A considerable amount of effort has obviously gone into this work, including that to produce such data from what sounds like a difficult field season. A few comments and questions that I have regarding the study are presented below.

We would like to thank Aron Buffen for his helpful comments and positive feedbacks that we addressed below. Our responses follow each comment in bold type.

1) Page 6822; Equation (4):
I'm confused about why the depth-integrated NO_2 flux was multiplied by the $k_{(R7)}/k_{(R8)}$ ratio, i.e., by a factor of 8 to 9. Isn't this the ratio of the quantum yields for the NO_2 (R7) and NO_2^- (R8) pathways, respectively? The photolysis rate constant, however, was already calculated specifically for the NO_2 pathway since the quantum yield from Chu and Anastasio is for (R7) only.

Changes in the manuscript were made upon reviewer's observation. The ratio $k_{(R7)}/k_{(R8)}$ was removed from our calculations. This implied a 10% increase of our original estimated atmospheric NOx emissions.

2) Use of HYSPLIT in Antarctica:
How confident are you that your back trajectories are accurate? I assume these are based on the NCEP reanalysis data that HYSPLIT uses by default. The reliability of the various reanalysis datasets (NCEP, ECMWF ERA...) differs in Antarctica (even in the satellite era) since they use different assimilation schemes and there is a paucity of surface and radiosonde observational constraint. This has been highlighted in several papers, e.g., *Bromwich, D. H., R. L. Fogt, K. I. Hodges, and J. E. Walsh (2007), A tropospheric assessment of the ERA-40, NCEP, and JRA-25 global reanalyses in the polar regions, J. Geophys. Res., 112(D10), D10111, doi:10.1029/2006jd007859*. "After 1979, large differences still exist between the reanalyses in the circulation, precipitation and SAM trends. It is even more apparent from this body of evidence that the reanalyses in the high southern latitudes are strongly dependent upon the satellite sounder data for guidance." And see: *Bromwich, D. H., and R. L. Fogt (2004), Strong Trends in the Skill of the ERA-40 and NCEP–NCAR Reanalyses in the High and Midlatitudes of the Southern Hemisphere, 1958–2001*, J. Clim., 17(23), 4603-4619, doi:10.1175/3241.1.* The general conclusion is that the ERA data may be more accurate for Antarctica.
Since you can import any downloaded ECMWF ERA-Interim (this replaced the ERA-40) data into HYSPLIT, I think it would useful to compute and compare back trajectories using these data with those based on the NCEP data.

The back trajectories from HYSPLIT were calculated with the Global Data assimilation System (GDAS) archived data that have been used in previous studies over the Antarctic continent (Preunkert et al., 2008; Legrand et al., 2009; Jones et al., 2009; Gassó et al., 2010; Buys et al., 2013).This detail was added to the manuscript.
We did not run any back trajectory based on the ERA-40 data, but we used the NCEP-NCAR meteorological inputs to drive HYSPLIT. Both archived data, ERA-40 and NCEP-NCAR, produce similar results over the Antarctic during austral summer when observations from satellites and stations are fully available (Bromwich and Fogt, 2004; Bromwich et al., 2007; Sinclair et al., 2013).
Results from the HYSPLIT model driven by the GDAS and NCEP-NCAR data are comparable and we now present the two resulting distributions of air-mass

sources in Figure 9.

3) Figures 5a and 6a:
I'm not sure how you can make any conclusions about trends or temporal variability in surface snow nitrate concentration when the range of values from each of the days with five simultaneous surface collections spans that of the entire dataset! The same would go for what you can say about surface variability being due to degassing and/or photolysis. Also, you seem to suggest on page 6818 (line 12) that nitrate can only be contributed by snowfall, but what about fog, riming or dry deposition?

We acknowledge this comment, but we have no additional data on post-depositional changes to discuss further details on the processes involved in the variability of nitrate in snow surface. We only referred to snowfall and snow drift because none of the other depositional processes was recorded or measured during the campaign. The text was edited in regards to this comment (Section 4.1.1).

4) Figure 1a (1 min NO averages):
Does this plot contain the data that were contaminated by camp pollution? Rejecting 25% of the entire dataset seems large, so it might be useful to see which points these were or to mark the threshold above which the samples were rejected. If these spikes are the polluted samples, then doesn't it seem like contamination is ubiquitous? If not, are these indications of large, rapid and short term NO fluxes?

The plot presents the NO data after filtering, therefore it does not show the rejected 25% of the data that was attributed to times with in situ pollution. We added "NO after filtering" in the figure's caption.
Since NO mixing ratios from local contamination (generators and heater exhaust) reached values that were orders of magnitude higher than the atmospheric NO background, we are confident that these spikes do not result from contamination. Analyzed with Figure 2 (see Section 4.1.1), these spikes in the NO dataset appear to be a combination of changes in the NO_x photolytic production from the top snowpack, in the atmospheric boundary layer height and in the turbulent mixing at the surface.

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