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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The diurnal variability of atmospheric nitrogen oxides (NO and NO_2) above the Antarctic Plateau driven by atmospheric stability and snow emissions" *by* M. M. Frey et al.

Anonymous Referee #2

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This publication presents a nice data set resulting from gas measurements of nitrogen oxides conducted at Dome C, Antarctica. Air was sampled from within the firn, and from multiple heights on a tower. The tower gradient measurements were used to derive NOx surface flux estimates. Measurements seem to be of good quality and the data interpretation was done in depth. However, in quite a number of places data processing and interpretation appears to be over-simplistic. The manuscript 'wanders' off at times and seems to be overly lengthy for the amount of new findings that are presented. The writing could be improved to be more precise and concise. Overall, I think this manuscript is a good fit for this journal but it needs some substantial improvements for publication in ACP.

Specific comments:

Page 22310/abstract: Mention measurement period and overall number of days for which data were obtained.

Page 22310/13: Please be very clear if data are reported as daytime averages, nighttime averages, or 24-h averages given that Dome C does have a significant diurnal cycle, as seen in these data.

22310/24: . . ..NO and NO2 (NO + NO2 = NOx) play a . . ..

22310/3-4: Mention how/why halogens prevent elevated NOx levels.

22313/10: Give dates of measurements.

22313/17-22: Provide sensor technical information.

22315/23: Instead of being speculative this effect should be quantified by laboratory tests.

22316/2: Please clarify what is meant by 'baseline'. Is this the instrument background?

22316/23-25: Here and in other places where instrument performance and data are reported, be very clear what the averaging period for the reported results is.

22317/4: Assuming that the background was determined properly and subtracted from measurements, then 'discarding' of negative values would not be an appropriate method for data reduction. Due to statistical noise a certain fraction of data would be expected to be negative. Just eliminating these will bias the calculation of data averages.

22318/8: I am not aware of any other flux study where turbulence and chemical gradient measurements were not co-located on the same tower. I understand the dilemma these researchers are facing here due to their sensor failure. This very unusual procedure raises questions about its applicability. This point needs to be evaluated and

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justified.

22319/5: Or 0.8 km?

22319/8-10: Revisit sentence structure.

22319/20-22: This explanation does not seem reasonable. Please provide more detail. How is the 1-ïĄş standard error defined here? Is this the error for the gradient? How was it determined? Or is this a measure of the variability of the gradient?

22319: Despite the fact that the authors try to be detailed in their description of methods and analysis procedures, there are some significant shortcomings of the flux data processing. Flux gradient determination is compromised by a number of environmental conditions. For instance, these relationships loose applicability under stable conditions or/and when boundary layers become so shallow that measurements are above the surface layer (lowest 10% of boundary layer). The flux calculations should be revisited with closer consideration of these limitations (see for instance (Cullen and Steffen, 2001), (Cohen et al., 2007), (Bocquet et al., 2011)). It would also be desirable to see an uncertainty estimate of the flux estimation.

22320/18: Are these really enough data to deduce seasonal trends?

22320/28: What was the threshold for 'significant' and how was it determined?

22322/4: Why do the authors not use the observations from this study?

22323/20: The authors use the term 'diffusion' as a surrogate for atmospheric turbulence or transport. This is probably not appropriate. Diffusion is one, and the weakest/slowest of the processes driving atmospheric transport (Neff et al., 2008). I recommend that the entire paper be revisited with a stricter adherence to the definition of atmospheric transfer terms.

22323/21: For boundary layer height estimation two approaches, i.e. the Pollard et al. and the Zilinkevitch equation were used. Unfortunately there is no discussion un-

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der what stability range these were applied the estimating BLH. It almost seems that these equations were applied regardless of conditions (i.e. were any stability conditions filtered out in the BLH estimates?). As far as I understand, in the literature these two diagnostic equations only provided good comparisons to observations for stable to weakly stable conditions - so there is no support for using these models outside of that range, for instance at night (based on the scaling estimates used to derive these equations it doesn't seem to make sense to apply them outside of the stable to weakly stable range). I suggest that the authors clarify what range and filters they used for the BLH estimation.

22325/22: This is a very large error for an atmospheric temperature determination?

22328/13-15: One should be very careful in applying South Pole data, resulting from conditions without diurnal cycle and boundary layer turnover, to conditions at Dome C. How were the diurnal changes in T, BLH, radiation, WS at Dome C considered for estimating diurnal OH cycles?

22329: . . ...vary considerably between sites . . .

22333/16-29: This section needs improvement in its writing. Furthermore, can these statements indeed be supported by valid flux data, given the limitations of the flux gradient technique under strong radiative cooling and low BLH conditions?

Figures: I recommend using a consistent format for labeling times/dates in all figures. All abbreviated axis labels and legends should be explained in the figure caption.

Fig. 4 caption line 4: Again, this procedure seems questionable, but is hard to fully evaluate without having a more concrete explanation how the 'standard error' was determined.

Fig. 5d: The same vertical axis scale should be used for displaying the two boundary layer estimates, otherwise this graph is misleading.

References cited

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Neff, W., Helmig, D., Grachev, A. and Davis, D., 2008. A study of boundary layer behavior associated with high NO concentrations at the South Pole using a minisodar, tethered balloon, and sonic anemometer. Atmospheric Environment, 42(12): 2762-2779.

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