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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.

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The paper needs major revisions before being accepted. The authors should better present their data in light of recent and past publications. Many important works are not referenced and it seems difficult to follow the conclusions (mainly part 4) of the authors based on only snow concentrations when other publications measuring all aspects of atmospheric parameters struggle to conclude on the fate of nitrate, its origin, formation, transport deposition and post deposition.

Reference to work suggesting an extraterrestrial source of nitrate in ice has been repeatedly dismissed (1-3 just for the most recent publications). Clearly state this fact or remove any reference to those works.

1-Wolff, E. W., Jones, A. E., Bauguitte, S. J.-B., and Salmon, R. A.: Reassessment of the factors controlling temporal profiles of nitrate in polar ice cores using evidence from snow and atmospheric measurements, Atmospheric Chemistry and Physics Discussion, 8, 11039-11062, 2008. 2-Wolff, E. W., Bigler, M., Curran, M. A. J., Dibb, J. E., Frey, M. M., Legrand, M., and McConnell, J. R.: The Carrington event not observed in most ice core nitrate records, Geophys. Res. Lett., 39, L08503, 10.1029/2012gl051603, 2012. 3-Duderstadt, K. A., Dibb, J. E., Schwadron, N. A., Spence, H. E., Solomon, S. C., Yudin, V. A., Jackman, C. H., and Randall, C. E.: Nitrate ion spikes in ice cores not suitable as proxies for solar proton events, Journal of Geophysical Research: Atmospheres, n/a-n/a, 10.1002/2015JD023805, 2016.

Volatilization of nitrate. In Erbland et al. 2013 and Berhanu et al., 2014, 2015 (4-5) isotope fractionations demonstrate that vitalization is not an important loss process in contradiction with the authors statement (line 96). This should be clearly mentioned. What do you call post depositional effects beside photo-dissociation and volatilization? For me they are the post depositional effects. If you think there is more effects to take into accounts please, indicate which ones?

4- Berhanu, T. A., Meusinger, C., Erbland, J., Jost, R., Bhattacharya, S. K., Johnson, M. S., and Savarino, J.: Laboratory study of nitrate photolysis in Antarctic snow. II. Isotopic effects and wavelength dependence, The Journal of Chemical Physics, 140, 244305, 10.1063/1.4882899, 2014. 5- Berhanu, T. A., Savarino, J., Erbland, J., Vicars, W. C., Preunkert, S., Martins, J. F., and Johnson, M. S.: Isotopic effects of nitrate photochemistry in snow: a field study at Dome C, Antarctica, Atmos. Chem. Phys., 15, 11243-11256, 10.5194/acp-15-11243-2015, 2015.

Please also consider this publication for your introduction

Bock, J., Savarino, J., and Picard, G.: Air–snow exchange of nitrate: a modelling approach to investigate physicochemical processes in surface snow at Dome C, Antarc-

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tica, Atmos. Chem. Phys., 16, 12531-12550, 10.5194/acp-16-12531-2016, 2016.

Acidity calculation is wrong. H+ = Σanions - Σcations, the equation used is a simplification and do not for instance takes into account ammonium ions.

Cv is not defined (line 206)

Erbland 2013 sampled many snow pits at a higher resolution than Frey 2009 (line 231). It is this reference that should be used and cited here.

Line 257 replace "may be" by "as a result of post depositional processing" This is no doubt about that.

Line 288 change proposed by demonstrated - Again isotopes of nitrate have demonstrated the correctness of this assertion.

line 291: Please add France 2011 reference, the first publication to have measured the optical depth of the snow pack in the UV range, years before Zatko

France, J. L., King, M. D., Frey, M. M., Erbland, J., Picard, G., Preunkert, S., MacArthur, A., and Savarino, J.: Snow optical properties at Dome C (Concordia), Antarctica; implications for snow emissions and snow chemistry of reactive nitrogen, Atmos. Chem. Phys., 11, 9787-9801, 10.5194/acp-11-9787-2011, 2011.

line 293 The idea that below the photic zone, nitrate is archived without further modification is an idea developed in Frey 2009, Erbland 2013 and 2015. This should be recognized.

line 306: Change dry deposition by apparent dry deposition. See Bock et al. but also the second reviewer's comments.

line 320: it is not the strong correlation between deposition flux and accumulation that makes wet deposition to dominate but the comparison between "dry" and wet fluxes (see your eq 5). The fact that a correlation exists only means that the scavenging ratio of atmospheric nitrate by snowfall is constant or in other words the concentration in

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snow fall is independent of the snow accumulation (see your equation 5).

line 331: K2 is not dimensionless as it allows to convert atmospheric concentration (mass/volume) to snow concentration (mass/mass), it has a unit of m3/g. How K2 is calculated? According to Eq5, K2 x Catm = Cf-snow, so K2 = 43/20 = 2.1 meaning that 1 g of snow scavenged 2 m3 of air. Also note that eq 5 & 6 is nothing else than your eq4. These models are not different models but the same, expressed in different way. It is thus not surprising to find the same dry deposition flux. Comment your dry deposition with respect to previous publication (eg Pasteris 2014)

line 342: give the reference for the deposition velocity at South Pole.

Line 347: K2 in eq7 cannot be equal to K2 in eq5. K2 in eq5 takes implicitly into account , the density of air, as K2/ = K in eq7, unless I have missed something

line 352: not sure these inferred parameters are better than concentration observations to provide useful reference values for modeling. These are macroscopic, apparent parameters that are unable to describe processes at microscopic scale. See Bock 2016.

Fig5a and fig5b are in contradiction. The same parameter (p-concentration) cannot be linear with respect to a variable A and its reverse 1/A (same for fig5e & fig5f). I also found p-concentration not very expressive. Archived, deep concentration seems more appropriate.

Why slope of fig5a & fig6b are so different if no nitrate is lost in coastal region ? In general, Cfirn, Cp-concentration, Cf-snow are poorly labeled on figures (why not using the same as Pasteris 2014), why in fig6 f-snow label is not used, same for fig4? This makes the reading of the figures very confusing.

line 381: replace snow accumulation by inverse snow accumulation. Also please comment the difference of nitrate flux loss between you (-73.9 ueq m-2 a-1) and Pasteris 2014 (-22 ueq m-2 a-1), as well as for the slope, 2.7 vs 1.1 when accumulation rates cover the same range.

Figure 6h: There is something difficult to understand and seems to be a circular reasoning in fig6. Since Flux = snow concentration x snow accumulation, and only concentration and accumulation are measured, how fig6g and 6h can produce both a linear trend. In fig6g, slope gives snow concentration, the linear trend then suggests a constant homogeneous snow concentration in fresh snow. Slope of fig6h gives a constant homogeneous accumulation (in clear contradiction with measurements), well if accumulation is constant and snow concentration is constant, how the flux can vary? (same observation for fig5) Your conclusion that accumulation is not the main driver of the preserved nitrate (line 387) contradicts fig6g and the linear trend plotted. I will suggest to remove the linear trend of fig6g, which obviously looks like more exponential than linear.

line 403: in reference add Erbland 2013, France, 2011

line 405: add Davis et al., 2004 reference

Davis, D., Chen, G., Buhr, M., Crawford, J., Lenschow, D., Lefer, B., Shetter, R., Eisele, F., Mauldin, L., and Hogan, A.: South Pole NOx Chemistry: an assessment of factors controlling variability and absolute levels, Atmos. Environ., 38, 5375-5388, 10.1016/j.atmosenv.2004.04.039, 2004.

line 413: do you mean fig6e, f instead of 6c & d ?

The part4 needs to be revisited in light of the references given by reviewer 2. There are many misconceptions. The first is that a correlation does not imply a causal effect. nitrate and sulfate summer peaks may have completely unconnected reasons (max photo-denitrification and max marine emission respectively followed by dry and wet depositions). Nitrate aerosols are not on the same aerosols size bin than sulfuric acid (Jourdain and Legrand, 2002). Even in heavily sea salt impacted coastal sites, half of the nitrate is in acid form and rapidly goes to almost 100% inland. There are no reasonable observations to support the conversion of NOx to nitrate by sulfate aerosols (in addition than N2O5 does not exist in summer), neither than nitrate is internally mixed with sulfate aerosols.

Jourdain, B., and Legrand, M.: Year-round records of bulk and size-segregated aerosol composition and HCl and HNO3 levels in the Dumont d'Urville (coastal Antarctica) atmosphere: Implications for sea-salt aerosol fractionation in the winter and summer, J. Geophys. Res., 107, 4645, 10.1029/2002jd002471, 2002.

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