

Interactive comment on “Spatial-temporal dynamics of chemical composition of surface snow in East Antarctic along the transect Station Progress-Station Vostok” by T. V. Khodzher et al.

T. V. Khodzher et al.

khodzher@lin.irk.ru

Received and published: 21 October 2013

Reviewer 2: The correlation between the variability of sea salt records and the proximity to the sea is already well known. The continental 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 this sector of the East Antarctic Plateau. However, the way

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



they are presented and discussed in the paper does not lead the reader to reach any substantial conclusion on that matter. Authors: Thank you for useful comment and suggestions. We agree with this comment and we include it in Introduction.

R2: I wonder how it is possible to perform a quantitative spatial comparison of chemical budgets without ever calculating or mentioning any precipitation or accumulation rate or accumulation rate gradient along the transect. Doing so would also serve the purpose of dating the different cores, primary task of any core studies. A: We will include section about snow layer dating in corrected manuscript.

R2: Besides the dating issue, observing a coarse peak in nss-sulfate at 150 cm is not sufficient to ascribe it to any specific volcanic eruption; one has to first address (and rule out) all the other potential sources of nss-sulfate in the snow. For instance and as underlined by Referee #1, the biogenic source of sulfate (oxidation of DMS occurring in spring) has been completely forgotten although it is a key player in the whole sulfur cycle in Antarctica! A: We agree and in new version it will be corrected.

R2: In general, I suggest this paper to be re-organized and the objectives of the study to be better defined in the introduction. I would also recommend on developing the site (or geographical sector) description in terms of meteorology, topography, etc: : : A: Yes, it will be done. The introduction will be rewritten.

R2: A snow chemical study of shallow cores taken along a transect in the neighboring Princess Elisabeth Land has been recently published in The Cryosphere (Mahalinganathan et al., 2012), how do the data of this paper compare with those published by Mahalinganathan et al. (2012) or those from Dixon et al. (2013, The Cryosphere)? Looking at similarities and differences with other transects snow data could be one additional way to exploit the valuable data collected by the authors. A: We are planning to discuss data of similar traverses in the corrected manuscript.

R2: I would advise the authors to address each ionic species at a time (or by group of ions from the same source by better defining the contribution of each of these sources).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



A: We agree and we will give description of results to each ion species in corrected version.

R2: The authors should also update their references that are usually quite old (those from the 70s, 80s and 90s) if not inappropriate. A: Yes, it will be done.

R2: Line 10-12: the hypothesis that some ions are from continental origin in central Antarctica is not new (see Delmonte et al. 2004 for instance). A: We will try to discuss it in new version of manuscript. R2: Line 13: “some profiles” Could you explain (maybe in the discussion) why the “Pinatubo horizon” was not found in all profiles? A: It depends on different snow accumulation rates at sites. We will explain it in new manuscript.

R2: Page 2008, Line 21: replace “water filter” by “circumpolar ocean”. A: We agree.

R2: Page 2009, Line 1-3: here the authors underline that decreasing trends of sea salt towards inland has long been demonstrated. The authors should explain better why it is interesting to show the same (or a different pattern) in the studied area. A: It will be better explained.

R2: Line 4: does “width” refer to “thickness” here? A: Yes, thickness.

R2: Line 6: add a reference for this statement. Line 7: contributing to (add “to”) A: It will be done.

R2: Line 9-11: How about the oxidation of DMS as a source of sulfate?? A: Yes, we agree and we will discuss it.

R2: Line 14-15: How does this relate to sulfate and to your study? Please clarify. A: We will delete this sentence.

R2: Line 17: name “nitrate” A: Yes.

R2: Line 17-19: Where was this cyclicity determined? Was this found in your snow cores? There is no figure showing such a result in your paper. A: It will be deleted.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



R2: Line 21: delete “researchers” A: Ok.

R2: Line 22: “[: :] to specify rates of snow accumulation “as well as determining ion sources”.” A: It will be done.

R2: Line 24: “these factors”: please specify which factors. A: We will rewrite this sentence.

R2: Note: in this section, more precisions should be given about the date of the field campaign, the altitude etc. : : About the sampling operation: was there any clean equipment (gloves, clean suits, face masks, : : :) worn by the operator to avoid contamination? A: This section will be clarified in more details.

R2: Line 4: 10 cm “down” Line 6: by “plane”, not by plain. A: Yes, it is misprint.

R2: Line 9: “melted under a laminar hood”. A: We will correct it.

R2: Line 24: delete “and the rate of eluent ejection was 250_Llmin-1”, this is not relevant. On the other hand, could you indicate the precision and the analytical errors for each component? A: It will be deleted.

R2: Line 9: by the arithmetical mean value of concentrations, do you mean that your “reference” is the average of all your data points of all the species for all the cores? Could you explain what is this supposed to represent? A: In new manuscript we will compare mean ion concentrations in surface snow along the traverse. We will use 2006-2008 time interval as a reference.

R2: Line 16-17: there is an attempt here to compare your results with those of other studies. Please, go further and explain (in the discussion) why your ionic load was lower than that of Kärögäs et al at the same distance from the coast providing you judge relevant to compare East Antarctica and Dronning Maud Land. If you do so, then be consistent and use the same reference for the rest of the discussion. A: It will be done in corrected version.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



R2: Page 2013, Line 6-9: this paragraph could be moved to the introduction. A: We are planning to rewrite introduction.

R2: Page 2013, Line 10-12: “It is noteworthy [: : :]” Please, explain in details how this statement is used to interpret your results. A: It will be deleted.

R2: Page 2013, Line 19-20: “[: : :] evaporation of sea water could be a source of elevated concentrations of Na+ [: : :]”. Please specify to which depth layer (and if possible to which season) your comment refers. Could you also address the role of winter sea ice in the sea salt budget of the snow? What about the role of mirabilite crystallization in the sea-salt fractionation for instance? A: Unfortunately we can not define seasonal signals due to low sampling resolution. It will be explained in the text.

R2: Page 2014, Line 6-8: “An attempt was made to determine possible continental sources [: : :]” Could you please explain “how” you tried to determine continental sources. A: It will be corrected.

R2: Page 2014, Line 9-14: This paragraph somehow addresses the climatology and the meteorological specificities of the study area. This should be moved to the introduction and references should be added. A: We are planning to rewrite introduction.

R2: Page 2014, Line 20: You forgot the oxidation of DMS as a source of MSA and sulfate. Page 2015, Line 1: Explain how you calculated the coefficient in equation (2) or give a reference for it. A: It will be corrected in new manuscript and references will be added.

R2: Line 14: Provide a newer reference for Pinatubo sulfate horizon in snow. A: We will do it.

R2: Line 24-26: This conclusion is not based on any quantitative result or statistical investigation. A: We will correct it. In new version we will use 5-day back trajectory analysis to explain our results.

R2: Including a map showing the topography and the transect path would be needed

C2120

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



and could replace Table 1. For better reading purpose, I would suggest plotting the Na, Mg, K, Ca concentration (+ standard deviation) rather than presenting them into 4 tables. Or, alternatively, Table 2-5 could be replaced by figures like Figure 3. Figure 3 and 5: what are the different shades of grey? Figure 4: an age scale is missing. A: We will redraw the map. Ion concentration will be shown on one figure as matrix diagram. Mean ion concentration will be shown on separate figure versus inland distance.

Interactive comment on The Cryosphere Discuss., 7, 2007, 2013.

TCD

7, C2116–C2121, 2013

[Interactive
Comment](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

