suggest that the large chloride depletions are driven by the steep slopes [...] preferential deposition of coarse sea-salt aerosol that are scavenged [...]” This sentence refers to the sea-salt particle size which was not a parameter discussed in the paper.

Table 1: coefficients below the 1 diagonal can be deleted

Figure 1: the map should show more isopleths and a close up view of the sampling line.

Figure 2: There is a sea-salt winter peak in that core, is this also observed at other locations?

Figure 3: I would suggest plotting the concentrations as a function of altitude (or slope) instead of the distance from the coast line or the steepest part of the transect should be marked.

Figure 4: In the caption, delete the sentence “Note the strong chloride [...] transect”.

Figure 5: This is an important figure but it is quite difficult to read it: triangles should be bigger.

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