The Cryosphere Discuss., 5, C1796–C1815, 2012
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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.

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Received and published: 23 January 2012

Response to Reviewer #2:

We thank the reviewer for his thoughtful comments and suggestions that helped improve the quality of our manuscript. We have addressed all the comments posted by the reviewer.

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.

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 Elizabeth 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.

Response: We have revised the manuscript in order to include the all the important details that are pointed out by the reviewer. Though additional details improved the quality of the manuscript, the main conclusion – i.e. the influence of steep slopes on coastal snow sea-salt chemistry did not change.

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.

Response: New point on accumulation rates included in the revised manuscript. We have discussed the accumulation rate gradient along the transect and discussed the variations.

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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.

Response: The accumulation rates showed a general decreasing trend inland and showed a large variation within the coastal section. Minimum accumulation was present at 23 km from the coast, the steepest zone of the transect. We have included a discussion on snow accumulation rate and explain the variations in terms of possible wind scouring effect on the steep slopes.

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.

Response: The possibilities of important sea-salt fractionation processes including mirabilite precipitation and frost flower formation acting as a source of sea-salt are added and discussed in detail in the revised manuscript as suggested by the reviewer.

Cl/Na variations cannot be interpreted in terms of chloride enrichment/depletion before sodium variability is also examined (which has not be done here).

Response: Na+ did not show any depletion in relation with the other salts. Increase in Na concentrations can be due to enhanced aerosol production or to more efficient transport, or to both causes. This point is added and discussed in the revised manuscript. We also calculated the sea-salt and non-sea-salt fractions of Na+ values for all the cores along and found that the majority of Na+ was that of marine origin.

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 account in the chloride depletion.

Response: As mentioned by the reviewer, the post-depositional re-emission of HCl

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from top layer snow plays an important role in regulating Cl/Na ratio values in snow. But, it is also known that the re-emission of HCl is highly dependent on the accumulation rate of that region (Traversi et al., 2000). For instance, Röthlisberger et al., (2003) and Benassai et al., (2005) showed that the post deposition re-emission of Cl becomes significant only below the accumulation rates of 40 kg m-2 a-1 and 80 kg m-2 a-1. The accumulation rates in the present study vary comfortably above this limit ranging between 140 kg m-2 a-1 and 399 kg m-2 a-1. Therefore, it is safe to assume that the post depositional re-emission of HCl in this region is negligible.

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.

Response: We have included the statistical results and rule out the altitudinal influence on sea-salt fractionation on the coast. The point is further explained in response to the specific comment on altitudinal influence in technical corrections.

I would also recommend some more work should be done on the description of the seasonal variations of each species measured.

Response: As suggested by the reviewers, we have calculated the seasonal concentrations of Na+, Cl- and SO42- and discussed the variations of Cl-/Na+

As the manuscript focuses on sea-salt content of snow, the sea-salt and non-sea-salt fractions of ions discussed (especially sulfate. Sulfate depletion should also be discussed along with Cl depletion) should be rigorously calculated.

Response: We have calculated the ssNa and nssSO4 and have added a brief discussion on the sulphate depletion as suggested by the reviewer.

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

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to be developed.

Response: We have included more discussion on the wind regime over the steep slopes involving the katabatic winds and the maritime air masses.

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.

Response: The general structure of the manuscript is improved with necessary language corrections. The important points suggested by the reviewer have been added to improve the structure and flow of the manuscript.

_____ 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.

Response: An experienced professional has reviewed the manuscript and improved the language. All the technical comments pointed out by the reviewer below are clarified and necessary changes are included in the revised manuscript.

Abstract

Line 5: replace "with" by "to"

Response: Replaced "with" by "to".

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

Response: Sentence corrected.

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

Response: Sentence removed as suggested.

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Line 13: “steepest” instead of “largest”

Response: Replaced “largest” with “steepest”.

Introduction

Note: in this section, the authors very often use vague expressions such as “the association” (line 23), “various factors” (page 2969, line 19), “as a result of differences” (page 2970, line 3), “changes” (page 2970, line 9), “various mechanisms” (page 2970, line 10) to refer to important post-depositional processes that should be explicitly named and explained in the introduction. Actually the whole section should be rewritten or better structured.

Response: The section 1 is slightly modified to include all the important points suggested by the reviewer.

The term “the association” page 2968, line 23 refers to how the spatial variations in snow chemistry are related to the physical features like distance from the coast, elevation and to an extent snow accumulation. Bertler and others (2005) for instance, has compiled previous works on snow and shallow core chemistry across Antarctica and statistically analyzed the relation between snow chemistry and physical features.

The term “various factors” on page 2969, line 19:refers to the post depositional loss of NO3 by photolysis and evaporation that are controlled by accumulation rate, air temperature, snow cover and solar irradiance.

The term “as a result of differences” page 2970, line 10 indicate the differences in modifications that sea-salt aerosols undergo during summer and winter in the presence of atmospheric sulfuric acid. During winter, the sulphuric acids are nearly absent in the atmosphere and therefore sea-salt modifications by sulphuric acids do not occur resulting in a clear summer-winter pattern (Aristarain et al., 2002).

The term “changes” and “various mechanisms” on line 9-10 are explained with references in the following lines 11-16.

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The part presenting the background chemical studies (the state of the art if Antarctic coastal chemistry) could be developed and improved (the authors seem to jump from one subject to another telling first about sea-salt then atmospheric acids and then sea-salt again, etc. . .)

Response: We have improved the structure of the introduction as suggested.

Page 2969

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"?

Response: The sentence is modified in the revised manuscript and appears as "High concentrations of the sea-salt aerosols on coastal parts of Antarctica have a profound influence on the formation of cloud condensation nuclei which in turn influences the coastal climate due to atmospheric scattering of solar radiation (Murphy et al., 1998; Quinn et al., 2000)"

Line 7: "very few studies have attempted to elucidate [. . .]", please give references here. Line 16-17: "high concentrations of H2SO4" what is "high"? Please give a range of concentrations here.

Response: Line 7: References are included as suggested. The sentence appears in the revised manuscript as "very few studies (e.g. Stenberg et al., 1998;Kreutz and Mayewski, 1999;Thamban et al., 2010) have attempted to elucidate the role of these surface physical parameters on snow chemistry across the Antarctic."

Response: Line 16-17: The concentration of H2SO4 is largely variable and depends on the inputs from different sources like biological productivity and volcanic inputs. Therefore exact values for the "high" range cannot be given accurately. Instead, we have changed the sentence on page, section which reads as "The major part of H2SO4 is formed due to enhanced biological activity in the surrounding ocean that peaks in the Antarctic coasts during summer (Legrand and Mayewski, 1997)"

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Line 24: the common way to write this equation is: 2NaCl + H2SO4 → 2 HCl + Na2SO4

Response: Equation corrected as suggested.

Line 25-26: this sentence does not make sense. By "NO3 flux observed in snow" do the authors mean "snow concentration in NO3"? 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.

Response: The sentence is rephrased as "Therefore, a significant variation in the SO42- and NO3- concentrations, observed in snow could be partly explained by the reactions of atmospheric acid with the sea-salt aerosol".

Both ion concentrations (g m-3) and flux (kg km-2 a-1) were compared to find out the possible dilution effect in snow sea-salt concentrations due to high accumulation rates in the transect. However, the comparison between ionic flux and concentrations show similar differences in statistics. The interpretations in this study, based on Cl/Na ratio changes do not change as the seasonal changes in Cl/Na ratio are well captured in snow layers. Therefore, we focus only on concentration based interpretations in this manuscript.

Page 2970

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.

Response: The point on sea-ice contribution is included with references. The revised line reads as "...during winter, the frost flower formation on the surface of the sea-ice form a dominant source of sea-salt concentration to the snow (Wagenbach et al., 1998; Rankin et al., 2002)."

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

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Response: References included as suggested. The revised sentence now “Previous studies therefore have mainly investigated the fluctuations in Cl-/Na+ ratio in terms of seasonal variation of the sea-salt aerosol concentration (Hara et al., 2004), the availability of atmospheric acids, as well as dust concentration in the atmosphere (Röthlisberger et al., 2008).”

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

Response: Sentence revised.

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 by the operator to avoid contamination?

Response: We have provided a table with the information on station, latitude, longitude, elevation and distance from the sea as suggested by the reviewer.

We have included a brief sentence on clean protocols during sampling operation in the revised manuscript. “The operators used face masks, polyethylene gloves over powder-free gloves to reduce the contamination risk.” Moreover, we highlight the fact that the snow core from the barrel was directly pushed into the sampling bags, thus avoiding maximum exposure to the atmosphere thereby reducing the contamination risk.

Page 2971

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?

Response: Katabatic winds are strong especially during polar nights when the surface

is cooled by radiation. They blow down the slopes and are essentially controlled by the orography. A study by Allison (1998) has indicated the presence of prominent katabatic winds in this sector of Antarctica. However, the automatic weather station data for the year 2008 obtained from Davis and Progress, both located near the study area, showed predominant surface winds in ENE direction. Though contradictory, we feel that both these wind regimes are active in this region resulting in a unique interaction along the slopes. We have clarified this in the improved manuscript.

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).

Response: The snow was compact enough for drilling a 1 m core, possibly due to wind compaction. Density measurements showed $\sim$0.4 g/cm3 for each core with variations along depths in some cores ranging between a maximum of 0.57 g/cm3 and a minimum of 0.2 g/cm3 at certain depths in few locations. This is clarified in the manuscript.

Line 22: "clean protocols": which are they?

Response: Two sets of protocols were followed – one during the field sampling and one during storage. During sampling, the operators used face masks, a layer of powder free gloves covered by another layer of polyethylene gloves to avoid contamination. The cores were then directly transferred into pre-cleaned LDPE (low density poly ethylene) bags which were labeled and sealed on the field in EPP (extra poly propylene) boxes. These EPP boxes were stored in clean conditions under -20°C until processing. The core barrel was cleaned between each sampling location using a clean hand brush. This is explained briefly in the revised manuscript.

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

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Response: Yes, we carried out sampling along the transect with the help of a helicopter.

Page 2972:

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

Response: The Cations analyzed were Na+, NH4+, K+, Mg2+, Ca2+. The anions analyzed were Cl-, MSA-, SO42- and NO3-.

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

Response: The analyzed ions include Na+, NH4+, K+, Mg2+, Ca2+, MSA-, SO42- and NO3- with precision better than 5% for all the ions except Cl-. Included in the revised manuscript.

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

Response: Sentence modified. The modified sentence reads as “δ18O values were used to delineate individual years as well as to determine the seasonal sea-salt concentrations in the snow cores along the transect”.

Line 21: “[. . .] δ18O records were obscured”: obscured by what?

Response: At certain locations, hardly any seasonal changes were observed in the seasonal amplitude of δ18O records lesser than 2 ‰.The δ18O records in snow are generally obscured by factors like variations in seasonal amplitude of condensation temperature and seasonal fluctuations in the latitudinal extent of the mean transport path. This point is clarified in the revised manuscript.

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 1m of snow always enough to get entire annual cycles in the chemistry?

Response: Annual accumulation layers were identified based on the extreme values

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of δ18O between summer and winter. At certain locations, the sea-salt chemistry changes were also used. We have explained these criteria used to determine the annual layers in the revised manuscript. We have included the details on the trend in the accumulation rates along the transect. Data from all 1 m snow cores represented entire annual layers, even on the high accumulation coastal zones. This is clarified as well.

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?

Response: This section briefly explains slope determination and is necessary for the manuscript. We used RADARSAT Antarctic Mapping Project (RAMP) 5 km DEM to create the map. This is clarified in the revised manuscript.

Results 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?

Response: Moved to section 2.2. Our manuscript mainly focuses on the role of slope variations on snow chemistry and therefore we feel that these topographic details deserve attention. We point the inset in fig. 5 for visualizing topography of the region.

Page 2973

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

Response: We agree with the reviewer's point. The seasonal 'range' of concentrations of Na+, Cl- and SO42- as a function of distance from the coast is shown in figure 3. The altitude is included in y-axis as suggested by the reviewer. This also shows the steep sloping region within 50 km. The text is modified appropriately.

Line 19: "range between. . . and . . ." instead of "range above."

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Response: We have modified this line which now reads as “Concentrations of Na+ and Cl- were exceptionally high along the initial 30 km of the transect and exceeded 100 μg L-1 throughout the year” on page, section of the revised manuscript.

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

Response: We modified this part to improve the flow and structure. We have rewritten the lines 20-25 to allow easy understanding of the results in the revised manuscript.

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?

Response: We have removed this sentence (line 26-27) from this section.

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

Response: We have replaced the word “reduced” by “decreased” in the revised manuscript.

Page 2974

Line 2: “beyond” instead of “above”

Response: Replaced the word “above” by “beyond” on lines 2 and 4.

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

Response: The word “mean” is removed from lines 9 and 11 since the term “average annual Cl/Na ratio” is more appropriate. “The” is included before “lowest”.

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

Response: We have provided the trend for the increase in Cl/Na ratio as suggested. This appears as “The annual Cl /Na+ ratios gradually increased from 0.33 at 23 km to

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1.28 at 40 km inland before reducing to 0.38 at 45 km inland" in the revised manuscript.

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

Response: We have deleted this sentence from our results.

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

Response: The average value Cl/Na ratio was 1.73 beyond 60 km. Added in the revised manuscript.

Discussion

Line 17: "measured" instead of "observed"

Response: Replaced.

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

Response: ssNa calculations were performed based on equations from Röthlisberger (2002), given as ssNa = Na – nssCa / (Ca/Na)Cr; where nssCa = Ca – (Ca/Na)sw x ssNa. We have included the reference in the revised manuscript. The line now reads as "Sea-salt sodium calculations (Röthlisberger, 2002) revealed that the majority (> 85%, data not shown) of Na+ in snow was of marine origin"

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). Response: We have clarified this sentence as suggested on section, page of the revised manuscript.

Page 2975

Line 2: "[. . .] studies have shown that 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 (1988) 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.

Response: We have removed this sentence from the manuscript. Instead, we statistically show that the altitudinal influence on snow chemistry is insignificant. Correlation statistics (Table 1) showed a significant negative correlation (> -0.8, 99% confidence level) between the sea-salt ions and elevation. In order to further explain the parameters affecting the snow sea-salt chemistry, we performed a multiple regression which revealed that the elevation component did not influence Cl-/Na+ variability. It is therefore clear from the present study region that altitudinal differences by itself in the coastal regime do not play an important role in the variability of sea-salt concentrations 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.

Response: It is made clear with additional statistics as discussed in the previous comment.

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.

Response: The point is made clear. The modified appears as "The seasonal variations of Cl/Na from bulk aerosol studies suggest that chloride depletion takes place during November and December (Jourdain and Legrand, 2002) when higher atmospheric concentrations of H2SO4 scavenges the sea-salt aerosols" in the revised manuscript.

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

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Response: Low Cl/Na was prominent throughout the year irrespective of the seasons. It is clear from fig. 4b, which shows a narrow seasonal range of variations at locations between 20 and 50 km. We have included a section in discussion to describe the seasonal (summer-winter) concentrations of sea-salt in the revised manuscript as suggested by the reviewers.

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

Response: The acidic species, in particular H2SO4, is prominent during the Antarctic summer months, especially during November and December (Jourdain and Legrand, 2002).

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

Response: Figure revised. This figure is improved as suggested by the reviewer #1.

Page 2976

Line 23-25: "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.

Response: We have included the significance of mirabilite crystallization in this section on page, section.

Line 25: "[. . .] the H2SO4 or its precursors like MSA", no, MSA is not the precursor of sulfuric acid, there is a dangerous shortcut here.

Response: Sentence corrected.

Line 27: "[. . .] sea-salt modifications were predominant throughout the year" again, no

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clear seasonality is put in evidence. I would like to see a plot of concentration vs time.

Response: The term “predominant throughout the year” is used here in order to highlight the message that in spite of the summer-winter differences in the sea-salt aerosol concentrations, the average annual Cl/Na ratio was close to 0.3. Here we mention seasonality as we are certain from our isotope data (fig. 2) which shows a clear-cut seasonality (summer-winter extremes).

The plot of concentration vs. time (depth) clearly shows the high concentration winter peaks explained by the high sea-salt input.

Page 2977

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

Response: Sentence revised.

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

Response: Please note the wind direction rose showing a predominant ENE surface winds the inset of Fig.1. The 2008 (Jan – Dec) data for surface winds were obtained from Davis (Australia) and Progress (Russia), both present on the coasts of Princess Elizabeth Land.

Page 2978

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?

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Response: The chloride depletion on the steep slopes could be due to the preferential wet deposition of sea-salt during precipitation (due to frontal wedging). When precipitation occur the snowflakes absorb sea-salt particles from the atmosphere and directly deposit them on snow. Cl losses are induced by the atmospheric H2SO4 during summer and possibly HNO3 during winter. Na, on the other hand is irreversibly deposited thus resulting in a higher concentration on these coastal slopes. Another possibility could be the interaction of sea-salt depleted katabatic winds and sea-salt rich maritime winds resulting in a turbulent mixing in the boundary layer. The katabatic winds blow at a maximum height of 200 m above the surface whereas the maritime wind covers a longer height. In the coastal part of the transect, the elevation changes from sea-level to $\sim$1100m in a span of 50 km. As a result of this, temperature inversion layers tend to form above the steep slopes. The rising maritime air mass becomes cooler when it ascends and could condense on sea-salt particles depending on the vapour conditions resulting in the deposition of sea-salt in the steep coastal section. These possibilities are disscussed in the revised manuscript with the aid of concentration and snow accumulation results.

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.

Response: We have included the wind scouring phenomenon to address the changes in accumulation rates along the transect

Conclusion

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

Response: Redundancy removed.

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 [. . .]"

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This sentence refers to the sea-salt particle size which was not a parameter discussed in the paper.

Response: Additional points on the seasonal concentrations, Na variability and chloride depletion are included and the conclusions consolidated as suggested by the reviewer. However, it is important to study the aspect of aerosol sea-salt particle size in the steep slopes in order to understand the complete processes active in these areas.

Table 1: coefficients below the 1 diagonal can be deleted

Response: Coefficients below 1 diagonal deleted as suggested.

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

Response: A close-up view with more contours and a larger wind rose are added to the main map as suggested by the reviewers.

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

Response: The winter sea-salt peaks are seen at all other locations. We have revised the figure to show representative cores from the coast (26km), midway (90km) and inland (160km) part of the transect. Sea-salt winter peaks are clearly seen all the way from coast to inland with only difference in the concentrations.

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.

Response: We have revised the figure, with y-axis representing the altitude changes. The updated figure includes distance from the coast in the x-axis and altitude in the y-axis.

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

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Response: Sentence removed as suggested.

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

Response: We have enlarged the ternary plots in the figure for a better readability as suggested.

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