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Interactive comment on "Nitrate deposition and preservation in the snowpack along a traverse from coast to the ice sheet summit (Dome A) in East Antarctica" *by* Guitao Shi et al.

Anonymous Referee #1

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GENERAL COMMENTS

This study reports new measurements of nitrate in a large number of Antarctic surface snow and pit samples collected over several years on a transect between the coast and Dome A. Based on a linear model it is concluded that on the coast nitrate flux to the snowpack is dominated by wet deposition illustrated by a positive correlation with accumulation rates, dry deposition contributing up to 44% and atmospheric nitrate being quite homogeneous. Further inland on the Antarctic Plateau a positive correlation between concentration and acculumlation rate is found suggestive of post-depositional loss. Contrary to a previous coastal study no association between nitrate and sodium

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Discussion paper

in snow was found, but rather with nss-so4 suggesting a role of small sized aerosol in nitrate scavenging and deposition.

This study contributes a large number of new observations from remote areas, which involved careful sampling on locations along the traverse, sample handling and analysis, and they clearly merit publication. The finding that no3 correlates with nss-so4 but not with na is very interesting and new. The main weakness is the discussion on no3 deposition processes, which needs significant improvement before I can recommend publication. In particular, a more thorough comparison with other studies and a critical discussion of model choice and interpretation are required.

SPECIFIC COMMENTS

- The authors apply a linear model to interpret their data. Contrary to their description Eq. 4-6 are esentially the same model, i.e. inserting Eq.4 into Eq.6 yields Eq.5. I strongly suggest to simplify (use maybe the notation of Alley et al, 1995), explain model assumptions, parameters and limitations. Note this model is the simplest plausible model to relate chemical flux and concentration in snow to atmospheric concentrations introduced more than 20yr ago (Legrand, M., 1987; Alley et al., 1995) and is a gross over-simplification of the complex nature of air-snow exchange of nitrate. It's probably ok near the coast, but fails inland due to post-depositional redistribution and loss of nitrate. Negative dry deposition rates can be interpreted as losses and should also be compared to other studies in the regions, e.g. Pasteris et al. (2014) and Weller et al. (2004, 2007). I suspect that precise values for dry deposition rates and fresh snow values depend which and how many locations are included in the regression analysis (and also to a minor extent if you use regression parameters from eq4 or eq5). The discussion on inland snowpack (Section 4.1.2) should be expanded accordingly; e.g. take a closer look at losses shown in Fig 4, how do they compare to loss rate from the regressions, how do they depend on environmental factors?

- the authors make surprisingly little mentioning of new isotopic tools in their brief literature review and discussion (including their own study Shi et al;, 2014), which in my view achieved significant reduction of the uncertainties related to post-depositional no3 processes and the origin of no3 maxima in Antarctic snow. I'd recommend to highlight better the progress in no3 air-snow exchange research and integrate it into the discussion. You could set out from the beginning that you don't expect your chosen model to work on the Plateau because of strong losses.

- the authors mention their unpublished measurements of atmosperic no3 on the coast (l337-38) and on the traverse (426-428). Is there any particular reason why they are not part of a manuscript on air-snow exchange of no3? I'd like to see these included in the paper, as they could add significantly to the discussion of deposition and association to nss-so4 and sea salt (the novel part of this paper).

TECHNICAL CORRECTIONS

l35 ... dry deposition velocity and scavenging ratio for NO3- was relatively constant near the coast ... is this not a model assumption? which then allows you to state that atmospheric nitrate is homogeneous on the coast, please clarify how you interpret the linear model.

l36 ... association ... throughout the text you use association but mean probably correlation. Please change and state R and p value

l55 tropospheric and stratospheric sources

l75 isotopes show stratospheric origin of nitrate peak in late winter/ early spring (Savarino, 2007; Frey 2009)

l80-84 it seems to me that the SPE hypothesis has recently been basically refuted; please update your summary & citations including e.g. Wolff et al. (2012 & 2016), Duderstadt et al. (2014)

l86 ... the relationship ... varies temporally and spatially l87-89 more correctly: ... Isotope studies suggest that under cold conditions photolytic loss dominates, whereas HNO3 volatilization becomes important at warmer temperatures > -20 °C (Frey 2009, Erbland 2013, Berhanu 2015)

l93 and field measurements on the East Antarctic Plateau at Dome C suggest e-folding depths of 10 to 20 cm (France et al., 2012)

l94-95 Clarify that photolysis dominates loss. This is also in support of your own assumption that no3 is archived below the photic zone of ~1m depth, where temperature still varies on diurnal to annual time scales. It implies that physical losses are assumed to be not important throughout the study region.

l105 please add also Bertler et al. 2005, Pasteris et al., 2014

l122 does SP20 correspond to the location of the station at Dome A?

l129 add lat/lon and elevation of station

l134 took OR lasted 4 summer seasons

l194 add a note that so4 fractionation may introduce a bias in nss-so4 (Wagenbach et al., 1998)

l250-52 Please be precise and expand: were the pits dated? do you see 1, 2 or more annual no3 peaks?

l256 careful with language: not maybe, but yes previous studies inland (on the Antarctic Plateau) have shown that the decrease is due to significant loss/redistribution of NO3-

l279-80 due to photolysis

l290-94 note you assume that photolysis is main loss process which is sensible, but explain better in intro (see comment on l94-95)

l302 do you mean deposition velocity or flux? explain model assumptions (see above)

l306, 329-30 consolidate your model (see above) l311 use consistently r or r2 throughout the paper, and include p value

l337-38 are these annual mean and std of atmospheric nitrate? Coastal observations (Neumayer, Halley, DDU) show a distinct annual cycle. how would that affect your estimate of deposition velocity?

l340 "... compares well to ..." I disagree, this is a large uncertainty, a range of 0.5 to 0.8 cm/s can make a big difference when modeling no3 in surface snow (see for example Erbland et al. 2013, Fig.7)

l352 is negatively correlated with

l354 based on what exactly? the R value? please explain

l365 correlation

l370 the correlation ... is reatively weak and of opposite sign

l375 why act surprised? we know based on previous work that this is of course due to losses, the model application is limited inland

l404-05 but uncertainties have been reduced over the last decade (see comment above)

l406 and snow optical properties (e-folding depth)

l426-428 I'd be very interested to see the atmospheric data; why are they not included in this manuscript?

l463-464 I don't understand, please expand (mirabilite is Na2SO4-10H2O)

FIGURES

Fig3 possibly add accumulation rate into ea figure to understand better at which threshold no3 spikes disappear

Fig4 possibly add site ID on the x-Axis to follow better the discussion

Fig5 improve figure readability (size, label font)

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