We thank Dr. Jennie Thomas for consideration of this manuscript and her positive review. Our responses follow each comment in bold type.

Overview

This paper provides a new dataset to further investigate ozone, NOx, HOx cycling in the polar regions. Given that this is a topic of great interest for those aimed at understanding atmospheric chemistry and snow-air cycling in both the Antarctic and Arctic, I recommend this paper be published in ACP after addressing the reviewers comments.

In general, the detailed 1D modeling for this dataset (similar to that presented in Thomas et al. (2011) and Thomas et al. (2012)) would be a good addition to our scientific understanding of these measurements, but goes beyond the scope of this present study.

Major comments:

I echo the second reviewers concerns about the simplified model framework used to estimate NOx fluxes. However, I think the approach still has some value even with the noted shortcomings. I recommend that the authors revise section 4.1.4 to clarify that this approach results in NO2 production – which represents the maximum NO2 released to the interstitial air. Then the authors assume all of this is mixed from the interstitial air and evenly distributed in the atmospheric boundary layer. This should be presented as the maximum emission flux that can result from the conditions near the WAIS Divide drilling camp.

Changes were made as suggested and more details were added following the comments of the referee #2.

There is also a major difference between the WAIS Divide and Summit, as pointed out by the authors, in that at Summit, it has been established that there is low levels of bromine oxide (BrO) in the boundary layer that counteracts the effects of ozone production from NOx released from the snowpack. Prior 1D modeling work has focused on the combined role NOx and bromine released from the snowpack at Summit have on ozone. Given that it has not been established if there is low levels of halogens present inland within the boundary layer in the Antarctic, I believe this should be mentioned as a difference between ozone production at Summit and in the present study. It may also be added at as a caveat that if it is established that any halogen chemistry is occurring in the WAIS Divide snowpack, this may act to counteract ozone production from NOx release.

We amended the manuscript as suggested (see Sections 4.1.3 and 4.1.7).

The value of 2 pptv for NO to trigger O3 production is very interesting and should be included in the final version of the paper, within the context of the previous comment.

This value is now compared to that of a remote mid-latitude troposphere and to a derived value from measurements at South Pole (Section 4.1.7). This result was also added in the conclusion.

More details of the NASA Goddard Flight Center (GSFC) model should be included. What is meant by "that included physical sources of H2O2 and CH2O"? What other species were constrained using measurements?

We did not run the GSFC model for this study, and we simply refer to Frey et al. (2005) who run and describe the model. We edited this part to avoid any confusion (Section 4.1.1).

I'm a bit confused about the discussion of HOx. The authors state: "The lifetime of NO2 was estimated with measurements of HOx at Halley by Bloss et al. (2007) ([OH]=3.9×105 molecule/cm3, [HO2]=0.76pptv)". Then the authors use these HOx values later in the paper to estimate ozone production. Why not take the estimated HO2 and OH from the box model run (I assume constrained to measurements of NO and ozone) to estimate the OH and HO2 and use these values later in the paper. There is not a good reason to assume the HOx measured at Halley can be used as an estimate of HOx at the WAIS divide.

Changes were made. We are now using the outputs of the box model runs. We estimated the OH concentration from its relationship with NO (Fig. 12c of Frey et al. (2005)). From the 1.3×10^{6} molecules cm^{-3} OH concentration that is assumed on a NO level of 19 pptv, we approximated a HO_2 concentration using the steady-state relation for OH/HO_2 in the upper troposphere (Seinfeld and Pandis, 1998):

$$\frac{[HO_2]}{[OH]} = \frac{k_{(CO+OH\rightarrow CO_2+HO_2)}\times[CO]}{k_{(HO_2+NO\rightarrow NO_2+OH)}\times[NO]}$$

The resulting HO_2 concentration, $4.9\ x10^{7}$ molecules cm^{-3} is an intermediate value between those reported from the Antarctic coast ($2.02\ x10^{7}$ molecules cm^{-3} (0.76 pptv) (Bloss et al., 2007) and the East Antarctic Plateau ($7\ x10^{7}$ molecules cm^{-3}(Frey et al., 2009; Mauldin et al., 2004), $8.3\ x10^{7}$ molecules cm^{-3} (Eisele et al., 2008)).

Minor comments:

In places I find this manuscript a bit difficult to read. It may help to review the text for clarity during the review process.

The text and the structure of the manuscript were fixed to improve its clarity.

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