

Interactive comment on “The diurnal variability of atmospheric nitrogen oxides (NO and NO₂) above the Antarctic Plateau driven by atmospheric stability and snow emissions” by M. M. Frey et al.

Anonymous Referee #3

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Comments on the manuscript acp-2012-635 from Frey et al.: “The diurnal variability of atmospheric nitrogen oxides (NO & NO₂) above the Antarctic Plateau driven by atmospheric stability and snow emissions”

This manuscript reports measurements of NO and NO₂ concentrations at three heights above the snow in the Antarctic Plateau. The concentration gradient was used to estimate the NO_x fluxes by the flux-gradient approach. They report NO_x emissions similar in its dynamics but somehow larger than modelled NO₃ photolysis rates. The dynamics of NO_x mixing ratios is shown to depend both on NO_x emissions and boundary layer height dynamics.

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This study reports new data of NO and NO₂ concentrations and flux measurements in the Antarctic Plateau. The experimental methods were well adapted and the cautiously analysed. The graphics and tables are clear (but could be improved though). The conclusions are correctly deduced from the results. The manuscript is quite well written and clear.

There are however concerns that needs to be addressed prior to publications:

* The main concern is the fact that the gradient method was used between 0.01 m and 1 m. The lowest height (0.01 m above the snow) is very close to the ground and may well be in the roughness layer. In such conditions the gradient method which is based on the Monin and Obukhov similarity theory may not be adapted. The authors should estimate the dynamic roughness length at the site and check that 1 cm is well above z_0 (at least twice as large). I am not familiar with Antarctica and Dome C in particular. Probably the roughness length is very small (for ice z_0 is probably smaller than 1 mm). However, even if z_0 is small the uncertainty on the sampling height is also larger at the smallest height, and especially when sampling in very calm air (the air is drawn from a volume around the sampling line. Why the authors did not used the gradient between 1 and 4 m height? Why did they not, at least, compare the flux estimated based on 0.01 and 1 m and 1 and 4 m height? If 1 cm is above the roughness layer, the authors could also use the three heights together to estimate the NO_x flux by the aerodynamic gradient method. I therefore would like the authors to comment on these points and evaluate the flux at several heights.

* Another concern is the fact that the NO₂ emission from the snow is evaluated based on the NO₃ photolysis at the snow surface. However no NO₃ measurements are reported in the manuscript and no mention is given on how it was constrained in the model. This should be detailed.

* A general comment is that the manuscript should be a bit reorganised so that all methodological descriptions comes at once in the material and methods. In the present

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manuscript some methodological details are given in the results and even in the discussion section. See detailed comments in the attached document.

* A question that arises from this work is: where does this nitrate in the snow come from? Does it come from precipitation mainly or also from deposition of HNO₃ which hence means a sort of cycling between NO₂ emissions from NO₃ in the snow which is deposited back to the snow. Could the authors comment on this.

There are also detailed comments in the attached pdf document which should be addressed by the authors.

Once all these comments have been addressed I therefore recommend publication of this manuscript in Atmospheric Chemistry and Physics.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/12/C8815/2012/acpd-12-C8815-2012-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22309, 2012.