

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.

Anonymous Referee #2

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General comments

The authors present new snow chemistry data from short snow cores extracted along a 1276 km long transect from the coast to Vostok region in East Antarctica. The results are interpreted in terms of spatial and temporal variation of the total ionic concentration. Emphasis is placed on the sea salt content variation with distance from the coast. The authors conclude that the decreasing ionic load observed in snow along the transect results from the least influence of sea-salt-laden air masses further inland. The authors attribute the rise of the total ionic content to the input of air masses of continental origin for the cores drilled beyond 560km from the coast. High concentrations in nss-sulfate at 150 cm depth are ascribed to the Pinatubo eruption's fallout. The correlation between

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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 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 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. 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!

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 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. 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). The authors should also

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update their references that are usually quite old (those from the 70s, 80s and 90s) if not inappropriate.

Technical corrections I will try to not repeat Referee #1's comments but will point out parts of the text that should be clarified, completed or changed.

Abstract

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). Line 13: "some profiles" Could you explain (maybe in the discussion) why the "Pinatubo horizon" was not found in all profiles?

Introduction

Page 2008, Line 21: replace "water filter" by "circumpolar ocean". 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. Line 4: does "width" refer to "thickness" here? Line 6: add a reference for this statement. Line 7: contributing to (add "to") Line 9-11: How about the oxidation of DMS as a source of sulfate?? Line 14-15: How does this relate to sulfate and to your study? Please clarify. Line 17: name "nitrate" 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. Line 21: delete "researchers" Line 22: "[. . .] to specify rates of snow accumulation "as well as determining ion sources"." Line 24: "these factors": please specify which factors.

Materials and methods

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? Line 4: 10 cm "down" Line 6: by "plane", not by plain. Line 9: "melted under a laminar hood".

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Line 24: delete "and the rate of eluent ejection was $250\mu\text{Lmin}^{-1}$ ", this is not relevant. On the other hand, could you indicate the precision and the analytical errors for each component?

Results

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? 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 K rg s 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.

Discussion

Page 2013, Line 6-9: this paragraph could be moved to the introduction. Page 2013, Line 10-12: "It is noteworthy [. . .]" Please, explain in details how this statement is used to interpret your results. 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? 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. 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. 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. Line 14: Provide a newer reference for Pinatubo sulfate

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horizon in snow.

Conclusion

Line 24-26: This conclusion is not based on any quantitative result or statistical investigation.

Figures

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

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